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ARTICLES

WAR CUES AND FOREIGN POLICY ACTS

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Cognitive psychological theory and research suggest that specific actions are primed or potentiated by prior related cues. Thus violent cues should influence subsequent conflictual action choices. Laboratory subjects were given one of two written vignettes recalling either the slaughter of World War I or Allied weakness before World War II. A control group received no vignette. Subjects were then asked to specify foreign-policy choices from an inventory of possible cooperative-conflictual acts, relevant to a dispute between two fictional countries, in five sequential rounds. Both war vignettes produced more subsequent conflictual choices for subjects with dominant (i.e., aggressive) personality traits, and less conflictual choices for subjects with submissive personality traits. The findings suggest that priming activates underlying personality predispositions that enhance cooperative or conflictual action choices. The results also point to the potential importance of prior cues in shaping foreign-policy decision makers' and mass-public responses to international events.

National decisions to go to war or to maintain peace are gradually being understood in a much more comprehensive way. This new understanding takes into account powerful psychological factors, in addition to more standard political variables.

Traditional analysis of international relations has long held that calculation of material factors, defined in terms of reason of state or national interest, are the primary determinants of decisions to go to war or make peace. Formal models of foreign-policy making also hold that such decisions stem from the calculation of national utilities in an interdependent system where all actors are rational, or at least behave as though they are.

Peace-war decisions, however, have other important dimensions, including psychological ones. For example, cogni-

tive psychology suggests that schemata provide important filters for perception and its translation into decisions and actions. According to schema theory, general knowledge and environmental events are stored in long-term memory as frames or scripts that can be retrieved and used as analogues of current events and, as such, serve as a guide to action. They allow extrapolation from past experiences to current or anticipated reality (see Anderson 1980; Gilovich 1981; Johnson-Laird 1983; Read 1984; Roediger 1980; Waterman and Hayes-Roth 1978).

A popular aphorism holds that those who do not remember history are condemned to repeat it. Our perspective suggests that those who do remember history apply it selectively. Particular foreign-policy makers recall different past events in different shading and detail; for exam-

ple, the assassination of the Archduke Franz Ferdinand at Sarajevo, Germany's invasion of its western European neighbors, the Japanese attack on Pearl Harbor, the Korean War, the Cuban missile crisis or Vietnam. And they see contemporary events, for example the Libyan crisis, as analogues of such earlier historical prototypes (see Alker, Bennett, and Mefford 1979; May 1973; Neustadt and May 1986).

Activation is the process by which memory representations become available. Such activation implies recall of, and attention to, a particular schema. The schema is connected or applied to a current or anticipated situation. It is mentally rehearsed or practiced. And eventually it is used as a concrete model for action (see Anderson 1983; Anderson and Pichert 1978; Bargh et al. 1986; Bargh and Pietromonaco 1982; Glenberg et al. 1983; Higgins and King 1981; Higgins, King, and Mavin 1982).¹

Of course, other psychological factors are also significant in this activation process. For example personality attributes supply important qualifications to perception and behavior. People with different personalities may see, remember, or use the same schema in different ways, give different weights to various schema dimensions, and draw different action conclusions (see Cartwright 1974, chap. 21).

Laboratory Experiment

These concerns led us to develop a laboratory analogue of peace-war decision making in which to examine the influence of war-related primes on selected foreign-policy acts. The basic idea was simple. One group of subjects would be primed with a written vignette describing the horrors of trench warfare in World War I, which we believed would enhance the subjects' cautiousness about acts of violence. The second group would get a

vignette reminding them about Allied appeasement of the Nazis prior to World War II and of the resulting costs of this passivism. This group, we thought, would be primed to conflictual acts. A control group would receive no prime prior to decision making.

We decided to complicate the experiment by examining other variables that might influence cooperative or conflictual behavior. Prior knowledge or information about international relations seemed especially likely to be important (see Ruben 1986).

We also wished to examine the role of personality in determining foreign-conflict behavior. Specifically, we focused on two personality dimensions, dominance-submission and experimentation-conservatism. Dominance-submission is especially relevant to a willingness to use forceful means to settle problems. Furthermore, previous research has established dominance as an important personality characteristic helping determine major U.S. foreign-policy decisions (see Etheredge 1978). While there is less extant evidence, an experimenting-conservative factor seems likely to be related to liberal versus conservative political ideology, again suggesting alternative approaches to solving international problems.

Our object in this experiment was to determine whether certain variables would allow us to predict action choices for cooperation or conflict in a simulated international setting. We decided to use a context modeled after the Falklands-Malvinas crisis. Thus, after they had been primed by one or none of the vignettes, the subjects would be given a scenario like the one existing at the beginning of this crisis. We would then ask them what a particular national actor did next, asking them to select one from an inventory of possible cooperative or conflictual actions.² We wanted to give this part of the experiment a dynamic cast in order to study the development of these choices over time. We, therefore, iterated the

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process, alternating the identity of the national actor and asking subjects to make choices over five successive turns.³

Method

Our subjects were sixty University of Colorado introductory-psychology students, who participated as part of a class requirement. The subjects were run in a single-group session that took less than one hour.

Each subject was given a booklet containing the nine parts outlined below:

1. The "sixteen-personality-factor" (16 PF) questionnaire, "factor E" (dominant-submissive) and "factor Q1" (experimenting-conservative)
2. a political-knowledge test constructed by the authors
3. a priming vignette
4. a fictional scenario describing the activities of two countries at war
5. five pages, each specifying an alternating national identity and containing 15 possible political-action choices
6. a true-false memory test for the vignette
7. a true-false memory test for the scenario
8. a recognition-memory test for the political-action alternatives
9. a question concerning the subject's recognition of the similarity between the scenario and recent political events

Each of these parts was used in a separate phase of the experiment. Phase 1 examined selected personality attributes. The first part of the booklet was taken from the 16 PF test. It contained 13 multiple-choice questions focusing on factor E. Individuals on one end of this scale were "dominant, assertive, aggressive, stubborn, competitive, bossy." Those at the other extreme were "submissive, humble, mild, easily led, accommodating." There were also 10 questions centering on factor Q1. Responses that were "experi-

menting, liberal, critical, open to change" opposed those that were "conservative, respecting of traditional ideas." The questions from each scale were intermixed with 13 neutral filler questions. All questions had three alternative answers (see Institute for Personality and Ability Testing 1979).

Phase 2 measured political knowledge, particularly in the area of international relations. It included 16 multiple choice questions developed by the authors to test the subjects' knowledge of political events preceding, during, and following both world wars. Each question had five alternative answers.

Phase 3 primed the subjects with a particular schema, presented as a written vignette. One-third of the subjects received Vignette A (Appendix). This described the costs involved in war, using as an example the high number of British and German casualties during the trench warfare of World War I and the effects of these losses on postwar morale.

Vignette B (Appendix) was given to a second one-third of the subjects. It described the events leading up to World War II. This scenario discussed the policy of Western statesmen in dealing with Germany's invasion of Czechoslovakia. It stated that appeasement of aggression was a mistake and led eventually to a much wider and longer conflict. This scenario emphasized the point that when a country becomes aggressive, war is inevitable and hence any attempts at peace are futile.

No vignette was given to the last one-third of the subjects, the control group.

Phase 4 presented the decision scenario that provided the basis for the subjects' actions (Appendix). It was modeled on the Falklands-Malvinas crisis of 1982. The vignette described two fictional countries, Afslandia and Bagumba, and the claims each had on a set of small islands. Afslandia represented Great Britain and Bagumba took the place of Argentina.

Phase 5 gave the subjects sets of alter-

native actions that the protagonists might take in the Afslandia-Bagumba situation. Five separate pages in the questionnaire each contained 15 different actions. These ranged on a scale from 1 to 15 in cooperation/conflict intensity. A scale value of 1 represented the most peaceful attempt to resolve the tensions; 15 constituted an all-out military retaliation. The actions in each set were presented in a random order. The appendix contains the five sets of actions given to the subjects, with actions in each set ordered along the cooperation-conflict dimension.⁴

On the first page that contained 15 alternatives, subjects were instructed to choose the action they felt Afslandia would take in response to Bagumba's invasion of the islands. On the second page, the subjects were instructed to choose the action that they felt Bagumba would take, given that Afslandia had taken the action chosen on the first page. This procedure continued through all five pages, with the actions alternating from one to the other country. Hence, Afslandia was given three actions, and Bagumba two.

The next three phases tested the subjects' memory. Phase 6 included 10 true-false questions about the priming vignette read in phase 3. No test was included for the control group of subjects who had not read a vignette. Phase 7 included 10 true-false questions about the Afslandia-Bagumba scenario presented earlier in phase 4. Phase 8 presented the five pages from phase 5 but with the action alternatives on each page in a different random order. The subjects were instructed to recall their previously chosen alternative by circling it on each of the five pages.

In phase 9 subjects were asked whether the Afslandia-Bagumba situation reminded them of any recent events in history. This question allowed us to determine whether the subjects recognized the situation as being similar to the Falkland Islands crisis.

Results

In addition to the three conditions differing in the vignette read during phase 3 (World War I, World War II, control), subjects were further subdivided into personality subgroups. This was done on the basis of a median split of subject scores on the dominant-submissive questions from the 16 PF questionnaire administered in phase 1. The questions were scored in accordance with the criteria outlined in the manual such that one of each of the three alternative answers received no points, one received one point, and one received two points. When more than one subject in a condition obtained the median score, those subjects were randomly assigned to the high and low groups.

A four-way mixed *analysis of variance* (ANOVA) was conducted on the alternative action choices made in phases 5 and 8. The score used for each choice was equal to its value on the 15-point scale, with 1 being most cooperative and 15 most conflictual. There were two "between-subjects" factors included in the ANOVA, *priming condition* (World War I, World War II, control) and *dominant-submissive personality group* (high, low). There were also two "within-subjects" factors, *phase* (original, phase 5; memory, phase 8), and *selection* (first, Afslandia; second, Bagumba; third, Afslandia; fourth, Bagumba; fifth, Afslandia).

The scores on which our analysis was based are summarized in Table 1, in which we break down the mean scale values for the two dominance groups in each of the three priming conditions as a function of phase and action selection. Subjects in the low dominance group yielded lower, more cooperative score values ($M = 8.99$) than did those in the high dominance group ($M = 10.45$), $F(1, 54) = 5.83$, $MSe = 55.36$, $p = .018$.⁵

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Table 1. Mean Cooperation-Conflict Scale Scores by Priming Condition, Dominance Group, Phase, and Action Selection

Priming condition and dominance group	Action Selections					Mean
	A	B	C	D	E	
Phase 5: Original						
World War I						
Low	10.8	9.0	9.0	7.5	6.2	8.5
High	12.0	11.0	10.7	10.3	11.5	11.1
World War II						
Low	9.5	8.1	8.4	7.7	7.3	8.2
High	11.8	9.1	11.3	11.4	10.0	10.7
Control						
Low	11.3	10.3	11.1	8.9	9.3	10.2
High	10.7	8.7	10.2	9.4	9.2	9.6
Mean	11.0	9.4	10.1	9.2	8.9	9.7
Phase 8: Memory						
World War I						
Low	10.8	9.0	9.0	7.5	6.2	8.5
High	12.2	11.1	10.7	11.0	11.6	11.3
World War II						
Low	10.0	7.8	8.3	8.4	7.2	8.3
High	12.2	10.4	10.6	10.1	9.6	10.6
Control						
Low	11.3	10.1	11.9	8.7	9.0	10.2
High	10.7	8.7	9.4	9.1	8.9	9.4
Mean	11.2	9.5	10.0	9.1	8.8	9.7

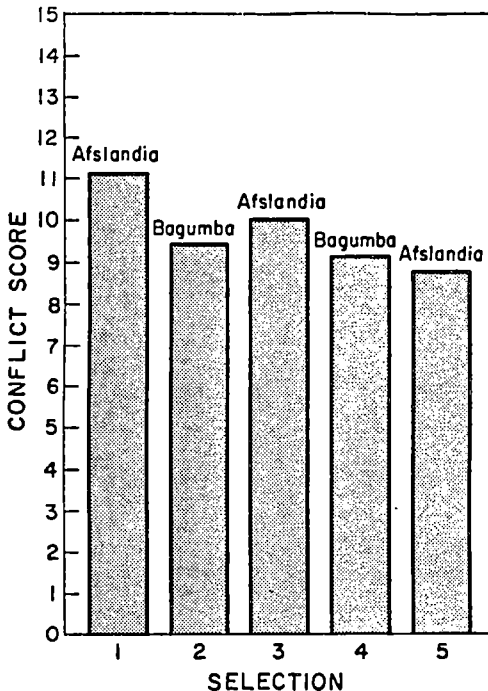
There was no main effect of the priming condition or the vignette, taken by itself, but we did observe a significant interaction of priming condition and dominance group, $F(2, 54) = 3.18$, $MSe = 55.36$, $p = .048$. Figure 1 illustrates this relationship.

Subjects in the two priming conditions that included war vignettes showed expected differences. The high-dominance group was more conflictual (World War I: low, $M = 8.50$; high, $M = 11.21$; World War II: low, $M = 8.27$; high, $M = 10.65$). Subjects in the control group, who did not read a vignette about war, did not show the expected difference between the two dominance groups (low, $M = 10.19$; high, $M = 9.50$). This finding is par-

ticularly interesting because subjects in the nonprimed condition tended to behave in a direction opposite to what we expected. More submissive unprimed subjects were more conflictual; more dominant subjects who did not receive the preliminary vignette were more cooperative. Our overall results thus suggest that subjects' propensity towards conflictual or peaceful behavior is activated by the general war scenario, regardless of whether the scenario itself specifically discusses the costs of war or the futility of attempts for peace.

There were negligible effects of phase on the scale values; subjects' selections in the memory phase ($M = 9.72$) were quite close in value to those in the original

Figure 1. Mean Cooperation–Conflict Scale Score ("Conflict Score") as a Function of Dominance Group and Priming Condition

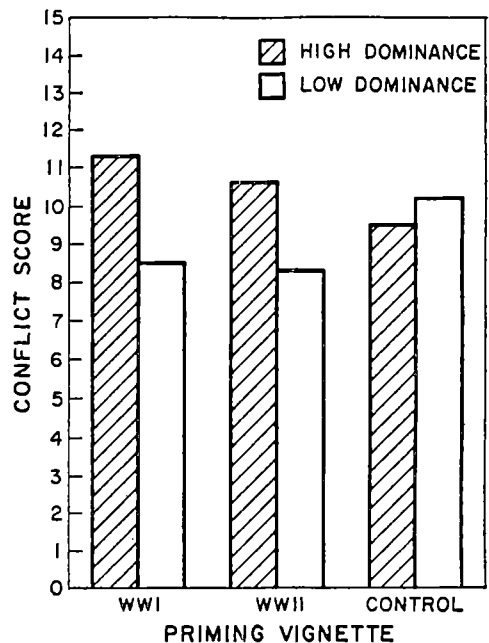


phase ($M = 9.72$), $F(1, 54) < 1$. The scale value did vary considerably, however, as a function of selection, $F(4, 216) = 9.56$, $MSe = 10.07$, $p < .001$. In Figure 2, we show that the subjects' first selection was usually most conflictual ($M = 11.11$) and last selection was generally the most cooperative ($M = 8.83$). Moreover, the second ($M = 9.44$) and fourth ($M = 9.17$) selections, those involving Bagumba, were also somewhat less conflictual than the third selection ($M = 10.05$), which involved Afslandia. Thus, subjects became less conflictual with successive selections concerning a particular country, and they were generally more conflictual in their selections for Afslandia than for Bagumba.

Two other variables on which subjects were divided were experimenting–conservatism personality attributes and prior knowledge. Neither had significant effects on subjects' choices. We used subjects' scores on the 10 questions from factor Q1 of the 16 PF personality test to separate them into two experimenting–conservatism groups, again using a median-split rule. We did not find a significant overall difference in action choices between more experimenting subjects who scored high ($M = 10.12$) and more conservative ones who scored low ($M = 9.32$), $F(1, 54) = 1.52$, $MSe = 63.16$, $p = .221$. Further, this personality dimension did not enter into any significant interactions with the factors of condition, phase, and selection.

We also formed two groups of subjects for each condition on the basis of their scores on the knowledge test presented in phase 2. To score this test, we gave one

Figure 2. Mean Cooperation–Conflict Scale Score ("Conflict Score") as a Function of Successive Selection



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point for each correct answer to the 16 five-alternative, multiple-choice questions; subjects' knowledge scores ranged from 2 to 13. Like the division into experimenting-conservative personality groups, the division into low and high knowledge groups did not yield any significant main or interactive effects; the scale values for subjects of low knowledge ($M = 9.75$) were equivalent to those for subjects of high knowledge ($M = 9.69$), $F(1, 54) < 1$.

Although only the division of subjects into low and high dominance groups yielded significant effects in the analyses of cooperation-conflict scale values, there was a significant positive correlation between scores on the dominance and experimenting questions of the 16 PF personality test, $r = .45$, $p < .001$, and between scores on the knowledge test and the experimenting questions of the personality test, $r = .28$, $p = .017$. There was, however, no significant relation between scores on the knowledge test and the dominance questions of the personality test, $r = -.08$, $p = .261$.

Scores on the 10 true-false questions given in phase 7 concerning memory for the Afslandia-Bagumba scenario were quite high ($M = 8.02$), ranging from 4 to 10 correct responses, thereby indicating that subjects did read and remember the scenario when making their action choices. As we should also expect, subjects in the World War I and World War II conditions produced high scores in phase 6 on the 10 true-false questions concerning those vignettes. The scores ranged from 4 to 9 for the World War I vignette ($M = 6.75$) and from 2 to 10 for the World War II vignette ($M = 7.20$).

We asked subjects in phase 9 whether the Afslandia-Bagumba situation reminded them of any recent events in history. Twenty-seven of the 60 mentioned the Falklands-Malvinas crisis: 9 from the World War I group, 8 from the World War II group, and 10 from the control group. An analysis of variance showed a

significant interaction on mean cooperation-conflict of dominance-submissiveness and recognition of the Falklands-Malvinas crisis, $F(1, 48) = 6.30$, $MSe = 49.15$, $p = .016$. There was little difference between the low dominance group ($M = 10.41$) and the high dominance group ($M = 10.13$) for those subjects who recognized the crisis. On the other hand, subjects who did not recognize the crisis responded in very different ways. Subjects from the high dominance group were substantially more conflictual ($M = 10.78$) than those from the low dominance group ($M = 8.14$). Subjects who identified the crisis seemed to react at least partly from their memory of actual historical events, rather than their personal inclinations. Personal predispositions, however, were a major influence for the subjects who did not recognize the crisis. No other effects involving recognition of the crisis were statistically reliable. In particular, the three-way interaction of crisis recognition, dominance group, and priming condition was not significant, $F(2, 48) < 1$.

Discussion

Our experiment supported the importance of priming with historical analogues for action choices in a simulated international environment, but not quite in the way that previous theory and research had led us to expect. We initially assumed that subjects reminded of the horrors of World War I trench warfare would generally be more inclined to cooperative-action choices and more cautious about initiating conflict. On the other hand, those reminded of the costs of appeasing Nazi Germany should have been less cooperative overall and more likely to act conflictually. The results suggest that priming has less specific effects. The particular context or meaning of the vignettes had no apparent influence by itself. In-

stead, there was a more complex, interactive result; war-related stimuli of either kind predisposed subjects to act in accordance with their dominant-submissive personality traits.⁶

We had expected that personality would also play an overall dispositional role, that is, that dominant subjects would be more conflictual, and submissive subjects more cooperative. This was true, but again in a more complicated way than we had anticipated. Personality was important only when activated by appropriate priming. War stimuli triggered personality dispositions and provided them with force and direction. Dominant personalities became conflictual, and submissive personalities cooperative, mainly when stimulated in a war-related context. Without such activation, personality dispositions seemed latent at best and without any significant influence on action choices. Further, personality differences only emerged among subjects who did not recognize a historical referent. Dominant-submissive tendencies were inhibited or not activated if subjects identified the decision scenario as the Falklands-Malvinas crisis.

A priori, and on intuitive grounds, it seemed reasonable that the experimenting-conservatism trait would have effects like those of dominance-submission. This aspect of personality seemed roughly isomorphic with liberal-conservative political ideology. But priming activated personality traits selectively; contrary to what we anticipated, conservatism was not associated with conflictual behavior, even when stimulated by war cues.

Another variable that we had thought might be important was not. The general level of prior political knowledge had no observable influence. This result, though negative, is consistent with our other findings. Knowledge, like personality, is a reservoir, potentially linked with action. By itself, however, the level of knowledge does not have a specific valence toward cooperation or conflict.

Two other sets of findings merit discussion: the more conflictual choices for Afslandia than for Bagumba and the declining conflict behavior through the set of five selections. Recall that the decision scenario of phase 4 ends with Bagumba undertaking a set of overtly conflictual acts. For this reason, it is not surprising that it evoked conflictual responses for subjects considering the appropriate action for Afslandia. Beyond this point, however, there were no more external conflict triggers. Without such stimuli, the impetus for conflict thus gradually wound down to lower levels, though the mean choices did not ever cross over to cooperation.

The results document an activation process for dominant or submissive personality predispositions, which, in turn, incline the subjects to conflictual or cooperative actions. This process may exist in many different contexts, from interpersonal to international.

Psychological priming may be important in shaping public responses to foreign-policy events through modern mass communications. Historians have long been aware of mass priming effects, for example the influence of "yellow journalism" on Western imperialistic adventures during the nineteenth century. Thus, the newspapers owned by William Randolph Hearst, Jr. had a particularly striking effect in promoting the Spanish-American War (see Dierks 1970, 4).

Priming may also be important in molding foreign-policy decision makers' responses to international events. It may seem that undergraduate psychology subjects, especially dealing with a fantasy world symbolized by the unlikely names of Afslandia and Bagumba, are a far distance from the corridors of real international experience and power. Nevertheless, subjects and decision makers share a common human identity, and we believe that the same psychological dynamics exist, to some extent, in both laboratory and real-world settings.

Our experiment suggests that peace/war actions are not necessarily taken only on the basis of rational calculation of benefits and costs (see Kahneman and Tversky 1979). We do not deny that such calculations are involved and may have an important influence. At the same time, they and the subsequent decisions may also be significantly influenced by immediately preceding cues. In our experiment, the priming vignettes provided such cues. In the real world of foreign-policy making and mass-opinion formation, this priming occurs as formal and informal, obvious and subtle, signals flow through the stream of political culture. These cues include prior conversations or conferences among decision makers; bureaucratic memoranda, for example, position or planning papers; public speeches; newspaper or magazine reports; popular television and movie entertainment.

Alternative cues may help explain how multiple actors with apparently similar interests may react differently to what appear to be the same foreign-policy events. The Libyan crisis of early 1986 provides a recent example. U.S. and European citizens were faced with the same terrorist stimuli; yet their responses were markedly different. The United States took a relatively conflictual line, mounting two separate military strikes. Most European political leaders and publics were much more cautious, failing to support and even opposing United States actions. Many underlying factors resulted in divergent reactions by the United States and Europe. Our research suggests that variable priming, in conjunction with disparate personality dispositions, may be one of these underlying causes.

Appendix

Vignette A: War Involves Terrible Costs

World War I was not a war of mobility and maneuver; it was a war of attrition—an organized, four-year attempt by both sides to gain victory simply by bleeding each other to death. It was an unsophisticated strategy and the losses were catastrophic. A British attempt to pierce German lines in 1916 resulted in the five-month Battle of the Somme. Although they pounded the German lines for eight days with artillery before the troops were even sent into battle, the British gained only 120 square miles—at the cost of 420,000 men, that is, 3,500 men per square mile. German losses were even greater: 445,000 men. Some estimates place the total Somme casualties at 1.2 million men, the highest for any battle in history. At Ypres in 1917 the British bombardment lasted nineteen days; 321 trainloads of shells were fired, the equivalent of a year's production by 55,000 war workers. That time the British forces captured forty-five square miles—at the cost of 370,000 men, or 8,222 men per square mile. By comparison, total British Empire casualties during the six years of World War II were almost 1.25 million, including 350,000 dead and 91,000 missing. Approximately 9 million men in uniform were killed during the four years of the Great War, and the number of dead civilians totaled several million more.

But the impact of war cannot be measured by citing statistics of the dead. The real impact can be understood only psychologically. Losses are not just quantitative; they are qualitative as well. A nation can ill afford to lose millions of its men. It can even less afford to lose almost its entire youth. Is it any wonder that the nations of Europe, having lost so many of their men—and especially their young

men and the children they would have fathered—also lost their élan, their self-confidence, their hope for the future? For the men who would have supplied this vigor and optimism—had they grown up and become the leaders of government, business, labor, and science—lay dead in Flanders fields. And those who returned from the battlefields, where they had left the corpses of their comrades in arms, were haunted by the war. In the interwar period they remained politically passive, withdrawing into their private worlds, avoiding public involvement. Erich Maria Remarque dedicated his famous novel *All Quiet on the Western Front* to this "generation of men who, even though they may have escaped its shells, were destroyed by the war." (Spanier 1975, 19)

Vignette B:

Appeasement of Aggression Leads to War

The most famous example of appeasement was the Munich conference, at which the Sudetenland was transferred from Czechoslovakia to Germany. Hitler had used a branch of the Nazi Party to stir up trouble in the Sudetenland, telling them always to ask for so much that they could never be satisfied. He then used these incidents as a pretext for demanding that the Sudeten Germans be put under German rule. The Czechs refused to be bullied and mobilized their reserves, but the rest of the world was afraid of war and insisted on an international conference instead. The conference, so eagerly sought as an alternative to war by Western leaders (including the American president), was far from a model of impartiality. It was held in the German city of Munich, site of the Nazi Party's headquarters. It was attended by Britain, France, Italy, and Germany but not by the country most directly threatened, Czechoslovakia. As a result of the conference, the Czechs were ordered to hand over the Sudetenland, including border

fortifications and economic resources as well as about 700,000 Czechs who would now go under German rule. Yet Neville Chamberlain, the British prime minister, came back from Munich with his black umbrella in one hand and a piece of paper in the other, saying, "I believe it is peace for our time." The paper was important because Chamberlain had on it Hitler's own promise that this was his last territorial demand in Europe. And it was reasonable to believe him. The German claim to rule Germans in the Sudetenland and Austria was justified by the principle of national self-determination. The issue of Germans in South Tyrol and Germans in the areas along the Polish border remained, but Hitler had concluded treaties with both Italy and Poland.

The Western statesmen quickly discovered that their appeasement policy was mistaken when, only six months later, Hitler took over the rest of Czechoslovakia in clear violation of his pledges at Munich. War had become inevitable. In retrospect most Western policy-makers agreed that they should have gone to war to aid Czechoslovakia. But it was too late for that. The next country to be threatened by Hitler was Poland. Its cause was less just, its regime less compatible with Western ideas of democracy or even human decency, and its terrain a lot harder to defend. It was, in fact, as Hitler calculated, irrational for the French and British to get into a major war with Germany over Poland. Nevertheless, the British and French leaders made that irrational choice and World War II began. (Ziegler 1977, 36-37)

Decision Scenario

Two nations have been arguing about the possession of a set of small islands for several hundred years.

Afslandia is located about 5,000 miles away from the islands. One of its explorers discovered and took possession of

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the islands at the end of the 16th century. Colonists from Afslandia settled on the island, which adopted the institutions and way of living of the mother country. The islands were the basis for a lucrative fishing industry. In addition they served as a resupply port for ships and also as a base for the exploration of neighboring natural resource deposits. Afslandia has undertaken negotiations with the government of Bagumba, seeking ways to settle the dispute while safeguarding its own rights, as well as those of the residents of the islands. It has also been willing to submit the dispute to the International Court of Justice. Finally, when necessary, Afslandia has defended its possession of the islands through military means.

Bagumba is located much closer to the islands, about 25 miles away. Bagumba was, in an earlier time, a colony itself of a distant power. It achieved independence at the beginning of the 19th century. Shortly thereafter, it made its first attempt to proclaim its sovereignty over the islands, sending a number of small military expeditions, which were repulsed. Bagumba has consistently asserted its "indisputable right" to the territory. It has sought to achieve its claims through political negotiation. These negotiations, however, have been without success. Bagumba has consistently rejected Afslandia's attempt to submit the dispute over sovereignty to the International Court of Justice.

Last week, fishermen and scrap merchants from Bagumba landed on some remote parts of the islands and set up camps. Yesterday, troops from Bagumba landed on the islands, defeated the small military garrison of Afslandia, and declared that their armed forces had recovered the islands for the nation, and that sovereignty over the islands was now assured. It declared that it had finally achieved the legitimate rights of the people of Bagumba which had been patiently deferred for over 150 years. Finally it

declared that the long series of fruitless negotiations to obtain what Bagumba had always considered to be its inheritance had finally ended.

Alternative Actions: A

1. Afslandia accepts the Bagumban head of state as its own head of state.
2. Afslandia and Bagumba form a joint military high command.
3. Afslandia gives Bagumba a new cruise missile system.
4. Afslandia grants disaster relief to Bagumba.
5. Afslandia announces its full support of Bagumba's new established government.
6. Afslandia announces that it favors Bagumba's new economic policies.
7. Afslandia sends revised draft of policies and programs to Bagumba.
8. Afslandia's broadcasting makes no special provision for reporting on the Bagumban situation/peace talks with Bagumba.
9. Afslandia's government requests a change in Bagumba's policy.
10. Afslandia's National Broadcasting System makes verbal accusations accusing Bagumba of unethical conduct.
11. Afslandia imposes extremely high tariffs on Bagumba products.
12. Afslandia breaks diplomatic relations with Bagumba.
13. Afslandia's border police kill 4 Bagumban soldiers who trespass inside border lines.
14. Afslandia's troops capture two of Bagumba's border provinces.
15. Afslandia firebombs 5 of Bagumba's major cities.

Alternative Actions: B

1. Bagumba joins, and becomes a province of, Afslandia.
2. Bagumba's air force joins Afslandia's

- air force in extended joint maneuvers.
3. Bagumba army officers spend six weeks in Afslandia training with their officers.
4. Bagumba lifts economic restrictions on trade with Afslandia.
5. Bagumba's farmers invite Afslandia's farmers to join in committees concerning new strains of plants.
6. Bagumba apologizes to Afslandia for untruthful accusations.
7. Bagumba sends a special Ambassador to Afslandia.
8. Bagumba's government allows Bagumba civilians and tourists to enter Afslandia, but only for certain periods of time.
9. Bagumba accuses Afslandia of unethical business practices.
10. Bagumba condemns Afslandia's foreign policy.
11. Bagumba's government allows no travel in or out of Afslandia.
12. Bagumba provides sanctuary to anti-Afslandia terrorists.
13. Bagumba's terrorists kidnap Afslandia's Ambassador.
14. Bagumba sinks 7 of Afslandia's commercial vessels.
15. Bagumba drops a nuclear bomb on Afslandia's largest industrial city which kills 1.5 million people.
7. Afslandia sends its chief negotiator to facilitate "peace talks" with Bagumba.
8. Afslandia's President has no comment about Bagumba's foreign policy.
9. Afslandia denies accusations of mistreating Bagumba's prisoners.
10. Afslandia recalls its Ambassador to Bagumba.
11. Afslandia cuts off all financial aid to Bagumba.
12. Afslandia finances paid agitators in Bagumba's capital.
13. Afslandia mines three of Bagumba's bridges and important industrial plants.
14. Afslandia bombs Bagumba's agricultural areas.
15. 100,000 Afslandia soldiers invade Bagumba.

Alternative Actions: C

1. Afslandia and Bagumba form a political confederation.
2. Afslandia joins Bagumba in forming a new international organization.
3. Afslandia invites Bagumba to conclude a military agreement.
4. Afslandia provides large loans to Bagumba.
5. Afslandia's scientists confer with Bagumba's scientists over a new discovery in medicine.
6. Afslandia raises the status of Bagumba's diplomatic legation to that of an embassy.

Alternative Actions: D

1. Bagumba and Afslandia form a new federal system.
2. Bagumba joins Afslandia in establishing a comprehensive common market.
3. Bagumba provides Afslandia with land to station troops in Bagumba.
4. Bagumba sells grain to Afslandia.
5. Bagumba proposes a friendship treaty with Afslandia.
6. Bagumba accepts Afslandia's terms for negotiation.
7. Bagumba accepts an invitation by Afslandia to have talks.
8. Bagumba carefully words its telegram to Afslandia, not committing itself at all.
9. Bagumba insists that Afslandia's foreign policy is unfair.
10. Bagumba refuses to participate in an upcoming conference because of Afslandia's presence.
11. Bagumba suspends economic treaties and agreements with Afslandia.
12. Pro-Bagumba marchers demonstrate

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violently outside Afslandia's embassy.

13. Bagumba shells a small island controlled by Afslandia.
14. Bagumba's ships drop mines in Afslandia's territorial waters.
15. Bagumba drops nerve gas bombs on 30 of Afslandia's major cities.

Alternative Actions: E

1. Afslandia joins, and becomes a province of, Bagumba.
2. Afslandia and Bagumba form a joint military alliance.
3. Afslandia sells Bagumba top secret information concerning nuclear power plants.
4. Afslandia offers financial aid to Bagumba for development.
5. Afslandia opens its borders to Bagumban citizens with little or no restrictions.
6. Afslandia offers aid to Bagumba to help put down riots by Bagumba's leftists.
7. Afslandia invites Bagumba to undertake negotiations.
8. Afslandia refuses any special acknowledgment of Bagumba's policies at the economic meetings and conferences.
9. Afslandia refuses Bagumba's protest note.
10. Afslandia's Foreign Minister postpones a state visit to Bagumba, denouncing its recent hostile actions.
11. Afslandia refuses all scientific exchange with Bagumba.
12. Afslandia's secret intelligence agency financially supports guerrilla attacks on Bagumba.
13. Afslandia places water barriers to interfere with shipping in a Bagumba port.
14. Afslandia shells border cities in Bagumba.
15. Afslandia poisons water supplies in 5 of Bagumba's major cities.

Notes

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1. Bordin (1986), Bosman (1986), Gilovich (1981), and Thomas and Sigelman (1984) describe experiments that relate prior activation specifically to political choice.

2. Although the wording of the question requested the subject to choose the next action undertaken by the national actor, we infer the subject's own action propensities from the responses.

3. This iterated approach to cooperation and conflict connects our work with earlier research on the prisoner's dilemma. See Axelrod 1984.

4. The scale was taken from the COPDAB project (Azar 1980). The project used the scale as a basis for coding an inventory of international events and interactions covering 135 countries since 1948, derived from over 70 journalistic sources.

In our experiment, an undergraduate student assistant, Lisa Garlanger, invented the names for the fictional countries. She used the 15 major COPDAB categories of cooperation and conflict as a framework and guide to generating a fictional set of events-interactions. Each of these items was then evaluated. The 45 items judged most faithful to the original COPDAB categories were retained for administration to subjects.

We used the 15 original COPDAB categories as a scale in its own right. Azar and Havener (1976) discuss some of the scaling issues and report the results of psychometric efforts to achieve more precise interval measurement. Their study generally supports the validity of the original 15-point scale.

A separate experiment was conducted to ensure that the 15 political actions chosen for each of the five sets were ordered accurately in terms of cooperation and conflict. Fifteen introductory psychology students from the University of Colorado were asked to rank the 15 actions from each of the five sets on a scale from 1 to 15. They were then asked to rate each of the 15 actions from each of the five sets on a scale from 1 to 100.

In the ranking task, 13 subjects (2 subjects failed to follow the instructions) were presented with the

five sets of 15 actions one set at a time. For each set, they were instructed to read each of the 15 actions and then rank the actions from 1 to 15 with a 1 being given to the action they considered to be the most peaceful and a 15 being given to the action they considered to be the most aggressive. They were instructed that each action was to be given one number and that each number 1 through 15 was to be used only once.

In the rating task, 15 subjects were presented with the five sets of 15 actions one set at a time. For each set they were instructed to consider each action and then rate it on a scale from 1 to 100 with a 1 indicating the action was extremely peaceful and a 100 indicating the action was extremely aggressive.

The mean ranking and mean rating were then calculated for each action within each set across all subjects. For each set of 15 actions, Pearson product-moment correlations between these means and the scale values were calculated. The resulting correlations between the mean rankings and the scale values are as follows: set 1, $r(11) = .85, p < .001$; set 2, $r(11) = .92, p < .001$; set 3, $r(11) = .95, p < .001$; set 4, $r(11) = .93, p < .001$; and set 5, $r(11) = .92, p < .001$. The resulting correlations between the mean ratings and the scale values are as follows: set 1, $r(13) = .91, p < .001$; set 2, $r(13) = .91, p < .001$; set 3, $r(13) = .95, p < .001$; set 4, $r(13) = .92, p < .001$; and set 5, $r(13) = .88, p < .001$. These high correlations indicate that the ordering of the 15 actions within each set was appropriate for the subject population employed in the main experiment.

The same experiment was conducted again using eight political-science graduate students considered to be more knowledgeable about international affairs than the undergraduate subjects. The resulting correlations between the mean rankings and the scale values are as follows: set 1, $r(6) = .91, p < .005$; set 2, $r(6) = .95, p < .005$; set 3, $r(6) = .97, p < .001$; set 4, $r(6) = .97, p < .001$; and set 5, $r(6) = .96, p < .001$. The resulting correlations between the mean ratings and the scale values are as follows: set 1, $r(6) = .91, p < .005$; set 2, $r(6) = .93, p < .005$; set 3, $r(6) = .96, p < .001$; set 4, $r(6) = .97, p < .001$; and set 5, $r(6) = .96, p < .001$. All of these correlations are equal to or higher than those found with the undergraduate subjects.

5. The mean value presented here and in the subsequent analyses is the grand mean, including both phases 5 and 8. As we note below, phase did not significantly affect the scale values. Inconsistencies with Table 1 result from rounding errors.

6. It is conceivable that the vignettes or scenario stimulated prior knowledge, understanding, or memories of peace-war dynamics that overrode personality. This seems to have occurred for subjects specifically reminded of the Falklands-Malvinas crisis. For other subjects, there is no evidence that supplemental cognitive effects existed, unless they were strongly linked with personality to begin with.

The lack of effects connected with our political-knowledge test reinforces this inference.

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CRISIS BARGAINING, ESCALATION, AND MAD

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Although incomplete information is recognized to be an essential feature of crises, game-theoretic formulations have not generally modeled this explicitly. This paper models a mutually assured destruction (MAD) crisis as a game of sequential bargaining with incomplete information, sufficiently simple that its equilibria may be found. These provide better game-theoretic foundations for the notions of resolve and critical risk and their role in crises and also make it possible to compare the bargaining dynamics of this model with those of descriptively richer, but incompletely specified models, revealing several inconsistencies: several analyses of MAD conclude that the state with the greatest resolve in this contest of resolve will prevail. Many models based on critical risks suggest that a state is less likely to escalate, the greater its adversary's resolve. In our model, however, the state with the weakest resolve sometimes prevails, and some states prove more likely to escalate if their adversaries' resolve is greater.

Those who have used game-theoretic formulations of varying degrees of rigor to study international crises and nuclear deterrence theory have generally viewed crises and escalation as a bargaining process in which uncertainty and the lack of complete information play an essential role (Brams 1985, 34; Brams and Kilgour 1985, 128; Ellsberg 1959; Jervis 1972, 1978; Schelling 1960, 187-203; Schelling 1966, 92-125; Snyder 1971, 1972; Snyder and Diesing 1977; Wagner 1982, 1983). But despite its recognized importance, the formal games underlying these analyses do not model incomplete information explicitly. To the extent that they are well defined, these games have been ones of complete, not incomplete information.

This essay extends previous studies of international crises and nuclear-deterrence theory by explicitly formaliz-

ing the lack of complete information. A mutually assured destruction (MAD) crisis is modeled as a game of sequential bargaining with incomplete information. The game is sufficiently simple that its equilibria may be found, and this makes it possible to begin to analyze the bargaining and escalatory dynamics of this crisis within a well-defined game. This analysis furthers two ends. The first is to provide more secure game-theoretic foundations for the concepts of *resolve* and *critical risk* and the part they play in crises. These concepts are defined precisely in the game presented below, and the game's equilibria clarify their role in the crisis. The second end is to compare the escalatory dynamics described by the equilibria with the dynamics inferred from other less formal or more historically oriented studies of bargaining and escalation. The comparison reveals

several points of agreement and some troubling inconsistencies. The model, for example, agrees with Jervis (1972, 1976, 1978), Snyder (1971, 1972), Snyder and Diesing (1977), and Wagner (1982, 1983) in that the uncertainty created by incomplete information is an important cause of crisis and war. If there were complete information in the model, there would be no crisis and no danger of escalation. As for the inconsistencies, models based on critical risks suggest that the greater an adversary's resolve, the less likely a state is to escalate (Ellsberg 1959; Jervis 1972, 1978; Snyder 1971, 1972; Snyder and Diesing 1977). And many analyses of MAD see crises as a competition in taking risks (Jervis 1979a, 1979b, 1984; Schelling 1960, 1962, 1966; Snyder and Diesing 1977; Waltz 1983). In these crises, "the state that is willing to run the greatest risks [i.e., the state with the greatest resolve] will prevail" (Jervis 1979b, 631). In the model developed below, some states are more likely to escalate when facing an adversary with greater resolve. Moreover, the state with the least resolve may sometimes prevail. Bluffing sometimes succeeds.

The analysis begins by identifying some of the essential elements of a MAD crisis, which will subsequently be modeled. There follows a brief review of some existing models of MAD and deterrence. This review describes some of the central issues that these models have tried to explain and some of the theoretical difficulties they have encountered. The essential elements of a MAD crisis are then incorporated in a game with complete information. Although the emphasis of this essay is on bargaining with incomplete information, analyzing the complete-information version of the game will help to clarify and overcome some theoretical difficulties. Despite solving many theoretical problems, there are, ironically, no crises in the game with complete information. Crises require incomplete information. In-

complete information is then included in the game and the equilibria are found and discussed.

Some Essential Elements of MAD

In the arcane lexicon of nuclear strategy, *MAD* has many meanings. It denotes a technological state of affairs in which at least two states have secure second-strike forces. It also denotes any one of a number of distinct strategies with which a state may try to cope with this state of affairs. This essay focuses on a crisis in which the states have second-strike forces and in which each state seeks to further its ends through a strategy based on what Schelling (1960, 1962, 1966) has called "threats that leave something to chance."

The strategy-that-leaves-something-to-chance is a direct descendant of the doctrine of massive retaliation. The sanction in both is that the crisis will end in an unlimited nuclear exchange between the superpowers. As Jervis puts it, the proponents of MAD "believe that any nuclear war will be all-out" (1979b, 617).

What distinguishes the strategy-that-leaves-something-to-chance from the doctrine of massive retaliation is the particular way in which this strategy seeks to overcome the doctrine's incredibility. Once the United States was perceived to be vulnerable to a Soviet nuclear retaliation, the doctrine of massive retaliation became incredible even if used only to try to protect vital U.S. interests like Western Europe. If the Soviets could reply with a devastating nuclear counterattack, the cost to the United States of carrying out its threat seemed to be greater than the cost of letting the Soviets have their way. When MAD is the technological state of affairs, the doctrine of massive retaliation is inherently incredible.

Schelling (1960, 1962, 1966) tried to solve the credibility problem through "threats that leave something to chance."

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He agreed that once MAD had become the technological state of affairs, the cost of imposing the sanction of an unlimited nuclear attack was greater than the cost of not imposing it. Accordingly, a state could not credibly threaten to impose the sanction deliberately. But, a state might be able to take steps that created a risk that the crisis might somehow "go out of control" and escalate to an unlimited nuclear exchange. Because no step was certain to lead to this outcome and only created a risk of it, the expected cost of taking such a step would be less than the expected cost of deliberately imposing the sanction. More importantly, if a state's stakes in the crisis were large and the risk small enough, then the expected cost of taking the step might be less than the expected cost of giving in to an adversary's demands. Thus a state might credibly threaten to take such a step.

Moreover, generating varying risks of disaster might provide a state with a means of exerting coercive pressure on its adversary. By raising the risk of an unlimited nuclear exchange, a state also raises the expected cost to its adversary of continuing the crisis. Indeed, a state might be able to force its adversary into submission by making it believe that the expected cost of continuing was greater than the cost of submitting.

Schelling (1960, 118; Schelling 1966, 92-125) and others (e.g., Snyder and Diesing 1977, 209-43) realized that the credibility of threats-that-leave-something-to-chance depends on the existence of a certain type of risk. Because engaging in an unlimited nuclear exchange is assumed to be the worst possible outcome, no rational actor would knowingly trigger this exchange. But deterrence in the strategy-that-leaves-something-to-chance rests on the possibility of a crisis ending in this way. Accordingly, there must be what Snyder and Diesing call an "autonomous" risk of this outcome (1977, 210) That is, there must be some chance

that the crisis will end in an unlimited nuclear exchange despite every actor's unwillingness to initiate it. It must be possible for this exchange to result directly from something beyond the actors' control, such as a technical failure or an unauthorized act. The risk of this exchange "must come from somewhere outside the threatener's control. . . . [I]t is an ingredient in the situation that neither we nor the party we threaten can entirely control" (Schelling 1960, 118). If there were no risk of the crisis "going out of control" in this way, there would be no danger of the crisis ending in an unlimited nuclear exchange because there would be nothing for the threats-that-leave-something-to-chance to leave to chance (Powell 1985, 83-86).

The last essential element to be examined is the meaning of credibility. The problem of credibility arises because the strategy-that-leaves-something-to-chance relies on the sanction of an unlimited nuclear exchange. The credibility of threats to impose this sanction in turn requires that states be able to manipulate an autonomous risk of this outcome. But precisely what makes a threat credible? The criterion suggested by the evolution of nuclear doctrine and the one assumed in this essay is that a threat is credible if the decision to carry it out is rational. And a decision to implement a threat is rational if, at the same time the decision is made, the expected cost of implementing the threat is less than the expected cost of not doing so.

This criterion accounts for the incredibility of the threat in the doctrine of massive retaliation. It might be perfectly rational to say that one would retaliate massively in response to a limited provocation. But when, after a limited provocation, the decision of whether or not to retaliate massively actually has to be made, the expected cost of acquiescing would be less than the expected cost of carrying out the threat. Thus, the decision

Figure 1. MAD as
Two-by-Two Chicken

		II	
		Stand Firm	Submit
I	Stand Firm	d_I, d_{II}	w_I, s_{II}
	Submit	s_I, w_{II}	c_I, c_{II}

to carry out the threat would be irrational and the earlier threat to do so incredible. This criterion of credibility also explains the motivations for Schelling's threats-that-leave-something-to-chance and how they solve the credibility problem. The expected cost of taking a step-that-leaves-something-to-chance varies with the level of the autonomous risk that taking the step creates. If, therefore, the risk is sufficiently small, the expected cost of taking the step may be less than the expected cost of submitting.¹

The image of crisis reflected in the strategy-that-leaves-something-to-chance is that of a competition in taking risks (Jervis 1979a, 1979b, 1984; Schelling 1960, 1962, 1966). Because both states can destroy each other even after absorbing a first strike, relative military strength does not matter. Crises are no longer a contest of military strength but of resolve, where resolve is defined as a willingness to run the risk of an unlimited nuclear war. During the crisis each state attempts to coerce its adversary into submission by manipulating this risk. By taking steps-that-leave-something-to-chance such as imposing a blockade, committing troops, or engaging in limited demonstrations, a state hopes

to raise the risk to a level that its adversary finds intolerable or to convince its adversary that the risk will be raised to this level. In this way, "the state that is willing to run the greatest risks will prevail" (Jervis 1979b, 631).

A Review of Some Existing Models of MAD and Deterrence

The game of chicken has been used more or less formally in many analyses of MAD and deterrence (e.g., Brams 1985; Brams and Kilgour 1985; Jervis 1972, 1976, 1979a; Schelling 1966; Snyder 1971; Snyder and Diesing 1977). The simplest form of chicken, which is a two-by-two game, has often been used as a point of departure. This game is shown in Figure 1. There are two actors, *I* and *II*, which are usually taken to be states. Each must choose between standing firm and submitting. If both stand firm, the crisis ends in disaster. If one stands firm and the other submits, the former prevails. If both submit, the crisis ends in compromise. Letting w , s , c , and d denote the payoffs to prevailing, losing, mutual compromise, and disaster respectively, the payoffs satisfy the relations $w_I > c_I > s_I > d_I$ and $w_{II} > c_{II} > s_{II} > d_{II}$.

These relations are intended to capture the dilemma created by second-strike forces.² Prevailing is still better than losing. But unlike the era before the development of second-strike capabilities, when standing firm might still be preferred to submission even if one's adversary also stands firm, the dilemma created by second-strike forces is that submission is now preferred to standing firm if one's adversary stands firm.

The game in Figure 1 has two equilibria in pure strategies. In the first, *I* stands firm and *II* submits. In the second, *II* stands firm and *I* submits. These equilibria are unaffected by changes in

the payoffs so long as the payoffs continue to satisfy the preceding relations. For example, as long as $w > c > s > d$, I 's standing firm and II 's submission remains an equilibrium even if the payoff to the "cooperative" outcome is only slightly worse than the payoff to prevailing.

The insensitivity of these equilibria to changes in the payoffs is troubling. Intuitively, it would seem that the level of a state's resolve² in a crisis should be affected by these payoffs and that changes in a state's resolve would be reflected in the outcome of the crisis. Yet, if these equilibria are taken to represent the potential outcomes of the crisis, then resolve has no role. The balance of resolve plays no part in affecting the outcome of a crisis. Worse, there is no way to decide which equilibrium ought to be taken to represent the outcome of the crisis.

The introduction of critical risks may be seen as an attempt to bring resolve into the game. A state's critical risk is generally described as the greatest risk of an adversary's standing firm that a state would be willing to accept. This notion has been formalized in several ways. Jervis (1972), for example, defines it, in terms of the game in Figure 1, as the probability of an adversary's standing firm that leaves a state indifferent to standing firm and submitting. Letting r_I be I 's critical risk, it makes the expected return to standing firm, $r_I d_I + (1 - r_I)w_I$, equal to the expected cost of submission, $r_I s_I + (1 - r_I)c_I$, and is given by $r_I = (w_I - c_I) / [(w_I - c_I) + (s_I - d_I)]$. The states' critical risks clearly depend on their payoffs. Thus, if the game's outcomes can be related to these critical risks, then the outcome will be sensitive to the states' payoffs. Jervis argues that if a state believes that the probability of an adversary's standing firm is less than the state's critical risk, then the state will stand firm. Accordingly, the greater a state's critical

risk, which is taken as a measure of its resolve, the more likely a state is to stand firm (Jervis 1972).

Jervis's formulation does bring the states' payoffs and their levels of resolve back into the model. Indeed, if his decision rule is applied to the game in Figure 1, in which there is complete information and, therefore, each state knows both states' critical risks, then, following Jervis's rule, the state with the greatest critical risk will stand firm and its adversary will submit. The balance of resolve will have determined which state prevails. Jervis, however, relaxes the assumption of complete information. In fact, a fundamental assumption in his discussion of the bargaining process is that each state is uncertain of the other's resolve. Unfortunately, this type of uncertainty is not present in the game in Figure 1, and its absence seriously weakens the game-theoretic foundations underlying his discussion of crisis bargaining.

Snyder (1971, 1972) and Snyder and Diesing (1977) formalize the notion of critical risk differently. They assume that if a state submits, it can be certain of obtaining the payoff to losing, s . Thus, I 's critical risk is the probability of II 's standing firm that leaves I indifferent to standing firm, which has an expected payoff of $r_I d_I + (1 - r_I)w_I$ and to submitting with its certain payoff of s_I . Hence, $r_I = (w_I - s_I) / (w_I - d_I)$. As Snyder and Diesing realize, this formulation is inconsistent with the game in Figure 1 in at least two important ways. First, the states in this game must choose their strategies independently. This means that even if I submits, it cannot secure s_I with certainty. Its expected payoff is $rs_I + (1 - r)c_I$, not s_I , where r is the probability that II will stand firm. The game Snyder and Diesing have in mind is a sequential game in which a state does not commit itself to a strategy independently of what its adversary does. "Our players move in sequence; thus the second player's choice

is taken in full knowledge of what the first has already chosen" (1977, 44). But as Wagner (1982) has emphasized, sequential play is not present in the formal models that they analyze, and this has important consequences.

Wagner (1982) also notes that Jervis's and Snyder and Diesing's formulations of a state's critical risk are inconsistent. Some algebra shows that Snyder and Diesing's measure is always greater than Jervis's. Consequently, a state's belief about the probability of its adversary's standing firm may be between these two measures. When this happens, a state will stand firm in Snyder and Diesing's model and submit in Jervis's.

The second aspect in which Snyder and Diesing's formulation is inconsistent with the two-by-two game of chicken is the assumption of complete information. Like Jervis, uncertainty about the opponent is crucial to their analysis. "A central assumption of the model is that, although each party knows its payoffs, it does not know its opponent's" (1977, 46). Yet, in none of the well-defined games they discuss is incomplete information modeled explicitly. The absence of incomplete information, as well as that of sequential play, weakens the game-theoretic foundations of Snyder and Diesing's examination of crises.

More recently, Brams (1985) and Wagner (1982) have extended the analysis of deterrence theory and MAD by considering extensive games that explicitly describe the sequence of play. Brams allows states to precommit themselves to particular strategies and thereby to carry out threats when the decision to do so is irrational (1985, 18, 35). As Brams realizes, the solutions to his games do not satisfy the credibility criterion described above. But he suggests that the strategies in his games may still be credible because there is always an autonomous risk that these strategies will lead to disaster. There is, however, no autonomous risk in his

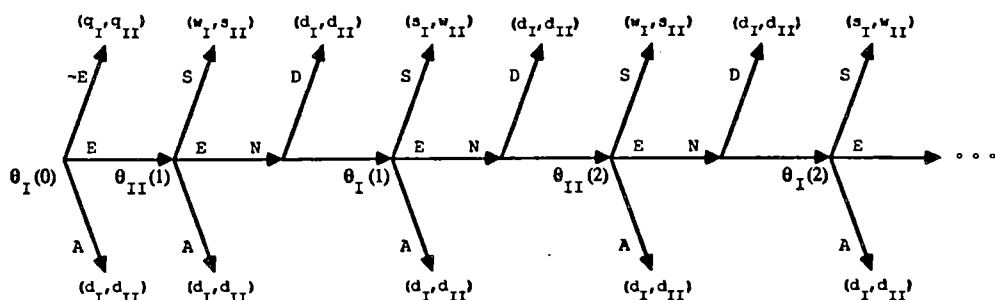
models and no way for the states in them to take this risk explicitly into account when formulating their strategies. Moreover, both Brams (1985, 22, 34, 44) and Wagner (1982, 345) recognize the importance of incomplete information and want to allow for it. Yet, incomplete information is not explicitly modeled in their games.

The ranking of the outcomes in the two-by-two game of chicken seems appropriate in that it captures the dilemma created by second-strike capabilities.³ But despite the appropriate ranking of these three outcomes, the fact that the game's equilibria in pure strategies are unaffected by changes in the states' payoffs is troubling.⁴ Somehow the balance of the states' resolve, that is, their relative willingness to run the risk of disaster, ought to influence, if not completely determine, which state prevails. Furthermore, a state's resolve should depend at least in part on its payoffs. Finally, this review also suggests that the game-theoretic foundations of these conclusions are problematic.

The Game with Complete Information

The essence of the strategy-that-leaves-something-to-chance is that during a crisis each state attempts to coerce its adversary into submission by manipulating an autonomous risk of disaster. This section tries to capture this in a game of complete information that is sufficiently simple that its equilibria may be found. The section also compares this game to critical risk and two-by-two chicken models of deterrence. The comparison shows that treating crises formally as a sequential process yields many of the conclusions suggested by these models in a way that avoids many of the theoretical difficulties that rigorous formulations based on the models have encountered.

Figure 2. The Game with Complete Information



The game with complete information is illustrated in Figure 2. Again, there are two states, *I* and *II*. The game begins with *I*'s having to decide whether to exploit an opportunity or not, *E* and $\sim E$, or to launch a massive nuclear attack, *A*. If *I* does not exploit the situation, the game ends with the status quo payoffs of (q_I, q_{II}) . Attacking ends the game in disaster with payoffs (d_I, d_{II}) . If *I* exploits, then *II* must choose among the options of submitting, continuing the crisis, or launching a massive attack, *S*, *E*, and *A*. If *II* submits, the game ends with *I*'s prevailing and the state's receiving (w_I, s_{II}) . If *II* attacks, the game again ends in disaster with (d_I, d_{II}) . If *II* continues, it does so by deciding to generate an autonomous risk *f* that the crisis will "go out of control" at this point and end in disaster. The autonomous risk is modeled as a move by Nature. That is, if the game continues with *II*'s decision to escalate, Nature moves next and plays disaster with probability *f*. If there is a disaster, it, as always, yields (d_I, d_{II}) . If there is no disaster, the onus of escalation shifts to *I*. It must select among submitting, escalating, or attacking. Submission ends the game with (s_I, w_{II}) , and attacking ends the game with (d_I, d_{II}) . If *I* escalates, then by assumption it can do so only by bidding up the risk of disaster by the increment *f*. The autonomous risk that *I* creates if it escalates at this point is $2f$. If *I*

does escalate, Nature then moves by playing disaster with probability $2f$. If disaster is avoided, the onus of escalation shifts back to *II*, which now must generate an autonomous risk of $3f$ if it chooses to escalate. The game continues in this way until one state submits or launches an attack or until there is a disaster.

The game can end in one of four ways, (q_I, q_{II}) , (w_I, s_{II}) , (s_I, w_{II}) , or (d_I, d_{II}) . Because prevailing is better than submitting, and submitting is better than disaster, $w_I > s_I > d_I$ and $w_{II} > s_{II} > d_{II}$. Because the status quo is also better than submitting, $q_I > s_I$ and $q_{II} > s_{II}$. For *I*, it must also be that prevailing is better than the status quo: $w_I > q_I$. Otherwise, *I* could obtain its highest payoff at its first move by not exploiting the situation, and there would be no crisis. Hence, $w_I > q_I > s_I > d_I$. The relation between q_{II} and *II*'s other payoffs is ambiguous but also inconsequential because by the time *II* has to make its first decision, q_{II} is already in the past.

The risks escalation creates in the crisis follow the pattern $f, 2f, 3f, \dots Kf$, where $Kf = 1$ and, for convenience, K is taken to be an integer. If K is even, I has the last move in the game because to escalate for the $K/2$ time, it must generate a probability of disaster equal to one. If K is odd, II 's $(K + 1)/2$ move is the last possible move. Although some analyses (e.g., Schelling 1966; Snyder and Diesing 1977;

Wagner 1982) have discussed the bargaining disadvantage that accrues to the side with the last possible chance to avoid disaster, who actually has the last physical move in the game tree has no effect on the solution to this game, as will be shown. For now, K is merely assumed to be even.

An obvious generalization of this game would be to permit the states to continue the crisis by letting them generate any level of autonomous risk they wish instead of restricting them to continuing in only one way, namely, to bid up the risk in increments of f . This generalization would allow states to move up the escalation ladder by increasing the risk and to move down the ladder by decreasing the risk. Unfortunately, expanding the strategy space in this way makes the game immensely more complicated.

It will prove convenient to adopt the following notation for labeling the states' information sets, which, in this game of complete information, are singletons. Let $\Theta_{II}(n)$ be the information set at which II must decide whether or not to escalate for the n th time where $1 \leq n \leq K/2$. Thus, $\Theta_{II}(n)$ presents II with the choice of submitting, attacking, or escalating by creating an autonomous risk of $(2n - 1)f$. (Recall that because the onus of escalation shifts back and forth, II only makes bids of odd multiples of f .) Similarly, $\Theta_I(n)$ for $1 \leq n \leq K/2$ is the information set at which I must decide to submit, attack, or escalate with a probability of disaster of $2nf$. $\Theta_I(0)$ is the information set at which I has to choose among attacking, exploiting the situation, or not exploiting it.

At each information set $\Theta_I(n)$ and $\Theta_{II}(n)$ for $1 \leq n \leq K/2$, a state has a choice among the three alternatives of submitting, attacking, or escalating. Accordingly, a behavioral strategy for I at any information set $\Theta_I(n)$ for $1 \leq n \leq K/2$ may be described by the probability of attacking at that set, $a_I(n)$, and the probability of escalating, $e_I(n)$. For $n = 0$,

I may attack with probability $a_I(0)$, exploit the situation with probability $e_I(0)$, or not exploit the situation. The probability of submission for $1 \leq n \leq K/2$ and of not exploiting the situation for $n = 0$ is given by $1 - a_I(n) - e_I(n)$. A behavioral strategy for II at any information set $\Theta_{II}(n)$ for $1 \leq n \leq K/2$ may be described by the probability of attacking, $a_{II}(n)$, and the probability of escalating, $e_{II}(n)$.

The notion of a "crisis equilibrium" will play an important role in the analysis of the game with both complete and incomplete information. For Snyder and Diesing, "there is no crisis unless one state challenges another and this challenge is resisted" (1977, 13). Formalizing this, a crisis equilibrium in the game will be taken to be an equilibrium in which there is some positive probability of I 's exploiting the situation at $\Theta_I(0)$ and some positive probability of II 's escalating or attacking at $\Theta_{II}(1)$. The positive probability of I 's exploiting the situation at $\Theta_I(0)$ means that in a crisis equilibrium there is some chance of I 's challenging II . Similarly, the positive probability of II 's escalating or attacking at $\Theta_{II}(1)$ implies that there is some chance that a challenge will be resisted. Crisis equilibria are the only equilibria of this game in which there is a positive probability of a challenge leading to disaster. This is also in keeping with Schelling (1966, 92-105) and Snyder and Diesing (1977, 6-21), who believe that there must be some risk of war for there to be a crisis.

Finally, the credibility criterion discussed above will be formalized by requiring the game's equilibria to be sequential. That is, starting from any place in the game tree, a state's equilibrium strategy is an optimal strategy for the rest of the game, given the state's information and beliefs at that point in the game and given the strategy of the other state. In a sequential equilibrium, a state cannot improve its payoff by deviating from its equilibrium strategy anywhere in the

game, given its beliefs about where it is in the game.⁵ Sequential equilibria thus require a state to decide to act in a way that minimizes its expected cost when it must actually make the decision, given its information and beliefs at that time. Accordingly, a state can never act "irrationally," and this seems to capture the essence of the credibility criterion.⁶

One might object that requiring the states' strategies to be sequentially rational and then relying on Nature to impose the irrational sanction does not really solve the credibility problem. I agree with this criticism. But it is important to realize that this is not so much a criticism of the model as it is a fundamental criticism of the way that the strategy-that-leaves-something-to-chance has attempted to overcome the credibility problem. The model only exposes this weakness.

The analysis of the extensive game begins by recalling that the two-by-two chicken models had two Nash equilibria in pure strategies in which one or the other state stood firm while its adversary submitted. These equilibria were insensitive to changes in the payoffs, and this seemed to leave no role for resolve to play. The obvious analogy in the game in Figure 2 to the strategy of standing firm in the two-by-two game of chicken is the strategy of always escalating. That is, a state escalates at every one of its information sets regardless of what the other state does. The equally obvious analogy to submitting is for a state to end the crisis by submitting at the first opportunity it has. For *I*, this occurs at $\Theta_I(0)$ when it can decide not to exploit the situation. The earliest time that *II* can submit is at $\Theta_{II}(1)$.

Given these definitions of standing firm and submitting, the combination of them is a Nash equilibrium in the extensive game. If *I* stands firm by always escalating, that is, $e_I(n) = 1$ for $0 \leq n \leq K/2$, *II* can never prevail. The best it can do is avoid running any risk of disaster by sub-

mitting at $\Theta_{II}(1)$ and obtaining s_{II} . Given *II*'s strategy of submitting at $\Theta_{II}(1)$, *I* can do no better than standing firm, which offers a payoff of w_I . Neither state can gain from deviating from its strategy, given the strategy of the other. A similar argument shows that *I*'s submitting at $\Theta_I(0)$ and *II*'s standing firm is also a Nash equilibrium.

These two equilibria, like their analogues in the two-by-two game, are insensitive to changes in the states' payoffs as long as $w_I > q_I > s_I > d_I$ and $w_{II} > s_{II} > d_{II}$. The balance of resolve does not affect them. So far, resolve plays no part in this model either, but, as will be seen, there is also something wrong with these equilibria. They are not sequential and, accordingly, are incredible. However, when the game is solved for its sequential equilibria, resolve begins to play a role. Indeed, the state with the greatest resolve prevails.

To demonstrate this, *resolve* must finally be defined. Recall that resolve in the critical-risk models is the greatest risk of disaster that a state is willing to run. But the greatest risk that a state is willing to run at any point in the crisis would seem likely to vary during the crisis depending on what the state believes about its adversary's willingness to run risks.⁷ Nevertheless, it is possible to specify a simple upper bound on the risk of disaster a state is willing to run. A state's resolve, R , is the probability of disaster that makes it indifferent between the payoffs of submitting and of escalating, given that the state believes its adversary is certain to submit at its next opportunity. R satisfies $Rd + (1 - R)w = s$ and is given by $R = (w - s)/(w - d)$, where the subscripts have been dropped because the formulae apply to both *I* and *II*. The expression for R is the same as that in Ellsberg 1959, Snyder 1971 and 1972, and Snyder and Diesing 1977, as one might expect because their analyses are based on sequential play. But here, R is only an upper bound

because in general a state cannot be sure that its adversary will submit at its next opportunity, and this uncertainty reduces the expected return to escalating.

A state's resolve may be used to determine an upper bound on the number of times that a state will escalate in a crisis, and this upper bound will be crucial to finding the game's sequential equilibria. A state clearly will not escalate if to do so it must generate an autonomous risk greater than its resolve. Thus, for I , the greatest integer n that satisfies the relation $2nf \leq R_I$ is an upper bound for the number of times that I can rationally decide to escalate. Similarly, the largest integer satisfying $(2n - 1)f \leq R_{II}$ is an upper bound for II . Let N_I and N_{II} denote these upper bounds and, to avoid some technicalities, assume $2nf < R_I$ and $(2n - 1)f < R_{II}$. Then, the following proposition may be demonstrated:

PROPOSITION 1: *The extensive game with complete information has a unique sequential equilibrium. If $N_I \geq N_{II}$, I prevails in this equilibrium, and if $N_{II} > N_I$, II prevails in the sense that I does not exploit the situation at $\Theta_I(0)$.*

Before proving this, it is worth noting that because of the simple bidding structure of the game in which bids must be made in increments of f and must alternate between I and II , it is not quite true that the state with the greatest resolve prevails. To see this, recall that N_I and N_{II} must satisfy $2N_I f \leq R_I \leq 2(N_I + 1)f$ and $(2N_{II} - 1)f \leq R_{II} \leq (2N_{II} + 1)f$. Hence, $[2(N_{II} - N_I) - 3]f \leq R_{II} - R_I \leq [2(N_{II} - N_I) - 1]f$. Now if $0 \leq N_{II} - N_I \leq 1$, both small positive and negative values of $R_{II} - R_I$ can satisfy these inequalities. Thus, because of the discrete bidding structure, if N_I and N_{II} are sufficiently close together, it may be the case that $R_I > R_{II}$ while $N_{II} > N_I$.

This issue may be finessed by defining the state with the greatest "effective

resolve" to be the state that is willing to make the last bid. So, I has the greatest effective resolve if $N_I \geq N_{II}$, and II has the greatest effective resolve if $N_I < N_{II}$. Proposition 1 may now be rephrased to say that the game has a unique sequential equilibrium and that in it the state with the greatest effective resolve prevails.

Proof of Proposition 1. A sequential equilibrium is a set of beliefs and strategies that is consistent and sequentially rational. Because all of the information sets in the game are singletons, there is a unique set of consistent beliefs. Accordingly, finding the sequential equilibrium amounts to finding a strategy for each state at each of its information sets, that is, specifying $a_I(n)$ and $e_I(n)$ for $0 \leq n \leq K/2$ and $a_{II}(n)$ and $e_{II}(n)$ for $1 \leq n \leq K/2$, such that at no place in the game tree can a state gain by deviating from its strategy, given its beliefs and its adversary's strategy.

The sequential equilibrium is found through backwards induction. Assume $N_I \geq N_{II}$ (the proof is identical if $N_I < N_{II}$). For any information set $\Theta_I(n)$ for $N_I < n \leq K/2$, I 's unique best reply is submission: $a_I(n) = 0$ and $e_I(n) = 0$. Similarly, II 's unique best response at $\Theta_{II}(n)$ for $N_{II} < n \leq K/2$ is submission: $a_{II}(n) = 0$ and $e_{II}(n) = 0$. Given II 's strategy of submitting for $n > N_{II}$, I 's only best reply at $\Theta_I(n)$ for $N_{II} \leq n \leq N_I$ is to escalate: $e_I(n) = 1$. Given I 's best reply of escalating for $N_{II} \leq n \leq N_I$, II 's only best response at $\Theta_{II}(N_{II})$ is to submit. Given II 's strategy of submitting for $n \geq N_{II}$, I 's only best reply at $\Theta_I(N_{II} - 1)$ is to escalate: $e_I(N_{II} - 1) = 1$. Given I 's only best reply of escalating for $N_{II} - 1 \leq n \leq N_I$, II 's only best reply at $\Theta_{II}(N_{II} - 1)$ is to submit. This process eventually yields a combination of strategies in which I escalates with probability one for $0 \leq n \leq N_I$ and submits with probability one for $N_I < n \leq K/2$ and in which II , foreseeing that I is willing to escalate to higher levels than II ,

submits with probability one for $1 \leq n \leq N_{II}$. Because all of the best replies are unique and there is only one set of consistent beliefs, this combination of strategies is also the unique sequential equilibrium. QED⁸

Thus, modeling the autonomous risks of disaster generated by escalation explicitly as Nature's moves and requiring equilibria to be sequential in a well-defined game yields many of the conclusions suggested by previous analyses of MAD and deterrence. First, note that in the equilibrium strategies, no state gives a positive probability to launching a massive nuclear attack: $a_I(m) = 0$ and $a_I(n) = 0$ for all m and n . No state ever deliberately attacks. Moreover, the sequential equilibrium depends on the states' payoffs. Indeed, the game is a contest of resolve in which the state with the greatest effective resolve prevails.

There is, however, a problem with the game as it has been developed so far. There are no sequential equilibria that are also crisis equilibria. If the balance of effective resolve favors I , then in the sequential equilibrium, II does not resist I 's challenge and there is no crisis. If the balance favors II , I never challenges II , and there is no crisis. On reflection, the lack of any crisis is hardly surprising. If a crisis is truly a bargaining process, then there must be something to bargain about and something to be communicated. But in a game with complete information, there is by definition no private information. Each state knows what the other state knows, and there is nothing to be communicated. As has so often been recognized, crises require incomplete information.

The Game with Incomplete Information

The game with incomplete information is illustrated in Figure 3. To simplify mat-

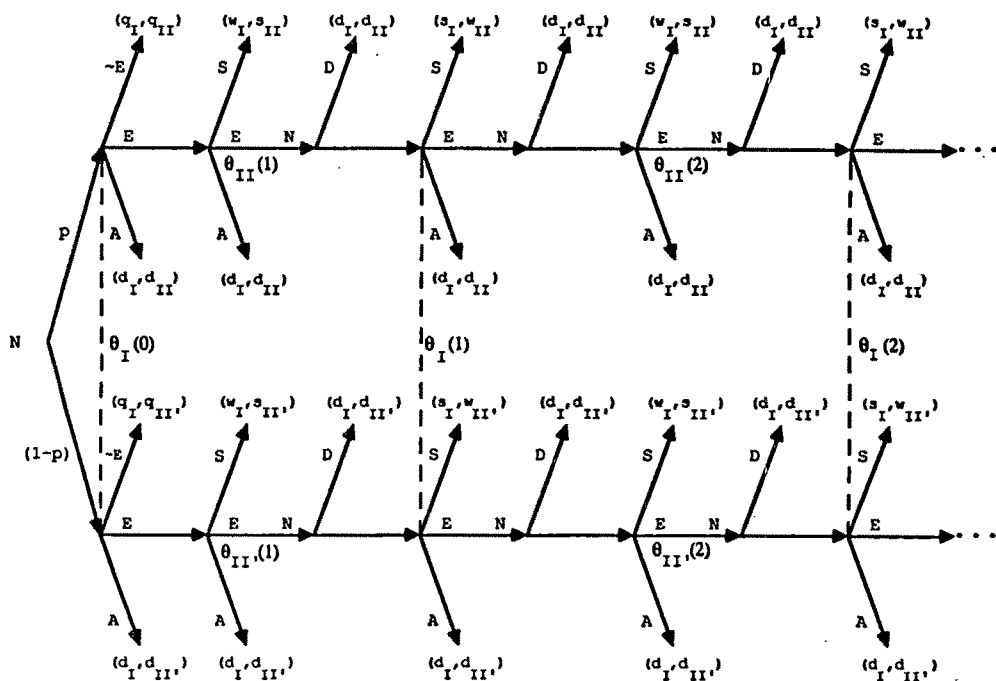
ters, there is only one-sided incomplete information: only I is uncertain of the type of adversary that it is facing. I 's adversary is certain that it is facing I . Although I is uncertain of the type of its adversary, there are only two possibilities. I is either facing an irresolute adversary denoted by II or a resolute adversary II' . Moreover, I believes that it is facing the irresolute adversary with probability p and the resolute adversary with probability $(1 - p)$ where p is common knowledge.

I 's uncertainty about the type of the adversary it is facing is modeled by letting Nature start the game by choosing the type of adversary I will face and by assuming that I does not know the actual type that Nature selects. After Nature's initial move, the order of play, the alternatives open to the two states at each move, and the simplified escalatory process in which the autonomous risk must be increased in increments of f are the same as in the game with complete information. The only difference between the games of complete and incomplete information is that when I decides what to do with incomplete information, I does not know whether it is facing II or II' .

The levels of resolve of all three types of states differ. Indeed, this is the only characteristic that distinguishes the irresolute from the resolute adversary. Assume that I is willing to escalate, at most, once: $N_I = 1$. Recalling that R_I and N_I are related as $2N_I f < R_I < 2(N_I + 1)f$, $N_I = 1$ implies $2f < R_I < 4f$. Also assume that the irresolute adversary is willing to escalate, at most, once, and the resolute adversary is willing to escalate, at most, twice. That is, $N_{II} = 1$ and $N_{II'} = 2$. $N_{II} = 1$ implies $f < R_{II} < 3f$, and $N_{II'} = 2$ means $3f < R_{II'} < 5f$.

Uncertain of the resolve of its adversary, I faces a dilemma. If I were certain it was facing an irresolute adversary, then by Proposition 1 and the fact that $N_I \geq N_{II}$, I would prevail. But if I were certain

Figure 3. The Game with Incomplete Information



that it was facing a resolute adversary, it would not challenge II' . I 's behavior in this situation depends on its initial belief that it is facing II , that is, on the value of p , and on what it may learn about the type of its adversary from its behavior in the crisis. The sequential equilibria of the game formalize this dependence.⁹

PROPOSITION 2. *If the incomplete-information game has a sequential crisis equilibrium, then the strategies are given by*

$$\begin{aligned} a_I(n) &= a_{II}(n) = a_{II'}(n) = 0 & \text{for all } n \\ e^*_{I(0)} &> 0 \\ e^*_{I(1)} &= [R_{II} - f]/(1 - f) [(1 - 2f) R_{II} + 2f] \\ e^*_{I(n)} &= 0 & \text{for } n > 1 \\ e^*_{II(1)} &= [\beta^*(1 - p)]/p(1 - \beta^*) \\ \text{where } \beta^* &= [1 - (1 - 2f)(1 - 3f)] [1 - R_{II}] / \\ & (1 - 2f) [(1 - 3f)R_I + 3f] \end{aligned}$$

$$\begin{aligned} e^*_{II}(n) &= 0 & \text{for } n > 1 \\ e^*_{II'}(n) &= 1 & \text{for } n \leq N_{II'} \\ e^*_{II'}(n) &= 0 & \text{for } n > N_{II'} \end{aligned}$$

And at $\Theta_I(1)$, I believes it is facing II with probability β^* . Finally, for any particular values of the payoffs, a sequential crisis equilibrium exists for some p .

Proposition 2, a proof of which may be found in the Appendix, suggests the following description of the crisis: For there to be any chance of a challenge, $e^*_{I(0)} > 0$. If there is a challenge and I happens to be facing an irresolute adversary, then this adversary escalates with probability $e^*_{II}(1)$. If I is facing a resolute adversary, then II' escalates with probability one. If the crisis does not go out of control at this point, the onus of escalation shifts back to

I. *I* began the crisis believing that the probability that it was facing an irresolute adversary was p . *I* now reassesses its belief in light of its adversary's response to *I*'s challenge and concludes that the probability that it is facing an irresolute adversary is β^* . Given this new belief, *I* escalates with probability $e^*_I(1)$. If the crisis remains under control, *I*'s adversary must now act. If this adversary is irresolute, it finds the risk of disaster too great and submits. If, however, *I*'s adversary is resolute, then *II*' escalates with probability one and shifts the onus of escalation back to *I*. Knowing that an irresolute adversary would not have escalated, *I* concludes that it is facing a resolute adversary and submits.

Results and Analysis

The explicit and relatively simple equilibrium strategies in Proposition 2 make it possible to compare the bargaining process they describe with the bargaining processes described in other analyses. The comparison reveals points of both agreement and disagreement.

As noted above, many analyses of crises have argued that incomplete information about an adversary plays a crucial role (e.g., Jervis 1972, 1976, 1978; Snyder 1971, 1972; Snyder and Diesing 1977; Wagner 1982, 1983). Indeed, "it is largely because of the lack of complete information that crises occur at all" (Snyder and Diesing 1977, 8). The models in this paper support this conclusion. As the proof of Proposition 1 shows, there are no sequential crisis equilibria in the game with complete information. These may exist only if there is incomplete information. Incomplete information is a prerequisite to crisis.

If there is no uncertainty, there is no crisis and no risk of disaster. How does the risk of disaster vary with the level of *I*'s uncertainty about the type of its adversary and, in particular, what level of

uncertainty creates the greatest risk of disaster? The probability of disaster, P_d , which is the probability of reaching a node with payoffs (d_I, d_{II}) , is given by

$$P_d = pe^*_I(0)(e^*_{II}(1)\{f + (1-f)[e^*_{II}(1)2f]\} \\ + (1-p)e^*_I(0)(f + (1-f)\{e^*_I(1) \\ [2f + (1-2f)3f]\})$$

Substituting the expression from Proposition 2 for $e^*_{II}(1)$ shows that P_d varies linearly in $(1-p)$ when $e^*_I(0) > 0$. When $e^*_I(0) = 0$, P_d drops to zero, as there is no longer a crisis. P_d is largest and the risk of disaster is greatest when p is just large enough to make *I* confident enough that it is facing an irresolute adversary to exploit the situation.

The intuition behind this is that because a resolute adversary pushes harder in the crisis than an irresolute adversary, the crisis is more dangerous, the greater the chances that *I* is facing the resolute adversary, that is, the smaller p is. If, however, the chances of facing a resolute adversary are too great, *I* does not exploit the situation because the status quo payoff of q_I is better. The crisis is most dangerous when the prospect of facing an irresolute adversary is just good enough to induce *I* to begin the crisis by exploiting the situation.

Perhaps the most interesting conclusions suggested by Proposition 2 are those that conflict with other analyses of MAD and deterrence theory. In these analyses, MAD crises are a competition in taking risks in which the state "willing to run the greatest risks will prevail" (Jervis 1979b, 631). In this essay's formulation of a MAD crisis based on the strategy-that-leaves-something-to-chance, sometimes the state with the least resolve prevails. Sometimes bluffing succeeds.

To see this, note first that the existence of autonomous risk means that the claim that the state with the greatest resolve will prevail must be qualified. Because the autonomous risk of disaster is genuine, there is some chance that no state will pre-

vail, regardless of which has the greatest resolve. At most, one might make the conditional claim that if one state prevails, it will be the state with the greatest resolve.

Proposition 1 might be seen to support this conditional claim. The Proposition shows that in the complete-information game, the state with the greatest effective resolve always prevails. But remember that because of complete information, there are no sequential crisis equilibria in this game. When incomplete information is included, sometimes the state with the least resolve prevails. In this incomplete-information game, the effective resolve of *I* is greater than that of an irresolute adversary because $N_I \geq N_{II}$. But there is some chance that an irresolute adversary will prevail. Indeed, this chance is equal to the probability of the game's reaching an outcome with payoffs (s_I, w_{II}) . Given that *I*, although uncertain of its adversary, is actually facing *II*, this probability is $e^*_{II}(0)e^*_{II}(1)(1-f)(1-e^*_I(1)) > 0$. Thus, in the contest of resolve, the state with the least resolve may prevail.

Many analyses based on critical risks suggest that a state is less likely to escalate the greater it perceives its adversary's resolve to be (e.g., Ellsberg 1959; Jervis 1972, 1978; Snyder 1971, 1972; Snyder and Diesing 1977). In the present game this is the case for *II*. But it is not the case for *I*. *I* is more likely to escalate, the greater the resolve of its adversary *II*.

To understand that *II* is less likely to escalate, the greater *I*'s resolve, consider the expression for $e^*_{II}(1)$, which is the probability that *II* will escalate. The partial derivative of $e^*_{II}(1)$ with respect to *I*'s resolve, R_I , is negative. Thus, as long as $e^*_{II}(1)$ is differentiable, an increase in R_I reduces $e^*_{II}(1)$. *II* is less likely to escalate.¹⁰

Unlike *II*, *I* is more likely to escalate if the resolve of its adversary is greater. The partial derivative of $e^*_I(1)$ with respect to

R_{II} is positive. Hence, $e^*_I(1)$ increases as R_{II} increases as long as $e^*_I(1)$ is differentiable. *I*, therefore, is more likely to escalate, the greater *II*'s resolve.

The mathematics of this result is easy to see. Suppose that in the initial equilibrium, $e^*_{II}(1) < 1$. Thus, *II* is randomizing over the alternatives of escalating and submitting at $\Theta_{II}(1)$. Randomization implies that *II* is indifferent to submitting and to escalating. Now let R_{II} increase because, say, w_{II} increases. The expression for $e^*_{II}(1)$ is independent of R_{II} , so $e^*_{II}(1)$ remains less than 1. *II* continues to randomize and, therefore, to be indifferent to submitting and escalating in the new equilibrium. The expected payoff to submitting is s_{II} in both equilibria, so the expected payoff to escalating at $\Theta_{II}(1)$ in both equilibria must be the same. But the increase in w_{II} makes the return to *I*'s submission at $\Theta_I(1)$ greater in the new equilibrium. If, therefore, the expected return to *II* of escalating at $\Theta_{II}(1)$ is to remain the same in the two equilibria, the increased value of *I*'s submission must be offset by a lower probability of *I*'s submitting. Consequently, the probability of *I*'s escalating must increase.

The intuition behind this counterintuitive outcome begins by considering the dilemma confronting *I*. During the crisis, *I* attempts to determine whether it is facing an irresolute or a resolute adversary by testing its adversary's resolve by forcing it to run various levels of risk. The greater the level of resolve of the irresolute adversary, the more *II* is like *II'*. Discriminating between the two types is thus more difficult and requires a more stringent test. A more stringent test, in turn, means that *I* must pose a greater, not a smaller, risk of disaster if *I* is to force a more determined irresolute adversary to backdown.

An immediate corollary follows from the relation between the probability of a state's escalating and the resolve of its adversary. Differentiating the expression for P_d shows that the greater the resolve

of I , the smaller is the probability of disaster and the less dangerous the crisis. But the greater the resolve of II , the greater the probability of disaster and the more dangerous the crisis. Sometimes having more resolve makes disaster less likely and at other times, more likely.

Conclusions

Many studies of bargaining and escalation have recognized the importance of incomplete information. But to the extent that these analyses have used game-theoretic models, they have not modeled the lack of complete information explicitly within a well-defined game. This essay has attempted to extend those analyses, at least in so far as they apply to a MAD crisis in which states follow the strategy-that-leaves-something-to-chance, by modeling the bargaining process in the crisis as a sequential game with incomplete information. This essay has also tried to provide more secure game-theoretic foundations for previous analyses of this type of crisis that have been based on the notions of *resolve* and *critical risk*.

The games developed here are very simple. Because the formulation is so simple, the game's sequential crisis equilibria may be characterized explicitly. This, in turn, makes it possible to compare the bargaining dynamics in this simple but well-defined game with the dynamics examined within the context of models that, although richer, tend not to be completely specified. The comparison reveals several inconsistencies.

A MAD crisis is a contest of resolve, and several analyses of MAD conclude that the state with the greatest resolve will prevail. This is not the case in the model developed in this essay. Sometimes the state with the weakest resolve prevails: sometimes bluffing succeeds. And, although many models based on critical risks suggest that a state is less likely to

escalate if its adversary's resolve is perceived to be greater, this need not be the case. Some states are more likely to escalate if their adversaries' resolve is greater.

Appendix

Proof of Proposition 2. Proposition 2 will be proved through backwards induction. Finding the sequential equilibria entails specifying a strategy for I , II , and II' at each information set $\Theta_I(m)$, $\Theta_{II}(n)$, and $\Theta_{II'}(n)$ for $0 \leq m \leq K/2$ and $1 \leq n \leq K/2$ and a set of consistent beliefs for I about the type of its adversary. As before, no state can attach a positive probability to attacking in equilibrium, for otherwise it would be possible for the state to improve its expected return by deviating from its strategy. Thus, $a_I(m) = 0$ for $0 \leq m \leq K/2$ and $a_{II}(n) = a_{II'}(n) = 0$ for $1 \leq n \leq K/2$. Finding the strategies in the sequential equilibria amounts to determining $e^*_{I'}(m)$ for $0 \leq m \leq K/2$ and $e^*_{II}(n)$ and $e^*_{II'}(n)$ for $1 \leq n \leq K/2$.

For any $n > N_I = 1$, I 's only best reply is to submit: $e^*_{I'}(n) = 0$. For any $n > N_{II} = 1$, II 's only best reply is to submit: $e^*_{II}(n) = 0$. And for any $n > N_{II'} = 2$, the only best reply for II' is to submit: $e^*_{II'}(n) = 0$. I 's strategy of $e^*_{I'}(n) = 0$ for $n > 1$ means that if II' escalates at $\Theta_{II'}(2)$, II' will have to run a risk of $3f$ of disaster. But if there is no disaster, I will then submit. Accordingly, the expected return to escalating at $\Theta_{II'}(2)$ is $3fd_{II'} + (1 - 3f)w_{II'}$. Recalling that $R_{II'} = (w_{II'} - s_{II'})/(w_{II'} - d_{II'})$, the assumption that $R_{II'} > 3f$ ensures that the expected value of escalation is greater than the payoff from submitting, that is, $3fd_{II'} + (1 - 3f)w_{II'} > s_{II'}$. Hence, II' will escalate at $\Theta_{II'}(2)$: $e^*_{II'}(2) = 1$. The only decisions that remain to be specified are those made at $\Theta_I(0)$, $\Theta_I(1)$, $\Theta_{II}(1)$, and $\Theta_{II'}(1)$.

Two observations show that in a crisis equilibrium, II' escalates with probability

one at $\Theta_{II}(1)$. Recall that in a crisis equilibrium there must be some chance of a challenge leading to disaster. This means that $e_I(0) > 0$ and that $e_{II}(1) > 0$ or $e_{II'}(1) > 0$ in equilibrium. The first observation is that $e_{II}(1) > 0$ in a crisis equilibrium. To see this, suppose the contrary. That is, assume $e_{II}(1) = 0$. From the definition of a crisis equilibrium, if $e_{II}(1) = 0$, then $e_{II'}(1) > 0$. But if $e_{II}(1) = 0$ and $e_{II'}(1) > 0$, then I is certain that it is facing II' if I reaches $\Theta_I(1)$. I 's best reply at $\Theta_I(1)$ is, therefore, $e_I(1) = 0$. If, however, $e_I(1) = 0$, then because I is certain to submit at $\Theta_I(1)$, II 's best response at $\Theta_{II}(1)$ is to escalate, not to submit. II 's best reply is $e_{II}(1) = 1$, not $e_{II}(1) = 0$. Hence, II 's strategy of $e_{II}(1) = 0$ is not a best reply to I 's unique best reply to II 's strategy, since II can gain by deviating from its strategy if I 's strategy is $e_I(1) = 0$. This means that $e_{II}(1) = 0$ cannot be part of an equilibrium that is also a crisis equilibrium. Having reached a contradiction, it must be that $e_{II}(1) > 0$ in a crisis equilibrium.

The second observation is that if $e_{II}(1) > 0$ in equilibrium, then $e_{II'}(1) = 1$ in equilibrium. Informally, this merely says that if an irresolute adversary would escalate, so would a resolute adversary. More formally, let \hat{e}_{II} be the value of $e_I(1)$ that leaves II indifferent to escalating at $\Theta_{II}(1)$ and to submitting. Thus, \hat{e}_{II} satisfies $f d_{II} + (1 - f) \{ (1 - \hat{e}_{II}) w_{II} + \hat{e}_{II} [2f d_{II} + (1 - 2f) s_{II}] \} = s_{II}$ and is given by $\hat{e}_{II} = (R_{II} - f) / (1 - f) [(1 - 2f) R_{II} + 2f]$. Because $f < R_{II} < 3f$, $\hat{e}_{II} < 2f / (1 - f) [1 - (1 - 2f) (1 - 3f)]$. Now let $\hat{e}_{II'}$ be the value of $e_I(1)$ that leaves II' indifferent to escalating at $\Theta_{II'}(1)$ and to submitting. Then, $\hat{e}_{II'}$ satisfies $f d_{II'} + (1 - f) \{ (1 - \hat{e}_{II'}) w_{II'} + \hat{e}_{II'} \{ 2f d_{II'} + (1 - 2f) [3f d_{II'} + (1 - 3f) w_{II'}] \} \} = s_{II'}$ and is given by $\hat{e}_{II'} = (R_{II'} - f) / (1 - f) [1 - (1 - 2f) (1 - 3f)]$. Because $3f < R_{II'} < 5f$, $\hat{e}_{II'} > 2f / (1 - f) [1 - (1 - 2f) (1 - 3f)]$. Therefore, $\hat{e}_{II'} > \hat{e}_{II}$. From the definition of \hat{e}_{II} , II can escalate in equilibrium, that is, $e_{II}(1) > 0$, only if $e_I(1) \leq \hat{e}_{II}$. But if $e_I(1) \leq \hat{e}_{II}$, then

$e_I(1) < \hat{e}_{II'}$ and from the definition of $\hat{e}_{II'}$, this implies that $e_{II'}(1) = 1$.

In sum, the first observation says that $e_{II}(1) > 0$ in a crisis equilibrium. The second observation shows that if $e_{II}(1) > 0$, then $e_{II'}(1) = 1$ in equilibrium. Hence, $e_{II'}^*(1) = 1$ if a sequential crisis equilibrium exists. Only the strategies at $\Theta_I(0)$, $\Theta_I(1)$, and $\Theta_{II}(1)$ remain to be determined.

II 's strategy at $\Theta_{II}(1)$ depends on what I does at $\Theta_I(1)$. Recall that \hat{e}_{II} is the value of $e_I(1)$ that leaves II indifferent to submitting and escalating at $\Theta_{II}(1)$. Thus, II 's best response to $e_I(1)$ is 0 if $e_I(1) > \hat{e}_{II}$; 1 if $e_I(1) < \hat{e}_{II}$; and anything in the interval $[0, 1]$ if $e_I(1) = \hat{e}_{II}$. II 's best-reply correspondence, $br_{II}[e_I(1)]$, is graphed in Figure 4, assuming that $e_{II}(1) = 1$, which is true over the region $0 \leq e_I(1) \leq \min \{1, \hat{e}_{II'}\}$.

I 's best-reply correspondence to $e_{II}(1)$, $br_I(e_{II}(1))$, can be found by considering its payoffs to escalating and submitting at $\Theta_I(1)$. If I escalates when facing II , it will have to hazard a $2f$ risk of disaster. But if there is no disaster, II will then immediately submit. I 's expected payoff if it is facing II is $2f d_I + (1 - 2f) w_I$. If, however, I escalates when it is facing II' , II' will escalate and, as long as there is no disaster, I will then submit. In this case, the expected return to escalation is $2f d_I + (1 - 2f) [3f d_I + (1 - 3f) s_I]$. Thus, the expected return to escalation is $\beta [2f d_I + (1 - 2f) w_I] + (1 - \beta) \{ 2f d_I + (1 - 2f) [3f d_I + (1 - 3f) s_I] \}$ where I believes that it is facing II with probability β . Let β^* be the belief that leaves I indifferent to submitting and escalating. Then, β^* satisfies $\beta^* [2f d_I + (1 - 2f) w_I] + (1 - \beta^*) \{ 2f d_I + (1 - 2f) [3f d_I + (1 - 3f) s_I] \} = s_I$ and is given by $\beta^* = [1 - (1 - 2f) (1 - 3f)] (1 - R_I) / (1 - 2f) [(1 - 3f) R_I + 3f]$. Algebra shows $0 < \beta^* < 1$. So, if $\beta > \beta^*$, I 's best reply is to escalate: $e_I(1) = 1$. If $\beta < \beta^*$, I 's optimal strategy is to submit: $e_I(1) = 0$. And if $\beta = \beta^*$, I does not have a unique

best reply and may choose any value in the interval $[0, 1]$ for $e_I(1)$.

In a sequential equilibrium, beliefs are consistent with Bayes's rule where this rule may be applied. Thus, β is given by the probability of I facing II at this information set, $pe_I(0)e_{II}(1)(1-f)$, divided by the probability of reaching this information set, $pe_I(0)e_{II}(1)(1-f) + (1-p)e_I(0)e_{II}(1)(1-f)$. Recalling that $e_{II}(1) = 1$ in a sequential crisis equilibrium, $\beta = pe_{II}(1)/[pe_{II}(1) + (1-p)]$. Because I 's best reply is to escalate if $\beta > \beta^*$, $e_I(1) = 1$ is I 's best reply when $e_{II}(1) > \beta^*(1-p)/p(1-\beta^*)$. If $e_{II}(1) < \beta^*(1-p)/p(1-\beta^*)$, then $\beta < \beta^*$ and $e_I(1) = 0$ is I 's best reply. And if $\beta = \beta^*$, I 's best reply can be any point in the interval $[0, 1]$. Figure A-1 also shows I 's best-reply correspondence.

As Figure A-1 suggests, there is a unique combination of $e_I(1)$ and $e_{II}(1)$ that is a best reply to itself when $\beta^*(1-p)/p(1-\beta^*) < 1$. If this condition is not satisfied, or if, equivalently, $\beta^* \geq p$, then, as will be seen below, there is no sequential crisis equilibrium. Accordingly, $e^*_I(1) = \hat{e}_{II}$ and $e^*_{II}(1) = \beta^*(1-p)/p(1-\beta^*)$ in a sequential crisis equilibrium. (Note also that because $e^*_{II}(1) > 0$, $e^*_{II}(1)$ satisfies the condition assumed above in the first observation.)

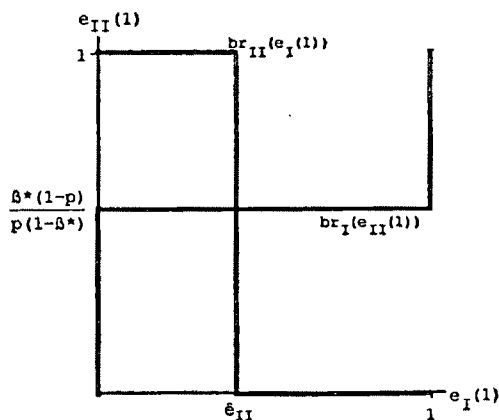
The only strategy left to be specified is $e^*_I(0)$. But by the definition of a crisis equilibrium, $e^*_I(0) > 0$. If a sequential crisis equilibrium exists, the equilibrium strategies are as described in Proposition 2.

It remains only to be shown that p may be chosen to ensure the existence of this equilibrium. From the construction of the equilibrium strategies, a sequential crisis equilibrium will exist if $e^*_I(0) > 0$. This inequality will be satisfied if

$$q_I \leq p\{[1 - e^*_{II}(1)]w_I + e^*_{II}(1)[fd_I + (1-f)s_I]\} + \{(1-p)[fd_I + (1-f)s_I]\}$$

Substituting the expression for $e^*_{II}(1)$

Figure A-1. Best-Reply Correspondences



gives $p \geq 1 - (1 - \beta^*)(w_I - q_I)/(w_I - d_I)[R_I + f(1 - R_I)]$. If $\beta^* \geq p$, this inequality cannot be satisfied, and there is no sequential crisis equilibrium. This justifies the assumption made above that in a sequential crisis equilibrium, $\beta^* < p$. Substituting for R_I shows that the second term on the right side of this inequality is between 0 and 1 for any $w_I > q_I > s_I$ and $1 > \beta^* > 0$. Thus, there exists a p that satisfies this inequality, and this ensures the existence of a sequential crisis equilibrium. QED

Notes

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1. This credibility criterion does not exclude the possibility of setting deterministic or probabilistic tripwires, which some analyses of deterrence (e.g., Brams 1985; Schelling 1960, 1966) have suggested may be used to solve the problem of the inherent irrationality of retaliating massively in response to a limited provocation. Tripwires solve this problem by eliminating the decision of whether or not to retaliate massively. That is, the supreme national authorities are no longer confronted with this deci-

sion because the tripwire makes it. Thus, the question of this decision's rationality is moot. The credibility criterion requires only that the decision to set a tripwire be rational when it is set, not that the tripwire, once set, act "rationally."

2. This term will be defined formally below.

3. Zagare (1985) argues that the preference pattern implicit in MAD is that of a prisoner's dilemma and not of chicken. Although this may be so of some strategies that come under the rubric of MAD, the preference pattern of prisoner's dilemma is not the pattern in the strategy-that-leaves-something-to-chance, for nothing in this strategy is worse than suffering the sanction.

4. Of course, the game's mixed equilibrium is sensitive to the states' payoffs. Indeed, in this equilibrium, I stands firm with probability r_{II} , and II stands firm with probability r_I where r_I and r_{II} are computed according to the formula Jervis uses.

5. See Kreps and Wilson 1982 for a complete definition of a sequential equilibrium.

6. Brams (1985) concludes that because it is never rational to launch a massive attack deliberately in response to a limited provocation, MAD "cannot lead to what is called a '[subgame-] perfect equilibrium'" (1985, 35). This is true of his models. If it were also true of the models developed in this paper, then, because all sequential equilibria are also subgame perfect, there would be no sequential equilibria in these models. As will be seen, Brams's conclusion does not apply to the models in this essay because these models explicitly include autonomous risk whereas Brams's models do not.

7. In fact, the manipulation of the states' critical risks is at the center of Snyder and Diesing's critical-risk model (1977, 48-52).

8. If $2N_I f = R_I$, then I no longer has a unique best reply at $\Theta_I(N_I)$. I may mix at this node, and this generates infinitely many sequential equilibria.

9. Letting $N_I = 1$, $N_{II} = 1$, and $N_{II'} = 2$ is the smallest and simplest combination of N_I , N_{II} , and $N_{II'}$ that satisfies the relations $N_{II} \leq N_I < N_{II'}$ where II' is the potential adversary with the greatest resolve. If N_I , N_{II} , and $N_{II'}$ satisfy these relations, I will prevail against one type of adversary with complete information and lose against the other type with complete information. Moreover, values satisfying these relations are the only interesting cases to consider in this game. An easy generalization of the proof of Proposition 1 shows that if $N_{II} \leq N_I$ and $N_{II'} < N_I$, then I knows that its resolve is greater than its potential adversary, regardless of type, and I will prevail. If $N_I < N_{II}$ and $N_I < N_{II'}$, then I knows that its resolve is less than its adversary and will never challenge.

10. This conclusion must be interpreted carefully. It does not say that if I takes steps that increase its resolve, then II will be less likely to escalate. To say this, these steps would have to be included as options in a new game and the new equilibria

would have to be found. The conclusion only says that if two games of the type in Figure 3 were compared, II would be less likely to escalate in the game in which I 's resolve is greater.

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LEEING THE IRON CAGE: POLITICS AND CULTURE IN THE THOUGHT OF MAX WEBER

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The problem of politics and culture emerged in European thought from Kierkegaard to Freud the encounter with modernity. In this paper I examine a major instance of that encounter in Weber's "science of culture" and his analysis of the cultural significance of capitalism. In Weber's work the most important and politically relevant responses to modern, subjectivist culture lie in attempts from within the ethical, aesthetic, erotic, and intellectualist life orders or value spheres to escape from the "iron cage" constructed by Western rationalism. I investigate the relative autonomy and paradoxical nature of these different attempts, and conclude with an explanation of Weber's choices with respect to the sphere of knowledge, or "science."

The theme of this essay is extensive and monumental, and it can be summed up in the question, what is the relationship between politics and culture? In its most modern statement, this question should be understood to refer to both the use of cultural forms of expression for political purposes and the political criticism of modern cultural forms. Briefly, I want to show that the relationship between politics and culture is defined by critical and adaptive responses to Western rationalism or, in the terms of my essay, by the search for alternatives to the "iron cage." I shall argue that these alternatives disclose specific, politically relevant "solutions" to the discontents associated with modern rationalist culture.

I have chosen to define the range and shape of the encounter between politics and culture through an investigation of Max Weber's thinking in the context of his intellectual milieu, for the relationship I want to discuss is essentially an expres-

sion of the stresses and strivings of a modern, post-Nietzschean experience that Weber shared, observed, and criticized. My emphasis will be upon interpretation. By placing Weber at the center of this inquiry I want to arrive at a new interpretation of a problem underlying his work, directed as it is toward understanding what he variously called "the fate imposed upon us" or "the fate of our times" (1897, 108; 1968, 612; 1946, 155), and I also want to contribute to reflection upon the larger theme that is so integrally connected with the meaning and consequences of modernity.

Statement of the Problem

There may be countless ways of approaching the relationship between politics and culture in order to make it intelligible, but in the present context two appear especially fruitful. In the first place, the critique of emergent modern

culture in its various guises (including the materialist culture of capitalism) becomes a dominant *general* theme of European thought that assumes different forms: a political revolt against the "modern"; a break with the liberal historicist outlook and moral order; a flight from politics into a "sphere" of culture, such as the aesthetic or the erotic, and sometimes back again to a "new" politics; an exercise in transcending the historically given via utopia; or a reflection upon humanity's "technique for fending off suffering" (Freud 1961, 26), for using "life *against* life" (Nietzsche 1967, 120). Regardless of its particular statement, the central problem appears the same in every case: a sense that unified experience lies beyond the grasp of the modern self and that malaise and guilt have become inextricably intertwined with "culture." It is within this general problem and movement of thought from Kierkegaard and Marx, through Nietzsche, Tolstoy, and Freud, to contemporaries like Lyotard (1984) and Habermas (1985; see Bernstein 1985) that Weber's reflections are to be located.

There is a second, *specific* setting for the problem, found in the writings and activity of Weber's most perspicacious colleagues: Simmel, Sombart, Tönnies, Lukács, Michels, and Rickert. Among these figures, surely Simmel develops the key ideas, the analysis of a specifically urban, aesthetic, ahistorical, and amoral *Gefühlskultur* or "culture of feeling," out of which grew a special sensitivity to psychic states, emotional nuance, and the public power of "personality" and personal style. In Simmel's conception, "the essence of the modern is above all psychologism," that is, "the experiencing and interpretation of the world according to the reactions of our inner selves . . . the dissolution of stable contents in the fluid element of the soul" (1983, 152). The question for him then became, What forms of expression would follow from

this "inward turn" in which, since Nietzsche, "life itself can become the purpose of life" (1907, 5)? At the first meeting of the German Sociological Association, Weber took up precisely this question: because the self encounters "all sorts of apparently inexhaustible possibilities for the conduct of life," culture now emerges as either "protest" or "adaptation," as an attempt to find "specific means of escape from this reality" through "the highest aesthetic abstractions, deepest dream-states, or most intensive kinds of intoxication" or as an effort to invent "defenses against this reality's fantastic, intoxicating rhythms" (1911, 98). Psychologism ushers in the reign of subjectivity and "interiority" with all their hidden and explosive potentialities.

The range of possibilities for Weber and his contemporaries can thus be most succinctly conceptualized, in line with Schorske's analysis of Freud's Vienna (1981), as established by the revolt of a new aestheticism against the liberal historicist culture of "progress." It would be mistaken, of course, to suggest that *every* aspect of this revolt was reflected in Weber's writings. But the theme that emerges from the cultural crisis does in fact bring us into Weber's world, even to the heart of his most famous Viennese declarations in 1909, of which the "modernist" content is unmistakable:

We know of no scientifically demonstrable ideals. To be sure, our labors are now rendered more difficult, since we must create our ideals from within our chests in the very age of subjectivist culture. But we must not and cannot promise a fool's paradise and an easy street, neither in the here and now nor in the beyond, neither in thought nor in action, and it is the stigma of our human dignity that the peace of our souls cannot be as great as the peace of one who dreams of such a paradise. (1910, 585)

Because Weber had just seen a performance of *Faust* in the *Burgtheater*, one is tempted to read his soliloquy as a *fin de siècle* reflection on Goethe's creation, one

that points toward the intensified struggle within ourselves (our "souls") occasioned by the rootless "age of subjectivist culture." The most serious modern motifs are accentuated: self-conscious creation of meaning, the psychological burdens of consciousness, the explosion of subjectivity, our feeble efforts to choose among competing "gods" under conditions in which the "will to truth" has become a *problem*. But this is admittedly a dense passage whose meaning will become clearer as the discussion unfolds. It illustrates the leading question emerging from an encounter with modern culture: what stance is one prepared to take and defend vis-à-vis the new aesthetics of a culture of "sensation" and "experience"? It is this question that leads to the deeper levels of Weber's work.

Weber's "Science of Culture"

Weber was, to be sure, many things to many different people: political economist, legal scholar, sociologist, historian, student of the world religions, organizer, politician, journalist, a man who "loved science as passionately as a young bride" (Michels 1927, 109). But the most consistently applied identification Weber used for himself from among these possibilities was political economist or *Nationalökonom* (e.g., 1968, 582; 1946, 129). As a scholar of political economy concerned with the subject matter of the social sciences generally, Weber chose as his starting point the view that the fields of inquiry within such a domain were "sciences" in two closely related senses: they were (to use his terminology) *Wirklichkeitswissenschaften*, "sciences of actuality" and *Kulturwissenschaften*, "sciences of culture."

Undoubtedly the best-known single comment to this effect appears in the 1904 "Objectivity" essay as a statement of editorial policy for the *Archiv*. Unfor-

tunately, Weber's original is distorted in the standard translation, which ought to read:

The social science that *we* want to promote is a *science of actuality*. We want to understand the surrounding reality of life in which we are placed *in its uniqueness*—that is, on the one hand, the relationship and cultural *significance* of its individual appearance in its present configuration and, on the other hand, the basis of its being historically so and not having become otherwise. (1968, 170-71; cf. 1949, 72) (my translation)

In this statement, whose subject is the "actuality" of life, Weber wanted to suggest that social science sought to comprehend the "uniqueness" of the historically given world in terms of (1) contemporary *cultural* meanings and (2) *historical* reasons that could be advanced for the presence of just this cultural "reality" and not some other. The movement in thought in these sentences is absolutely crucial to understanding Weber's reasoning: science of the "actual," the "uniqueness" of a form of life, cultural meaning, and historical explanation are all necessarily connected.

That the conception of a science of the "actually existing" could not be separated from an attempt to establish specific cultural content was underlined even more emphatically in the little-known "preface" also written by Weber upon assumption of coeditorship of the *Archiv*. Alluding to the distinctive contribution of the journal from 1888 to 1904 under its founding editor, the socialist Heinrich Braun, Weber noted,

the step which distinguished the "Archiv" from its predecessors was that it placed the problems identified under the heading "the question of labor" [*Arbeiterfrage*] in the most general context, that it grasped the "question of labor" in its *cultural significance* as the outwardly most clearly visible expression of a much larger complex of phenomena: the fundamental process of transformation experienced by our economic life and thus by our cultural existence as a whole through the advance of capitalism. (1904, 1-2) (my translation)

And the "new direction" announced by Weber and his colleagues, starting from the ubiquitous labor question, was even more firmly anchored in the cultural problem:

Above all today the research domain of the "Archiv" must be fundamentally expanded. . . . Our journal will have to consider historical and theoretical knowledge of the *general cultural significance of capitalist development* as the scientific problem whose understanding it serves. (1904, 5) (my translation)

One cannot imagine a more concise and unambiguous statement of the specific direction charted by this new science of the "actual" as a "science of culture." The posing of familiar questions in relation to "cultural significance" and "cultural existence" gave this form of knowledge its basic claim to preeminence.

If the science of society was conceived as a *cultural science*, then the most urgent question became, What could one expect to find in its *particular* contents? Stated somewhat differently, What was the "uniqueness" of Weber's own culture? What was the central problem he considered worth investigating?

The Uniqueness of Modern Culture: Capitalism

The answer to these questions that is closest to the inner dynamic of Weber's thought commences from the "fact" of modern capitalism, and it moves toward an interpretation of what I shall call the *cultural discontents of modernity*. This unfolding pattern of interpretation and argumentation takes numerous forms and directions, only the most important of which can be considered here. In every case, however, as Weber says, "it is a question of the specific and peculiar 'rationalism' of Western culture" (1920, 11; 1958a, 26) and the responses to it. Paradoxically, this "rationalism" is emphatically not necessarily "rational" but is

persistently called into question by variations on the formula, What is rational is not actual, and what is actual is irrational (with apologies to Hegel).

Weber's first analysis of the "general cultural significance of capitalist development" is found, like numerous aspects of his thought, in embryonic form in the 1890s studies of the East Elbian territories' agrarian "organization of labor relations" (*Arbeitsverfassung*). In the context of the problem of culture, what is important to notice about these studies is their invocation of a traditionalist world, the "unmodern patriarchal organization of labor" (1984, 895), collapsing under the onslaught of new capitalist productive forces; they tell the story of sociocultural, and *therefore* political, dislocation. The process of capitalist rationalization not only puts an end to the historically articulated relations of social stratification in the East; it also creates a new propertyless class of "formally free" labor *and* dissolves the centers of authority that had previously served as carriers of "culture." A change of such magnitude will reverberate throughout the social and political order, Weber maintains. But his conclusion in the early studies of the agrarian world leaves undefined the precise character of the culture that might emerge through this irreversible process.

That "definition" of the cultural significance of capitalism appears as the key-stone to Weber's developing argument most clearly and powerfully in the essays of 1905-6, that is, in the second part of "The Protestant Ethic and the Spirit of Capitalism" and in the essays on Russia. The central passages are well known, although Weber's meaning is obscured by Parsons's translation (which I have therefore altered):

The Puritan *wanted* to work in a vocation [*wollte Berufsmensch sein*]; we *must* do so. For when asceticism was carried out of monastic cells into vocational life and began to dominate inner-worldly morality, it helped to build the tremen-

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dous cosmos of the modern economic order. This order is now bound to the technical and economic presuppositions of mechanical, machine-like production, which today determines with irresistible force the life-style [*Lebensstil*] of all individuals born into this mechanism, not only those directly engaged in economic enterprise, and perhaps will determine it until the last ton of fossil coal is burned. In Baxter's view the care for external goods should only lie on the shoulders of the saint like "a light cloak, which can be thrown aside at any moment." But fate [*Verhängnis*] decreed that the cloak should become an iron cage. (1920, 203; 1958a, 181) (my translation)

The infamous "iron cage," perhaps Weber's most telling Epimethean metaphor, is formed out of the objectification of material culture and its "inexorable power" but also out of the projection of asceticism into the external world of production, labor, and above all, "style of life." For the modern individual, action and existence are regimented by the necessities of *vocational* activity, now stripped of its sustaining structures of meaning. We are all, as it were, conscripted as unconsenting participants in a universalized vocational culture, our horizons limited to the rationalized, inwardly meaningless certainties of "vocational humanity" (*Berufsmenschentum*). For one to attempt, as a "cultural being" (*Kultur-mensch*), to "advance" this culture from the standpoint of its own assumptions, is only to advance farther along the path of modern discontents. For Weber, as for Nietzsche, asceticism is not only our "fate" but our "fatality."¹ We are left with the demand of calling forth our own ideals from within ourselves, of transvaluing our values. Anything else is weakness.

It is this problem of the modern orientation toward life, so characteristic of Weber's interpretation of the deepest meaning of capitalism for culture, that also informs the essays on Russia. For historical and cultural reasons, the Russian case reveals in particularly strong form the clash occurring everywhere between

capitalist rationalism as an "economic inevitability" and the cultural values firmly rooted in a premodern Western tradition—the values of a "democratic" and "individualistic" character—which, in Weber's conception, become increasingly difficult to realize.

Those who live in constant anxiety lest there might in the future be *too much* "democracy" and "individualism" in the world, and too little "authority," "aristocracy" and "respect for office" or the like, may take heart; all too much care has been taken to make sure that the trees of democratic individualism do not grow to the skies. . . . It is utterly ridiculous to suppose that it is an "inevitable" feature of our economic development under present-day advanced capitalism, as it has now been imported into Russia and as it exists in America, that it should have an elective affinity with "democracy" or indeed with "freedom" (in *any* sense of that word), when the only question to be asked is: how are all these things, in general and in the long term, *possible* where it prevails? (1906, 347; 1978, 282)

In Weber's view no one can be exempted from this final question. "Everywhere," he wrote, "the cage for the new serfdom is ready" (1906, 347; 1978, 281) and every "developmental tendency" points to its perfection and permanence. But can the question Weber poses—the most political expression of the cultural dilemma—in principle be answered? For the moment let us say only that for Weber an adequate and, above all, honest answer requires in his phrase "swimming against the stream of material developments." It can have nothing to do with obediently following the course determined by the inexorable flow of "historical laws."

Weber's analysis of capitalism reaches its apogee in invoking the imagery of the "iron cage." But as a cultural analysis, a search for the significance of a unique process, it does not end there. Modernity is characterized not just by a kind of petrification and homogenization of external conditions of life but in addition by inescapable conflict among the contents of different value spheres, life orders, and

powers—each with its autonomous and internally coherent “lawfulness” (*Eigen-gesetzlichkeit*)—that recur at critical junctures throughout Weber’s work. Not only are different value spheres, such as the political and the ethical, *not* identical, but it is also the case that *within* a sphere of value a system of uniform rules *cannot* be found that will “solve,” once and for all, the problems of action and choice. Instead, “ultimately everywhere and always it is really a question not only of alternatives between values but of an irreconcilable death-struggle like that between ‘god’ and the ‘devil.’ Between these there are no relativizations or compromises” (1968, 507; 1949, 17–18). If the first theme—the cultural significance of capitalist development—is intertwined with Marx’s thinking, then this second one—the “struggle among the gods”—is woven from materials discoverable in Nietzsche. If the implications of the first line of thought are external and political, then the implications of the second are internal and ethical; the latter touch the conduct of life itself.

It is interesting to note, furthermore, that what can be said about “ethics” can also be said about “modern culture”; that is, it, too, is not exempted from Weber’s line of reasoning but rather becomes only a part of the “deadly struggle.” This is why, according to Weber, “culture” is for the person who derives life meaning from it alone, who treats it as a “vocation,” never in actuality a source of personal “salvation.” It must instead be associated with “guilt” or, in the specific case of aesthetic culture, with “the persistent presence of conscience in the temple of Narcissus” (Schorske 1981, 10).

The cultural significance of capitalist development thus finds final expression in the great tensions and conflicts—in a word, the *discontents*—of modern life. Having perceived this outcome, Weber appears on balance committed to a two-fold response: on the one hand, he is pre-

pared to consider certain attempts from *inside* modern culture to overcome the problem posed by that culture, although for Weber it is undeniably true that all these attempts are paradoxical and questionable, whether they are judged to be “authentic,” or not. But on the other hand, Weber offers a path toward meaning for the individual who stands face-to-face with such a culture, arguing in the last analysis “that every single important action and finally life as a whole . . . signifies a chain of ultimate decisions, through which the soul, as in Plato, *chooses* its own fate—that is, the meaning of its doing and being” (1968, 507–8; 1949, 18). This disguised reference to the *Phaedrus* demarcates a realm of existential choice, of Weberian authenticity. But the meaning and combination of both responses—the searches for alternatives to the “iron cage” or for “the demon who holds the fibers” of one’s life (1968, 613; 1946, 156)—are far from self-evident. How can they be understood in relation to the problem of culture that has now emerged out of the analysis of capitalist rationalism?

Culture and Its Discontents

The answer to this question is found at the very center of Weber’s thinking in the “Zwischenbetrachtung,” or as it is commonly known, “Religious Rejections of the World and Their Directions.” In theme and problem the leap from the essays of 1905–6 to this “Intermediate Reflection” of 1915, appended to the first volume of the *Sociology of Religion*, is deceptively short, for it is in the latter that Weber begins to confront the most serious responses to the threatening constraints of the “iron cage.” According to Weber (1920, 237) and to Lukács’s testimony (1982, 362), at least portions of the text were read to Weber’s Heidelberg circle before World War I—probably, therefore, in 1913—partly in reply to Lukács’s “eschatological hopes” for “salvation

from the world" through creation of a new "socialist society founded upon an ethic of brotherliness" (Marianne Weber 1926, 474; 1975, 466). This context announces Weber's principal intention in the "Reflection," namely, to provide a commentary on the relentless struggles waged among different life orders and value spheres in their attempts to cope with the historically given "world" through adaptation, rationalization, manipulation, and escape. Although other aspects of the text are important, they are subordinate to the cultural theme—the great tensions among the various orders of life in confronting "the fate of our times"—that builds the groundwork for Weber's entire inquiry.

The "Reflection" contains not only the basic elements of a comprehensive analysis—parts of which surface elsewhere in Weber's essays—of the "internal and lawful autonomy" of the familial, religious, economic, political, aesthetic, erotic, and intellectual (scientific) life orders or spheres of life activity and value, but it also contains the rudiments of Weber's own evaluation of each in relation to the problem of culture, modern culture in particular. In Weber's terminology, so-called religious rejections of the world are characterized in terms of an "ethic of brotherliness" (*Brüderlichkeitsethik*), which is a species of an absolutist ethic of ultimate ends or convictions (*Gesinnungsethik*). Although the ambiguous category *ethics* cannot in itself be a sphere of value with its own "lawful autonomy," Weber's entire treatment of the religious sphere of action and valuation can be interpreted as suggesting that there are distinctively "absolutist ethical" paths, sharing an affinity with the ascetic religious life yet (as with syndicalism) political in their consequences, that some choose to follow as a way of counteracting the dilemmas of worldly action.

While the sphere of absolutist ethics is thus fundamental, it is far from exhaus-

tive; there are alternative responses to the "iron cage" as well, especially in the aesthetic, erotic, and intellectualist value spheres. The first two, which above all others Weber treats "developmentally," tend increasingly, as they become more differentiated, to be absorbed into the escapist "culture of feeling" of pure experience and "inwardness." Once this has occurred, the intellectualist culture so characteristic of modernity then tends to *substitute* one sphere of value for another, one life order for another, based upon the principle that the most subjective judgment is the most authentic and "valid." Citing Weber's example, "the refusal of modern men to assume responsibility for moral judgments tends to transform judgments of moral intent into judgments of taste ('in poor taste' instead of 'reprehensible')." The consequence of such a shift is to reinforce the transcendent claims of subjectivity, for "the inaccessibility of appeal from aesthetic judgment *excludes discussion*" (1920, 555; 1946, 342; italics added). That is its function. One thereby arrives at the complete victory of aesthetic culture in the age of intellectualization.

What of the economic and political life orders, the most soulless and diabolical of powers? In this schema they may be said to represent the paradigm cases for adaptation, the conditions of action given on the one hand by the abstract rationalism of the money economy and on the other by the "inescapable pragma of violence" (1920, 549; 1946, 336). The economic sphere merely recapitulates the problem of capitalism's cultural significance: in a phrase, "Money is the most abstract and 'impersonal' element that exists in human life" (1920, 544; 1946, 331) and is thus for Weber, as for Georg Simmel in *The Philosophy of Money* ([1900], 1978), the source par excellence of all objectifications, human self-alienation, material gratification, and plain misery. If there is meaning to the acquisitive quest, then it is

found simply in analogies with games and sport. Not so the political sphere, Weber's "old secret love" (Baumgarten 1964, 671), where matters are more serious both because of the threat of violence and because of the contradictory invasion by moral purposes and sublimated eros—the action of those who, in Weber's paraphrase of Machiavelli, love their native city more than the salvation of their own souls (1958b, 546; 1946, 126). The modern rebel, repelled by the depersonalized orders of the bureaucratic state, can not only flee *from* but also *into* politics. He will never find salvation there, but he may find a close second, "a pathos and a sentiment of community" (1920, 548; 1946, 335).

Given this interpretation of the life orders and value spheres and the *directions* taken by action within them, let us consider the specific avenues that open up in Weber's reflections on the uniqueness of his own modern culture, which is still ours to comprehend.

The Ethical Alternative

The most important first-order problems are those appearing on the large and differentiated terrain of ethics, serving under the heading *ethic of brotherliness* as the nodal point against which all spheres of value are placed in tension and from which, to a great extent, the various orders of life derive their meaning. The issue of "ethicism" had in fact occupied Weber on many occasions, from early critical comments on "ethical culture" to the later disputes over value judgments. Keeping in mind his Vienna statement from 1909 concerning the absence of "scientifically demonstrable [ethical] ideals," one can sharpen the issue further by quoting Weber's challenge to Michels on the same theme:

There are two possibilities: either (1) "my kingdom is not of this world" (Tolstoy, or syndicalism thought to its conclusion, which is nothing

more than the sentence "the goal means nothing to me, the movement everything" translated into a revolutionary-ethical, personal statement . . . or (2) affirmation of culture (that is, objective culture, expressing itself in technical and other "achievements") through *adaptation* to the sociological conditions of all "technique" [Technik], whether it be economic, political, or whatever else. (Such adaptation is embodied *most often* precisely in "collectivist societies.") In the *second* case all talk of "revolution" is a farce, *any* thought of replacing the "domination of man over man" by *any* kind of "socialist" society or ingeniously devised forms of "democracy" is a *utopia*. In this respect your own critique surely doesn't go far enough. Whoever wants to live as a "modern man," if only in the sense that he has his newspaper, railroads, trams, etc. every day, *renounces* all those ideals which hover darkly around you as soon as he completely *abandons* the revolutionary standpoint *for its own sake*, without any "goal," indeed without the *conceivability* of a "goal." You are a thoroughly honest fellow and will on your own . . . complete the critique that has long since brought me to this way of thinking and *thereby* stamped me as a "bourgeois" politician, *so long as* the least that one as such can want doesn't get pushed into the limitless distance.²

In the entire corpus of Weber's writings there is no more pointed and compact statement of the either/or alternatives pushed to their logical extremes.

It should be noted that the statement of alternatives is presumed valid at two levels. On the first level one must choose, Weber insists, between two fundamentally opposed possibilities for the general conduct of life: rejection of the world, or world abnegation, (*Weltablehnung*) on the one hand and affirmation of the world or of its objective culture (*Kulturbejahung*) on the other. In this context, "objective culture" means for Weber exactly what it meant for Simmel, namely an autonomous order of values external to the self, although produced by it, whose contents presuppose a process of spacial and temporal "distancing" between ourselves and the forms of our activity (Simmel 1978, 59–78, 446–48). By selecting the former possibility of world

abnegation, one must be prepared to live with "fictions" in order to achieve ethical unity of belief; for the latter case one must be prepared to live with "antinomies" or "tensions" in order to achieve clarity about the world as it "is."

On the second level, one must choose between value spheres: either an absolute ethic of brotherliness that promises to end all forms of domination or acceptance of the relative conditions of political conduct. If the former, one must be prepared to live with the maddening incongruities between "ideal" and "real"; if the latter, one must be prepared to live with the diabolic uncertainties of responsibility for consequences of action. Weber insists upon a necessary alignment among these alternatives: there is an "elective affinity," so to speak, between world abnegation and an absolute ethics and between world affirmation and an ethics of (political) responsibility. Efforts to recombine alternatives are the breeding ground for both intellectual confusion and practical catastrophes.

Ignoring for the moment Weber's deceptive and ironic "bourgeois" affirmation of modern culture, we can readily use his statement to delineate the most formidable world-abnegating ethical alternatives: pacificism (Tolstoy); revolutionary syndicalism, or an ethically oriented commitment to "pure democracy" (the position Michels toyed with); and a kind of "socialist" credo affirming the possible elimination of all relations of domination (the early Lukács). There was another concrete variant of ethicism as well, visible in some versions of radical feminism, as Marianne Weber acknowledged in several of her essays and in an exchange with Simmel on the possibility of an autonomous "female culture" (Simmel 1983, 207-41; Marianne Weber 1919, 95-133). For if feminism could be conceived as an alternative to the constraints of everyday life organized in vocational work, then it could also be thought to

foreshadow a qualitatively different world: "Perhaps womankind will succeed better" than have men "in unifying the dissonances between knowledge and action," thus keeping humanity from ending "in torpidity and sheer vocational specialization" (Marianne Weber 1919, 9). Max Weber left this fourth possibility to his wife's and Simmel's diligence, but he did, as promised, grapple with the first three as "directions of world abnegation" or as ideological ethics of "ultimate ends" or convictions.

Absolute internal consistency was the *sine qua non* for these ethical positions, according to Weber, and it required an unflinching determination to suppress any "compromises" with the realities of the existing world. All consistent ethical alternatives shared some postulate of absolute justice—in effect, *fiat justitia, pereat mundus* ("Let justice rule, and the world perish")—at the level of both collective and individual ethics. They also shared complete indifference to rational "ends," and to any rational calculation of the consequences of an act. Demonstration of the purity of moral conviction, motive, or intention qualified as the exclusive criterion of justification.

But what does all of this have to do with the cultural problematic of capitalist development? For Weber and his contemporaries, capitalist development was bound up with "technical progress," with the working out of purposive-rational or instrumental orientations toward action, with the standardization and routinization of life activity, and with control of the conditions of existence through calculation. Compared to what Tolstoy (and following him, Weber) conceived as the "organic cycle of life," these forces created an endlessly "progressive" life of specialization, dislocation, and fragmentation. Every "ethical" school of thought sought, in opposition, to recreate the world as an "organic and natural whole" (Tolstoy), a "totality of being" (Lukács), or an ethical-

ly "rational" unity (Michels). The spirit of these views is perfectly captured in Lukács's later observation that his pre-1917 position as summarized in *The Theory of the Novel* expressed "the hope that a natural life worthy of man can spring from the disintegration of capitalism and the destruction, seen as identical with that disintegration, of the lifeless and life-denying social and economic categories" (1971, 20). Not only must the capitalist productive system be destroyed, but so must its modes of language, thought, and being. Lukács was at least sufficiently honest to see in such views "a highly naïve and totally unfounded utopianism."

The utopianism of a rigorous and consistent ethical alternative certainly accounted for one of Weber's objections (though perhaps the least persuasive) to ethicism. After all, a "science of actuality" was already in conception a science of the historically given world, not apart from it. In addition, from the standpoint of the *political* sphere, utopianism meant "irresponsibility" or the complete repudiation of Weber's preferred "ethic of responsibility." On the other side of the issue, Weber could still honor the dignity of an absolutely consistent *Gesinnungsethiker*—a source, no doubt, of his interest in Tolstoy or in Lukács's apocalyptic and Michels's syndicalist convictions. But the brunt of Weber's rejoinder to ethicism lay elsewhere, in a remarkable critique, filtered through Tolstoy, of the *meaning* of modern culture under conditions of "technical progress." The essence of Weber's reflection on Tolstoy's protest was to show, first, that the search for meaning in "technical progress" and everything associated with it, engaged in by the modern "cultural being," could never, from the ethical standpoint, be successful: culture, "necessarily burdened with guilt," would "become ever more meaningless" (1920, 571; 1946, 357). However, Weber also wanted to show,

secondly, that the alternative construction of "meaning by the ethical escape route would just as surely prove to be an illusion; it would, in his words, "succumb in the end to the world dominion of unbrotherliness."

Why is this the case? The most interesting reasons are found toward the conclusion of the "Reflection" and "Science as a Vocation" (see esp. 1920, 569-71; 1968, 594-95, 609-13). A comprehensive ethic for the totality of life conduct presupposes a genuine prophecy, which, Weber insists "is simply not there" and cannot be given "the technical and social conditions of rational culture" "for purely external reasons." (It is well known in any case that Weber rejected all forms of modern prophecy, whether stemming from Marx, Nietzsche, Tolstoy, Wagner, or even Sombart.) Furthermore, in this culture, ethicism has economic presuppositions: only among those privileged to exist outside the vocational world—to remain economically independent as rentiers or otherwise—can it become a comprehensive and consistent life order, a clear allusion to Tolstoy and to some members of the George circle (notably Stefan George himself). Finally, in so far as it aims for universality, the ethical alternative must founder on a contradiction: in Weber's words it is "not accessible to everybody" and therefore "definitely means aristocracy" (1920, 571; 1946, 347). Ethicism then faces the unenviable and impossible task of achieving the new order of equality (i.e., eliminating the "domination of man over man") through the inequality of aristocratic domination. Whatever form it takes, the ethical alternative seems to run afoul of a kind of "reality principle" embedded in capitalist development. It represents a possible choice, available to a few, capable of renewal from generation to generation, but always at the mercy of the unrelenting pressure of the "fateful" forces of culture continually calling it into question.

Socialism as Cultural Criticism

The "socialist" alternative was intriguing to Weber because of its mixture of contradictory elements: organizational imperatives, allegedly "scientific" principles, prophetic vision, and above all, criticism and reconstruction of modern culture. Given these varied dimensions, from Weber's standpoint the modern proletariat's "most magnificent" task was "its attempt to represent itself as a cultural community," to develop a political movement that "fostered the rapturous hope of opposing from within itself the bourgeois world with completely new values in *all* areas" of culture (Weber 1911, 97-98). Robert Michels, Weber's close friend and junior colleague, had of course emphasized the undermining of democratic purpose by organizational necessities within socialism, partly in reply to Weber's persistent questioning. But when Weber recruited Michels to write an article on "the international socialist movement" for the *Outline of Social Economics*, it was with the understanding that he instead give priority to the problem of culture: "I assume that you will analyze . . . Social Democracy or the socialist movement above all as a party of culture [*Kulturpartei*]. The movement certainly wanted to create and *believed it had* created not only its own *social* outcomes, but also its own *cultural* content. What content? From which *ultimate* ethical or other standpoints? It *wanted* a substitute for *religion*, even to be a religion itself. In what *sense*? Is this still the case?"³ Despite his critical inspiration, Michels remained too much the participant to follow these questions to their conclusion.

Clearly for Weber, however, not merely party programs and organizational forms but the deeper "spirit of socialism" mattered most in what he called "a universal history of culture" (1920, 10; 1958a, 23). In effect, his proposal called for nothing less than a study of socialism

parallel to his own earlier cultural investigations of the "spirit of capitalism." Were socialism in general to lose its peculiar "spirit," conjured by the sorcery of capitalism's revolutionizing of actually existing culture, then it would likely also lose its uniquely attractive and critical powers.

What might have gone into a study of this kind remains open to speculation. However, in those scattered passages where Weber addresses his own questions a clear line of argument emerges: as a movement of cultural criticism, socialism is both a specific product of the forces that have produced modern culture and a wishful negation of those forces. As it moves seriously to implement its critique, it undercuts the conditions for its own existence and appeal. But this formal paradox, although interesting, is less significant than socialism's actual substantive rationalizations. For to the extent it adapts to "technical progress," *any* style of socialism, regardless of "ethical" overtones, will tend to reify the forces of industrialization unleashed by capitalism into everyday culture. In such experiments, Weber argues, "it is the dictatorship of the official, not that of the worker, that for the present at any rate is on the advance" (1924, 508)—and not only for the present! Combined with "submissive attitudes" and "spiritual apathy," resulting partly from the abortive "eschatological" politics of the movement (1906, 121; 1978, 283), this seems in Weber's eyes a ready-made formula for multiplying the constraints of vocational life. What this socialism has in fact brought forth (which is all to the good) is new subjects and problems for culture. But it has not and will not ever create new cultural *forms*, for to do that would require renouncing the world of "objective culture" that it desires to inherit. As a positive movement, socialism is set against itself, trapped in what Weber (following Sombart) once called its "two-souled

character" (1906, 54). It is at its best as cultural critic.

The Flight into Aestheticism

Weber characterizes the aesthetic and erotic value spheres as "inner-worldly powers of life" whose fundamental essences are "arational or antirational" and which are therefore opposed per se both to the acosmic "ethic of brotherliness" and to the rationalization of the world through intellectualism, technical "progress," or any of the various forces aligned with modern capitalism. Within the region of the aesthetic it is music, the most "inward" form of human expression, that can set itself against moral and scientific culture as a "diabolical" power, as the finest and highest expression of amoral and irrational interiority. Of all the arts, music is capable of immediately awakening an inwardly authentic and plastic expressiveness, not merely in sharp contrast to the dull beat of technical rationalization and the vocational life world but as "a surrogate for primary religious experience" (1920, 556; 1946, 343). However, in Weber's view "art" in general develops in a peculiar relationship to the modern: it "becomes a cosmos of more and more consciously grasped independent values" that "provides a *salvation* from the routines of everyday life, and especially from the increasing pressures of theoretical and practical rationalism" (1920, 555; 1946, 342). With such a breakthrough to a core of unique meaning the aesthetic becomes a distinctive, autonomous sphere of value.

It is instructive to notice that Weber's line of reasoning, with its characteristic juxtapositions and dialectics, is in important ways representative for the entire fin de siècle milieu. Among contemporaries engaged with the aesthetic, Simmel, for example, offers numerous representative commentaries of a similar kind, in which "salvation from the confusion and turmoil

of life" is proposed as "the essential goal of art" (1983, 153). In Weber's, as in Simmel's, thought the aesthetic comes to be endowed with such significance *because* it is grasped by the mind in dialectical opposition to the grinding forces of routinization and rationalization. Translated into the idiom of philosophical idealism, it henceforth stands for the positive "moment" in which the modern "cultural being" is lifted out of the shadowy cage and into the brilliant sphere of "transcendental truth," with the "transcendent" now conceived as a category of aesthetic culture.

Among Weber's contemporaries the intrusion of aesthetic *criteria* into science and politics was a widespread phenomenon, and itself suggests that we are in the presence of the modern. However, aestheticism's attempted conquest of other value spheres, including the political, presents a rather different problem: the appropriation of the aesthetic as a genuine sphere of redemption bearing cosmic significance. In the latter sense for Weber it was without a doubt the meteoric rise of Stefan George and the amazing transformation of his circle that demonstrated the overpowering spell of aestheticism, for in this case the aesthetic of poetry was transformed into a cult, a temple of devotion, with George as prophet, high priest, and savior. Carried beyond the community of devotees, his flight from the world's rationalization became the original model for an entire generation.

What was most noteworthy about George, however, was the combining of aestheticism with the drive toward ascetic world mastery. To Weber's mind, George's poetry showed that he "himself steps out of the aesthetic cloister in order to renew and rule the world from which he had at first fled as an ascetic (with aesthetic rather than with ethical characteristics), following the pattern of so many other ascetics."⁴ But the path to "ecstatic rapture" through deployment of

aesthetic "forms" resulted not in any sort of substantive political message of salvation from objective culture, judged in terms of *content*, but rather in a "purely formal prophecy" with "nothing positive to offer" except the usual platitudes concerning the (formal) need to overcome "rationalism, capitalism, and so forth." The very attempt to substitute form for content then led, in Weber's judgment, to subverting original aesthetic purposes:

One perceives most clearly how strongly this undermining of content affects artistic form, if one hears the poems read in the artistically correct manner by a disciple of the school, that is, in a liturgical monotone that acoustically shakes one up. It is a violent and completely unartistic exertion. Here the work of art is steeped to its last pore in a purpose that lies beyond the work of art—a powerfully ironic comment on the original strict exclusion of all non-artistic purposes.³

Such prophetic ambitions seemed in the end not to derive from the poet's expected sources of inspiration—experience, mystical revelation, myth, memory—but rather from an inner need or "necessity" that was essentially reactive, an agitated counterthrust against the new forces of objective culture. George's aesthetic politics was all movement and motion, as if in imitation of the very world it rejected. Instead of vanquishing the forces of rationalization, this new aestheticism spiraled back within them and became their hapless captive.

Eroticism as Escape

Of all the life forces, eros deserves for Weber to be labeled "the most irrational" and in its sublimated expression as "eroticism" the most capable of pressing to the limits of the extraordinary. The escape from the "everyday" is to be understood in Weber's idiom as an unfolding process of separation from "the organic cycle of peasant existence" that leads finally in the "intellectualist culture" of vocationally organized humanity to a

peculiar characteristic of the modern era: eroticism becomes *the* source of "inner-worldly salvation from the rational" or the *only* power "which still bound humankind to the natural source of all life" (1920, 560; 1946, 346).

The choice of language for designating eroticism as a specific characteristic of the modern is significant for two reasons. First, more so than the discussion of other value spheres, it contains an explicit demonstration of developmental "stages" in the rationalization of *Erotik*, starting from the fiction of an "organic cycle of life" and ending in the paradoxical "eroticism of intellectualism." Second, whether historically permissible or not, the fictional possibility borrowed from Tolstoy's novels becomes logically appropriate as an anchor for Weber's mode of argumentation: it represents "natural" eros without the heightened sensibility and tension of eroticism. Given such a starting point, Weber's line of interpretation then proceeds through a radical opposition between the "cold skeleton hands of rational orders" and deadening "banality of the everyday" on the one side and the "natural," the "real," the "irrational," and the "truly living" on the other (*das Lebendigste*). The latter four-part configuration, a transposition of the "organic," must be fashioned in the crucible of the modern consciousness: eroticism is *experienced* as closest to the real and natural because eros has been reconceived as the ultimate source of "life" or as life's most *irrational* force. Precisely these connections in thinking, which appear so self-evident to modern humankind, already disclose a uniquely modern conception of "life": only in the secret, "inward" sphere of the "irrational," far beyond the banalities of routine existence, can one directly sense life's pulsating forces. In order to assume its fullest meaning, "life" in this world, the only world there is, must be lived "beyond good and evil." Only under such conditions can

its irrational core—eroticism—ever be imagined to offer an avenue of eternal renewal. This view of eros—Weber's view—is of course itself fully dependent upon the fin de siècle experience of desublimation, of release from repressive culture.⁶

Where does the paradox lie in this last embrace of eroticism in "intellectualist culture"? Despite all rationalization under conditions of "technical progress," the "eroticism of intellectualism," in its deadly seriousness, "affirms precisely the natural quality of the sexual sphere again, but consciously, as an embodied creative power" (1920, 561; 1946, 347). The possibility of a conscious affirmation of the natural—defying the material culture that is historically so and not otherwise—is the essence of this most modern of eroticisms. But as Weber well knows, "modern humanity," precisely because of the sources of its identity, has lost all direct knowledge of the "natural" as an immanent "creative power." Our affirming consciousness is a feat of imagination and will, and whatever "meaning" it can acquire is generated as a rebellious reply to nature's negation. The one thing reserved to humanity is self-consciousness of a refined sort, that is, a realm of inwardness, of Simmel's "lived experience." Importantly, residing in this realm is an eroticism Weber can affirm—if only because it expresses what one might call a heroic pathos of *distance*, aware of repetition coupled with lost natural innocence—"again, but consciously."

Weber's attempt to grant some dignity to the life-sustaining but guilt-ridden value sphere of eroticism emerges from the background of his deep reservations concerning the erotic life cult (*Lebenskult*) of the age. From Schwabing to Monte Verità and eventually Heidelberg, the disciples of eros, personified for the Weber circle by the "Freudian" Otto Gross, began to propagate a new *Weltanschauung* of liberation derived from a specific clinical practice. But

instead of the promise of sexual-personal liberation followed by a new community of "brotherliness," Weber foresaw a reign of confusion, disappointment, and self-deception. Any thought, no matter how ingenious, of deducing a personal morality or practical world view from scientific investigation was greeted by Weber with extreme skepticism. In this case, he observed,

the Categorical Imperative, "Go to Freud, or to us his pupils, to learn the historical *truth* about yourself and your actions, or else you are a coward" not only reveals a kind of childish "departmental patriotism" of the psychiatrist and the modern type of professional "*directeur de l'âme*," but also totally debases itself from the ethical point of view because of the fatal admixture of purely "hygienic" motives. (Baumgarten 1964, 647; Weber 1978, 386).

In addition, under the motto *know thyself*, pursuit of the erotic as a value ended not in unbounded human brotherhood but rather in the exact opposite—an exclusive and incommunicable possession of the individual other; ineradicable conflict characterized relations between these two value spheres. No doubt the recommended therapy of freeing oneself from psychic mechanisms of "repression" in order to promote "health" (or moral traits such as "honesty") could be interpreted as a revolt against inner-worldly asceticism *tout court*. But the question was, could the new "freedom," based upon a critique of sexuality and the erotic life, in a positive sense be anything more than an age-old hedonism? Only a fine line might be seen to separate the escape into eroticism from the nihilistic enjoyment of pure sensuality. Eroticism as pleasure seeking led not to higher self-knowledge, but to those "last men who invented happiness," the "sensualists without heart" excoriated in the closing pages of *The Protestant Ethic*. In this form, eroticism as a path of escape from the rational and the everyday ceased to be humanly interesting.

The decisive characteristic of what

Weber called "subjectivist culture," with its ubiquitous "searches" and confused arrogance, was in sum a carefully cultivated, redemptive inwardness. Not only was modernity characterized by technical "achievements" of world-historical significance; it was also characterized by reactive flights into the obscure interior spaces of the modern subject re-presented as ascetic, aesthete, ethicist, and erotic. To chart these psychic spaces in a new soteriology of "inwardness" became for the convinced "cultural being" a calling of the highest order, a dare to live at the "height of the times," which was itself symptomatic of modern discontent and inseparable from it. Modern culture was thus deeply divided against itself: the "objectivist" strain appeared to erode the sources of "subjectivist" inspiration, while the latter lived in a condition of permanent hostility toward all purely purposive-rational *Technik*. Sociologically considered, in Weber's diagnosis the deepest paradox emerged from subjectivist culture's essential dependence on its objectivist counterpart. That is, the blossoming of an inwardness of cultural redemption was scarcely imaginable without the new technologies of publication and communication, the cultural hot-house of the modern city, new possibilities for economically independent urban existence—in short the complete intellectualization of even the most sacred value spheres of subjectivity. For the privileged creators and guardians of "subjectivist culture" this was simply a paradox that could not be resolved.

But what of Weber's own position in relation to this paradoxical endpoint? What is one to make of his cryptic references to choosing one's own fate and to "affirming" modern, objective culture through adaptation to the "conditions" of its existence? Where did Weber's choices lie, and why?

The Requirement for Choice in the Age of Subjectivist Culture

In "Science as a Vocation" Weber denied himself the possibility of giving a comprehensive answer to the Tolstoyan question, How should we live and what should we do?, a question that begged for new prophetic solutions that he considered illusory. But in confronting the critique of culture at the turn of the century, he did seek clarity as to the ways in which such a question might be provisionally answered given the economy of choice posed by the struggles among rival value spheres and life orders. The dynamics of this provisional answer were set by the contrast between the conditions of the "modern" and the cosmos of the "organic." Stating the matter boldly, the latter choice never appeared as an issue, as meaning was prescribed by ethical unities, which were actually projections by modern memory of what might have been had Western culture followed a different course. But for those striving for the fabrication and possession of rationalized knowledge and culture, there was a responsibility to choose in the context of a plurality of value spheres, none of which could be universally preeminent. This Weberian "perspectivism" rested not on metaethical or metaphysical postulates but rather on a claim about the nature of action and experience in modern culture. No doubt it asserted that one *ought* to side with the historical present, with a "disenchanted" world from which there can be no magical escape but from which contingently hopeful futures might emerge. Why this particular choice, however? What reasons lie behind Weber's affirmation? In the last analysis there seem to be three kinds of answers, or if one prefers, three "levels"—psychological, political, and scientific—to a complete answer to this last and most fundamental of all questions.

First of all, Weber frequently enough expressed his *personal* choice for understanding, as he put it, "what is": "I am not an astrologer and not rash enough to attempt to satisfy those craving belief. My most decisive inner need is 'intellectual integrity.' I say 'what is.' " This "decisive inner need"—which appears so strongly accentuated in all of Weber's discussions of "vocation"—from the religious to the scientific and the political, certainly underlies the "pathos of distance" that characterizes his most brilliant work. But it should also be said, simply with respect to the personal conduct of life, that the values Weber objectified for himself and others, particularly the need for "integrity" and "matter-of-factness," strove for the same kind of "distancing" effects. No better illustration of what might be called the "therapeutic" implications of such objectification can be found than in his advice concerning his sister Lily:

What would *objectively* do her some good is the most straightforward acceptance of life and fate in the spirit that life not only wants to be lived as it is, but also again and again *deserves* to become lived, not emotionally but rather in plain sincerity with respect to persons and things, in uncomplicated simplicity with all of its "problematics"—in short, all that which lies in the opposite direction from the modern chase after "experience."

The dualism operative in such a formulation—the stance of an unpretentious directness against the emotionally alluring and exhausting search for genuine experience—contains a plea and a strategy for fending off the demons of "inwardness" and "subjectivism" that must for cultural reasons hover around every modern individual (at least around those who are not, in Weber's words, "spiritually dead") and determine the entire problem of life's purposes. The personal significance of "integrity" then lies in the struggle for the mastery of self within a world in which the manifold of "experience," the testing ground for self-expression, has come to

challenge the very possibility of an "integrated" self. Weber's choice for the world that is historically "as it is" can in this sense represent a way of coping with discontent brought about by fundamental contradiction. Only by repudiating its own presuppositions could Weber's analysis be expected to have a different character.

Beyond the personal, Weber's choice also rested upon reasons having to do with his conception of the political. The affirmation of "objective culture" expressed through "technical achievements" had practical-political consequences: it led, above all, to a political stance (and reputation) widely, if misleadingly, characterized as "bourgeois." But if Weber accepted the historical results of the socio-cultural transformation inaugurated by ascetic Protestantism, it was not because of a secure faith in progress through technological development or belief in the inherent superiority of new forms of economic rationalization and their salutary political and cultural effects. Rather, the essential rationale of his position was found in (1) the choice in favor of a "politics of responsibility" toward the future of history; and (2) the search for a political dynamic adapted to the culture of modernity, an arrangement of powers and orders that would measure up to the irreversible facts of "bureaucratization" of institutions on the one hand and the demand for "democratization" on the other. These twin pillars of argument implied a politics anchored in the historical world of causes and consequences, means and ends, facts and explanations. Weber even radicalized this assessment to the point of seeing in the most plausible self-proclaimed "political" alternatives, such as revolutionary socialism, a mirror image of the present: the call for revolution often meant merely the introduction of state socialism (i.e., the reconstruction of the state as "rational" entrepreneur) and thus a social order still encased in the

constraints of the "iron cage," but now without even the "freedoms" permitted by voluntary association. Those who elected politics as a vocation, regardless of ideological persuasion, were in Weber's idiom condemned to bear the burden of action within the world as it is, informed by a "knowledge of tragedy" (1958b, 535; 1946, 117). All attempts through politics to deny this burden altogether would predictably have precisely the reverse effect and make the state's dominion even more complete, unbearable, and unaccountable.

Finally, faced with the turn-of-the-century cultural problem, Weber sought clarity above all in the diagnostic powers of *scientific* reflection, or in what could be called "the sphere of intellectual knowledge" (1968, 564; 1946, 350), surely one of Weber's most openly expressed passions. Like the political, the aesthetic and the erotic, knowledge or *Intellekt* was conceived in the terminology of the "Reflection" to be a particular value sphere possessing an internal logic and a self-sustaining commitment to "intellectual integrity," which set it in opposition to any "ethic of brotherliness" but whose own presuppositions it was nevertheless prevented from justifying:

Rational knowledge, which has followed its own autonomous and inner-worldly norms . . . has fashioned a cosmos of truths which no longer had anything to do with the systematic postulates of a rational religious ethic. . . . Although science has created this cosmos of natural causality and has seemed unable to answer with certainty the question of its own ultimate presuppositions, in the name of "intellectual integrity" it has come forward with the claim of representing the only possible form of a reasoned view of the world. Thus the intellect, like all cultural values, has also created an unbrotherly aristocracy of the rational possession of culture [*unbrüderliche Aristokratie des rationalen Kulturbesitzes*], one that is independent of all personal ethical qualities of humankind. (1920, 569; 1946, 355) (my translation)

Undoubtedly one of Weber's most passionate choices—the flight into the "unbrotherly aristocracy" of rational scien-

tific culture—represented a specific, permanent, and comprehensive answer to the various "escape routes" of modern discontent. In the words Weber borrows from *Faust*, this "devil is old; grow old to understand him." If the impersonal, disenchanting rationalism of "knowledge" could not possibly solve the Tolstoyan problem of "meaning" while remaining true to its own critical premises, it could nevertheless minimally fulfill the "duty of bringing about self-clarification and a sense of responsibility," helping the willing human agent give "an account of the ultimate meaning of his own conduct" (1968, 608; 1946, 152). On this ground and this ground alone, the option for "science" could be "justified" within the total economy of choice.

Having distinguished among three kinds of reasons for Weber's choice in the "age of subjectivist culture," it is necessary to emphasize in the end the logical consistency and substantive unity among all three. Again and again Weber returns to the same configuration of principles: responsibility, integrity, self-discipline, clarity of vision, the pathos of distance, and objectivity toward self and world. With these ideas we have touched the foundations of Weber's evaluative response to the sensuous, emotive, inward culture of modernity. It does not seem possible to go beyond them to anything more fundamental.

Conclusion

Weber's fully articulated position is open to criticism from other value positions or value spheres, but its strength lies precisely in its having accounted already for these alternatives, their assumptions, and their paradoxes. There are, as it were, no unanticipated surprises left to startle and derail the inquiring mind. Whether we can share Weber's formulation of the issues, his juxtaposition of the antinomies

and ironies of existence, and his choice for personal-political responsibility and knowledge or whether we are attracted instead to the rival claim of an alternative will depend not merely on those "demons" who "hold the fibers of our lives" cited in the closing sentence of "Science as a Vocation" but also on our understanding and judgment of the culture that surrounds us. This is itself preeminently a task for knowledge, whose performance would lead far beyond the scope of this paper.

However, to the extent that we continue to share the larger problem of politics and culture with Weber and his fin de siècle generation, to the extent that modernity and the adaptive maneuvers and protests provoked by it continue to pursue us, we should at least be able to gain in understanding through recovery of an analysis prepared to heed the warning in Goethe's lines,

Day is still upon us,
while we remain active;
But the night approaches,
where none can be productive.

One would like to think that remembrance of what has gone before and might yet lie ahead can provoke a modest step away from the shadows of silence, forgetfulness, and self-deception.

Notes

This paper was written in connection with a seminar taught jointly with Wilhelm Hennis at the University of Freiburg, and it has benefitted greatly from his helpful suggestions.

1. *Verhängnis* carries this crucial double meaning, as in Nietzsche's use of the term in *The Gay Science* (bk. 5, sec. 371) and in the discussion of asceticism as "our fate" or "fatality" in *The Genealogy of Morals* (e.g., "First Essay," sec. 11), a text Weber knew well; see the provocative remarks in Hennis 1986.

2. Weber to Michels, 4 August 1908, Archiv Max Weber 59, Turin (my translation); Weber is replying to Michels's *Archiv* article (1908), the earliest published statement of the thesis developed in *Political Parties*.

3. Weber to Michels, 30 May 1914, Archiv Max Weber 116a, Turin.

4. From a programmatic letter on the George circle to Dora Jellinek, 9 May 1910, reprinted with omissions and errors in Marianne Weber (1926, 465-67; 1975, 457-59); the original typescript is in the Zentrales Staatsarchiv Merseburg, Nachlass Weber [the Weber Papers] 30/6, 6-8; Dora Jellinek, the daughter of Weber's Heidelberg colleague, and not Margarete Susman (as Harry Zohn suggests), is the "gifted woman" who inspired this exchange.

5. Nachlass Weber 30/6, 6-8 (my translation).

6. Most recently H. N. Fügen (1985, 118-22) has interpreted the passages on eroticism containing the most extensive 1919-20 revisions in the "Reflection" as a sublimated expression of Weber's relationship with Else Jaffé-Richthofen—a plausible suggestion that must await publication of Weber's correspondence in the *Max Weber Gesamtausgabe* for fuller discussion.

7. 15 July 1909; Nachlass Weber 30/4, 85-86 (my translation).

8. 1 January 1917, Nachlass Weber 30/8, 6-9 (my translation).

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THE INDIVIDUAL POLITICAL ECONOMY OF FEDERAL TAX POLICY

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Public policies are the product of conflicting demands within society. These demands are both collective in nature and the product of individual interests. Individuals must balance these conflicting interests in developing their policy preferences. We estimate the role of collective and individual interests in preference formation, using tax policy as a substantive example. Survey data are used to estimate the model for evaluations of several tax policies debated during consideration of the 1978 Tax Revenue Act. Our analysis indicates that individual preferences for tax policies are greatly affected by attitudes towards collective issues—particularly redistribution attitudes—and by self-interest considerations. We also find that aggregate variables measuring individuals' local economies have significant effects on symbolic and self-interest preferences.

Public policies are the product of many conflicting demands and interests within society. One source of conflict is disagreement over collective goals, such as how wealth is to be distributed among individuals and generations, how to balance competing economic objectives, how individual rights will be constrained by society to achieve common goals, and even how collective decisions are to be made. It is no surprise that decisions on policies designed to achieve these goals can generate a great deal of conflict. Individuals will disagree about the proper goals of society and about the proper means to achieve those goals.

A second source of conflicts is the result of personal interests and the effect of society's policies upon each person. Public policies are not likely to treat individuals identically. Some individuals

will be advantaged by a policy while others are not, or some will share more of a policy's cost than others.

A model of attitudes towards public policies must reflect both collective and individual interests. Preferences for a particular policy are influenced by three factors: the policy's effect on collective attributes, the value individuals place on the collective attributes, and the policy's personal impacts. If these factors conflict, the individual must decide what weight to give each. People who support a policy's general objectives but oppose its personal impacts must decide which interests are more important, collective or personal, and their willingness to make personal sacrifices to achieve the desired collective objectives.

We begin with the statement that public policies affect both collective and private interests simultaneously. The analysis of

policy preferences first identifies people's collective values and the conflicts among those values. Our first concern, for example, is to estimate the relative values individuals place on such collective values as the aggregate income distribution, the level of environmental quality, or the level of racial integration.

The second part of the analysis identifies the impact of alternative policies on personal interests. This is accomplished by estimating how individual incomes or life styles will be altered by a particular public action. These individual interests are often very difficult to enumerate and measure, as they are determined by a complex set of individual and organizational behaviors. Their assessment, however, is central to any effort at estimating individual preferences. Finally, we assess the relative strength of collective values and private considerations in individual policy evaluations by examining choices in specific situations.

We apply this approach to the study of preferences for federal tax policy using survey data collected during debate on the Tax Revenue Act of 1978. Tax policy is an excellent subject for this analysis for several reasons. First, it is a policy that affects all citizens and on which most people have opinions and some knowledge. Thus, we can be less concerned about nonattitudes producing biased responses to the survey questions, though such concerns are addressed within the model. Second, the individual impacts of alternative tax policies can be easily evaluated by comparing the resulting change in tax burdens. Questions about tax changes can be written so that individuals perceive their personal interests more clearly than with many other survey items. Thus, we should be better able to ascertain the relative influence of collective values and personal interests in forming individual policy preferences.

Our analysis is presented in four sections. One describes the general dimen-

sions of tax policy. Following this, we develop a formal model of individual policy preferences. The third section discusses the estimation of the model through analysis of survey data. Finally, the implications of the model are discussed. Throughout these discussions, our principle focus is the utility of this model as a means of describing and analyzing issue preferences and policy conflicts.

Individual Preferences and Tax Reform

In developing the model of individual preferences for changes in tax policy, we first discuss the public objectives served by tax policy. We then present a discussion of individual interests and how they are related to any tax change.

Public Objectives

Tax policy has four collective objectives: to (1) finance public services, (2) redistribute income, (3) promote economic growth, and (4) maintain economic stability. We present a brief discussion of each objective, including consideration of the conflicts within each objective (see Musgrave 1959, chap. 2 and Musgrave and Musgrave 1976, chap. 8, for discussions of these objectives).

Financing public services. The most obvious objective of tax policy is to raise money to pay for publicly provided goods and services. An important issue is how to determine both the amount of goods and services to be produced and who will pay for them. Individuals differ in the value they place on public services, and thus will differ in their assessment as to whether taxes, in general, are too high or too low. For example, people opposed to additional federal services are more likely to favor tax cuts than people who favor more services (the level of public services

is denoted as G and the value a person places on additional services as U_G^i).

Income redistribution. A second objective of tax policy is to achieve a particular distribution of income within society. Although we have several different ways to redistribute income, the most economically and administratively efficient means is through the tax system. Naturally, an important source of conflict is disagreement over what is the proper distribution of income within society and whether more redistribution is needed (society's income distribution is denoted by R and the value of a more progressive distribution is U_R^i).

Promoting economic growth. Another important objective of tax policy is to promote economic growth and increased productivity, particularly through encouraging capital investment. Taxes are commonly used to discourage current consumption and promote savings by offering deductions for money spent on capital investments or by exempting income from capital investments. However, in recent years, as typified by the Kemp-Roth and Reagan tax packages, tax cuts are used to increase certain individuals' income, with the hope that this additional income will be used for investment rather than private consumption and that lower marginal rates will encourage greater work effort (the country's growth rate is symbolized by r and the value of increasing that growth rate by U_r^i).

Stabilizing the economy. The final public objective of tax policy to be discussed here involves economic stabilization. Changes in tax policy can be used to reduce demand, and thus inflation, or to stimulate demand and boost aggregate output. Debate about the proper stabilization policy and thus the proper tax changes becomes clouded when high inflation rates and falling demand exist

simultaneously, as in the late 1970s, generating a great deal of conflict over whether economic stimulation or restraint is needed (the nation's employment level and inflation rate are denoted by E and p respectively, while U_E^i and U_p^i indicate the values placed on increasing employment and decreasing inflation).

Private Interests

The private interests associated with proposed tax changes are derived from the effect of the tax change on disposable incomes, where disposable income, I_i , is gross income minus tax payments, for example, $I_i = Y_i - t_i(Y_i - D_i) = (1 - t_i)Y_i + t_iD_i$. Personal deductions and average tax rates are denoted by D_i and t_i respectively, and the t_iD_i shows that the value of any deduction depends upon the person's tax rate (for simplicity of exposition, we assume a proportional income tax so that average and marginal tax rates are identical). Changes in disposable income arise in three ways: changes in individual deductions, ΔD_i ; changes in pretax incomes as a consequence of the aggregate economic effects of the tax change, ΔY_i ; and changes in individual marginal tax rates, Δt_i . The total change in disposable income from these three different effects can be approximated by

$$\Delta I_i = t_i \Delta D_i + (1 - t_i) \Delta Y_i - (Y_i - D_i) \Delta t_i$$

How individuals value expected change in disposable income depends upon their marginal utility for private consumption, which we denote as U_I^i .

Individual Preferences

To model individual support for or opposition to any proposed tax change, we must model how people weigh preferences for the different collective objectives and for changes in their personal income. We denote by ΔG , ΔR , Δr , ΔE , and Δp the expected changes in each public objective

as a consequence of the tax alteration. The evaluation of each changed collective outcome can be approximated by multiplying the value placed on a change in the public objective by the change in that objective expected to result from the tax alteration. For example, the evaluation of any change in the income distribution is $U_R^i \Delta R$. Obviously, any tax change is likely to affect all public objectives and the person's income. To get the overall evaluation we need to sum each of these individual evaluations (the Appendix derives this statement from a simple utility maximization model). This expression is

$$\Delta U^i = U_G^i \Delta G + U_R^i \Delta R + U_r^i \Delta r + U_E^i \Delta E + U_p^i \Delta p + U_j^i \Delta j \quad (1)$$

If $\Delta U^i > 1$, we expect the person to favor the proposed alteration, and, conversely, if $\Delta U^i < 0$, we predict they will oppose the change.¹

Our model of tax-policy preferences is constructed in three recursive blocks. The first block models variations in individual evaluations of the public objectives as a function of exogenous variables measuring individual and contextual economic and demographic factors. We discuss the specification of these variables subsequently but for now simply denote their existence by X_1 . We specify one equation for each public objective (and for convenience drop the i superscript):

$$U_G = X_1 B_{1G} + V_{1G} \quad (2)$$

$$U_R = X_1 B_{1R} + V_{1R} \quad (3)$$

$$U_r = X_1 B_{1r} + V_{1r} \quad (4)$$

$$U_E = X_1 B_{1E} + V_{1E} \quad (5)$$

$$U_p = X_1 B_{1p} + V_{1p} \quad (6)$$

These are reduced-form linear equations in which the elements of B_1 estimate how evaluations of different public objectives vary among individuals described by the variables in X_1 .

The second block in the model specifies the relationship between the public-objective evaluations and the responses to survey questions, with separate equations estimated for each question. Responses to any one question, regardless of its format, will reflect considerations specific to that question and possible response biases, as well as positions on the public objectives (see Jackson 1979 and 1983 on these two points and Franklin and Jackson 1986 on the general issue of measurement error). In addition, in several cases we have more than one question that can serve as a measure of a specific public objective, such as several questions about attitudes towards redistribution. In this case, we want to model responses to each question separately and use one of the multiple indicator statistical methods (such as LISREL) to estimate both the underlying model for public objectives and the relationship between these evaluations and the survey response. If we denote by Q_j the responses to question j , by U the public objective evaluations associated with that question, and by X_{2j} the exogenous factors that are also likely to affect these responses (X_{2j} includes factors that might produce any systematic response bias), we have the following model for the survey response:

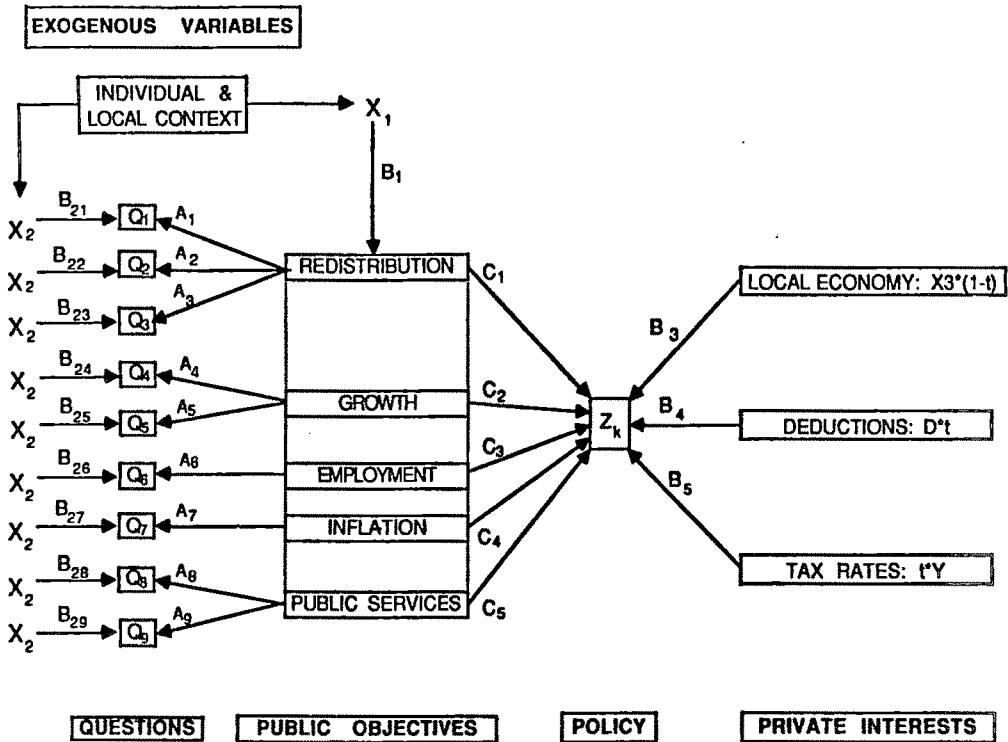
$$Q_j = U \cdot A_{.j} + X_{2j} B_{2j} + e_j \quad (7)$$

The third part of the model estimates the preferences for specific policies, as shown in Equation 1. Included in this equation are the estimated individual evaluations of the public objectives, which are obtained from Equations 2-6, and variables measuring expected changes in disposable income, multiplied by the appropriate tax-rate variables. This model is shown as follows:

$$\begin{aligned} Z_k = & U_G C_{1k} + U_R C_{2k} + U_r C_{3k} \\ & + U_E C_{4k} + U_p C_{5k} + (1 - t) X_3 B_{3k} \\ & + t^* D B_{4k} + t^* Y B_{5k} + V_k \end{aligned} \quad (8)$$

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Figure 1. Schematic of Tax-Preference Model



Z_k is the person's stated preference on policy k , as measured by an appropriate survey question; t , Y , and D represent individual tax rates, gross incomes, and deductions respectively; and X_3 is a set of variables measuring the local economy and how tax changes are likely to alter individual gross incomes. The coefficients C_{1k} - C_{5k} assess the relative importance of collective considerations in setting preferences on specific tax-policy changes while the coefficients B_{3k} - B_{5k} measure the importance of personal considerations in these preferences. If people are purely sociotropic, the coefficients C_{1k} - C_{5k} will be large with small values for B_{3k} - B_{5k} . A purely private-regarding model of preference formation predicts just the opposite pattern. Significant coefficients for both C

and B in Equation 8 indicate that people combine both sociotropic and personal elements in their preferences.

These three blocks, with their appropriate variables and coefficients, are depicted in Figure 1. Evaluations of collective objectives and their relationship with exogenous variables, Equations 2-6, are shown in the center of the diagram. To the left are the multiple indicators of the evaluations of collective objectives. As represented in Equation 7, these responses are also related to specific exogenous variables that may affect responses. Lastly, the effects of collective evaluations and personal interests on specific policy preferences (Equation 8) are shown on the right for a representative policy, Z_k . Our next task is to relate this structure to some

specific tax proposals and to survey data assessing preferences for these proposals.

The Tax Revenue Act of 1978

We can estimate this model using proposals that arose in the debate over the Tax Revenue Act of 1978. The 1978 legislation is particularly interesting because it reversed a number of trends in tax legislation.

The Act

1. created tax cuts that were much less progressive than those passed in the late 1960s and 1970s;
2. reversed the trend in increasing capital-gains taxation;
3. represented the debut of the Kemp-Roth proposal;
4. demonstrated a conscious attempt by the minority party, the Republicans, to develop a package of policy proposals to serve as an alternative to those pushed by the Carter administration and the Democratic leadership.

We can observe how collective and personal interests are involved in individual preference decisions by analyzing several specific tax changes proposed during the 1978 tax debate. One change is the Carter administration's proposal for a stimulative package of progressive personal-income-tax cuts and expanded-investment tax credits. We will also consider the Republican Kemp-Roth plan and the proposal for a reduced capital-gains tax. Finally, we analyze some special proposals that made their way into the 1978 debates, such as tax credits for college tuition, a change in the taxation of corporate dividends, and changes in certain other deductions. Individual evaluations of public objectives and the influence of collective and personal considerations in their positions on the specific taxes are estimated with data collected by the Roper Organization during spring 1978.²

Evaluation of Public Objectives

The questions used to measure preferences for each public objective, the objectives they are presumed to measure, and the distribution of responses to each question are shown in Table 1. The responses for these questions are trichotomous and scored on a 0./0.5/1.0 basis, with the *don't know* response defined in our analysis as a middle category. Consequently, we can measure the signs, but not the magnitude, of the marginal utilities in Equation 1. For example, we know that 47% of the respondents say the poor pay too much in taxes. However, we are unable to determine by how much each of these respondents would change tax rates for the poor or for which respondents this preference might be very important and outweigh a personal income loss associated with a specific tax proposal directed towards this objective.³

Also included in our analysis is a general measure of individual dissatisfaction with the amount of income tax paid. There was considerable speculation during 1978 that the public was simply fed up with and was revolting against any and all taxes. Much of this speculation was sparked by the approval of Proposition 13 in California. Although there are numerous explanations for the passage of this referendum (see Sears and Citrin 1983; Proceedings 1979), it was widely believed in 1978 that voters displeased with the amount of taxes they were paying would vote to reduce their own taxes whenever they had the opportunity. However, they were also seen by some as "free-riders," unwilling to reduce government services to compensate for these tax cuts. Dissatisfaction, if it is a broad symbolic attribute, predicts support for any tax reduction, regardless of personal interests. Dissatisfaction is measured with a 10-point scale.⁴ Respondents rated their feelings about the amount of income taxes they paid on a 10-point scale, with 1 being perfectly

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Table 1. Distribution of Preferences for Tax Objectives (%)

Questions	Favor	Oppose	Don't Know
Redistribution			
1. Poor people pay too much in taxes?	47	4	50 ^a
2. Redistribute income?	60	32	8
3. Redistribute income with taxes ^b ?	30	62	8
Stimulation of Economic Growth			
4. Use business tax credits to create jobs?	24	68 ^c	8
5. Use taxes to help depressed firms?	27	62	11
Stabilization			
6. Most important to control inflation ^d ?	38	62	—
7. Most important to lower unemployment ^e ?	12	88	—
Spending for Services			
8. Purpose of tax system not just to raise money?	46	41	12
9. Increase government services ^f ?	86	14	—

^aIncludes the 37% who feel the poor pay the right amount of taxes.

^bUses the same question as "Redistribute income." This separates those who would use other means from those who favor using taxes to redistribute income.

^cIncludes the 48% who preferred government-created public-service jobs and the 11% who favored direct business subsidies to combat unemployment and welfare problems.

^dQuestion asks whether respondent feels that controlling inflation is the first or second most important thing that needs to be done in our country.

^eQuestion asks whether respondent feels that lowering unemployment is the first or second most important thing that needs to be done in our country.

^fHalf of the sample was given a short list of government programs and asked which they would be willing to pay more taxes to have increased. The 14% opposed said no to all programs.

satisfied and 10 meaning extremely angry; 16% of the respondents gave an answer between 1 and 3, 60% between 4 and 7, and 24% between 8 and 10.

Responses to the questions in Table 1 and, presumably, perceptions of tax policy goals are dominated by evaluations of the redistribution objective. In Figure 1 and Table 1, responses to each question are treated as multiple indicators of the objective with which the question is grouped and a LISREL procedure was used to estimate the basic preference structure represented by Equations 2-7. These estimates produced two important results. The statistical fit indicated that our expectations were likely incorrect and that a very high degree of similarity existed between the equations for each objective (the statistical fit indicated that

our hypotheses, if correct, would produce by chance results with a worse fit less than 10% of the time).⁵ A likely explanation for the poor fit and for the equations' similarity is that responses to several of the questions are related to a single basic objective as well as to the hypothesized objective. For example, if responses to the question about using taxes to help depressed businesses were strongly influenced by demands for redistribution, our model would obviously fit poorly. Furthermore, the statistical method used would attempt to estimate demands for business support that are highly correlated with redistribution preferences (this result is the classic specification bias resulting from omitted variables found in any linear model).

Based on these results, we respecified

Table 2. Estimated Model of Preferences for Collective Objectives

Demographic Variables	Redistribute Income	Finance Services	Stimulate Growth	Control Inflation
Individual Measures				
Income ^a	-.078**	-.021		
Income > \$25,000	.001	.025	.032	
Non-white	.199**	-.005		
Age ^a	-.124**	-.245**		.218**
Years of schooling ^a	-.079	-.035	.477**	
Union	.005	-.045**	-.057**	
Economic Context Measures				
Tax rate ^a		.011		.089*
East/Midwest	.103**	.005	.036	
South	.052**	-.006	-.024	
Unemployment ^{ab}	.154**	-.308**		.003
Percent non-white ^{ab}	-.014	.038**		
Income per capita ^{ab}	.291**	.130		.457**
Number of cases = 1,922.				

^aVariable in logarithmic form.

^bVariable based on respondent's county.

*Coefficient larger than 1.65 times estimated asymptotic standard error.

**Coefficient larger than 1.96 times estimated asymptotic standard error.

and reestimated the model, incorporating the hypothesis that redistribution demands influence the responses to most questions. This model change is accomplished by including redistribution evaluations, U_R , in all but the inflation (Question 6) and government services (Question 9) equations. The model discussed and rejected above also implied that desires to promote economic growth influenced responses to the inflation question. Based on this result we included the growth preference in the inflation equation (Question 6). This respecification led to a new model. In the respecified model, certain exogenous variables are omitted from the growth, unemployment, and inflation equations because they have no relationship with these evaluations once the effects of redistribution and growth preferences are included (the exact specification of each preference equation will be seen in Table 2). Responses to the unemployment question are solely a func-

tion of redistribution preferences, so that equation is omitted. The statistical fit of this model is quite good, in that we could expect a worse fit by chance 40% of the time. We first discuss the models for each collective objective and then show how these preferences affect the responses to the questions shown in Table 1.

Preferences for Public Objectives

Individual preferences for public objectives are related to two types of factors, as shown in Table 2: (1) characteristics of the respondents and (2) measures of respondents' local economies and communities. Each column in the table is a regression equation. The table entries give the expected change in each collective preference for a unit change in the associated explanatory variable (tables of standard errors are available from the authors on request). All of the continuous variables—income, education, age, tax rate, the

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area's per capita income, and the area's black population (as percentage of total population) are expressed in logarithmic form.

There are several important relationships explaining people's evaluations of public objectives. Given the central role of redistribution preferences, we will concentrate our discussion on that equation. As is to be expected, support for more income redistribution decreases with income. What is surprising is the weakness of the relationship (the survey response variables are scaled from 0 to 1, so coefficients can be interpreted in terms of the resulting difference in the probability of favoring more redistribution). The variables are in log form, so the coefficient of $-.078$ implies that support for more redistribution decreases by .02 with each doubling of income and that the difference between a person earning \$10 thousand and someone earning \$50 thousand in 1978 dollars is only .05. By contrast, support decreases by .04 with a doubling of respondent age, and being black is associated with a .20 increase in support.

The variables associated with the largest differences in redistribution preferences are the contextual variables describing respondents' communities and local economies. To illustrate, an individual living in a wealthy county with a high unemployment rate (a per capita income of \$6 thousand and 12% unemployed) is .18 more likely to favor redistribution than the "same" individual living in a low-income county with a low unemployment rate (\$3 thousand per capita income and 3% unemployed). This probability difference could increase to .29 if we consider the regional difference between the North and Midwest and the West regions. The magnitude of the contextual differences is much greater than the individual variations cited above.⁶

There are several notable results in the other equations. The only evaluation that

is not strongly related to the contextual variables is the wish to use tax policy to promote economic growth. Variations in this preference are strongly related to education and weakly to union membership but not to other variables in any significant or substantial way (inclusion of the contextual variables in this equation indicated they had little or no effect). Increasing age was strongly associated with opposition to increased federal-government services and with support for controlling inflation. Finally, the only strong but counterintuitive result is the opposition to any increased federal-government services in areas with high unemployment. We have no obvious explanation, or rationalization, for this outcome.

These findings highlight an important methodological conclusion concerning the typical omission of contextual variables from models estimating policy attitudes. Studies that include only respondent attributes assume that people are homogeneous in all respects other than those individual variables in the analysis. Our results show the weakness of this assumption. Individuals sharing personal characteristics—such as age, education, income, or race—do not maintain similar policy preferences as they are "placed" within different local economic and social environments.

Preferences and Question Responses

In Table 3 we give the equations modeling the responses to each question shown in Table 1. Again, each column should be read as a separate regression equation relating responses to collective-objective preferences, to personal considerations, and to evidence of interest in, and knowledge of, tax policy. The latter two variables are included to represent the effects of any systematic bias from nonattitudes. It is apparent from these entries that redistribution preferences influence responses

Table 3. Model of Question Responses and Relationship to Tax Objectives

Variables	Questions (See Table 1)									
	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10
Collective-objective Preferences										
Redistribution	.46**	1.00	.67**	-.50**	.46**		1.18**	.44**		.30**
Business growth				1.00	-.07	.88**				.08
Inflation						1.00	-.03			.15*
Services									1.00	
Personal Considerations										
Income	-.04*									.12**
Income > \$25,000	-.03*									.02*
Tax rate	.05*	.07*	-.06*		-.13**			-.08*		.08**
Age								-.21**		
East/Midwest							-.10**			
South							-.04*			
Local										
unemployment ^a					.22**					
Percent non-white ^a		-.05*								
Interest and Knowledge of Tax Policy										
No interest	.04**	.02	.04*	-.03*	-.01	-.06*	.04	.04*	-.06**	.00
No knowledge	.02*	.02	.05**	-.05**	-.02	-.04*	-.01	.03*	.02	.01

^aVariable based on respondent's county.

*Coefficient larger than 1.65 times the estimated asymptotic standard error.

**Coefficient larger than 1.96 times the estimated asymptotic standard error.

to most of the questions about public objectives. Only responses to questions about additional spending and about the importance of halting inflation are not affected by the redistribution component. The influences of redistribution preferences are also quite large, as indicated by the magnitudes of the coefficients. Respondents who desired a more progressive income distribution wanted to use taxes for reasons other than just to raise money, reduce unemployment, and help distressed businesses, and they opposed tax subsidies for businesses as a means to attack welfare and unemployment problems. These results support the arguments that policy preferences are rooted in basic, fundamental values (or symbols) and that attitudes towards a small set of values influence a broad set of preferences, or at least responses to questions about preferences (Sears et al. 1980).

Average tax rate was the only variable representing personal economic interest that was consistently related to question responses and then only when the question contained a reference to using taxes for some specific purpose. Individual tax rates were strongly and negatively related to a desire to use taxes to subsidize depressed business, for purposes other than raising money, and to redistribute income. They were positively related to the desire to redistribute income by any means and to the belief that poor people should pay lower taxes. The other variables included to assess personal interests were occasionally important but not systematically so. Income was negatively related only to the proposition that the poor pay too much in taxes, after controlling for redistribution preferences. Similarly, local unemployment levels played an important role in individual

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Table 4. Preferences for Specific Tax Policies (%)

Tax Policy Options	Responses		
	Favor	Oppose	Don't Know
Capital-gains exemption	43	40	17
Gasoline-tax deduction	76	19	5
Mortgage-interest deduction	88	7	5
College-tuition tax credit	23	70 ^a	7
Index rates for inflation	57	32	11
Carter's \$25-billion tax cut to stimulate economy	53	37	11
	Only Corporation	Only Those Receiving Dividends	Both and Don't Know
Where corporate dividends should be taxed	35	15	50
	Increased Revenue	Leave the Same and Don't Know	Decrease Revenue
Tax cuts will stimulate more federal revenue or raise deficits	18	59	23

^aIncludes the 34% of the sample who favored making more people eligible for loans, the 20% who wanted more direct grants to college, and the 16% who opposed any form of aid.

preferences, once redistribution demands were controlled.

Expressions of dissatisfaction with personal taxes were significantly related to both collective and personal interests. Responses to this question were included to represent either some broadly based dissatisfaction with the nation's income-tax system or self-interest (free riders wishing to reduce their own taxes without making any compensating changes in other policies). We found no evidence of any broad-based dissatisfaction of the tax-revolt sort. Self-interests were clearly present, as indicated by the positive coefficients on the income and tax variables, but so were public concerns. Dissatisfaction was significantly related to interests in redistribution and controlling inflation. These results have two implications for our subsequent analysis. They clearly point out the importance of collective considerations in evaluating questions about tax policy, even one supposedly loaded with self-interest. Secondly, we will omit "dissatisfaction" from our con-

sideration of specific tax proposals. It does not constitute a broad public concern about taxes in general nor does it supply information about private interests beyond that represented by the income and tax-rate variables, which are already included in Equation 8.

Positions on Specific Tax Policies

The specific tax policies studied are shown in Table 4, along with the support each received from the respondents. Support or opposition to the specific taxes and tax reforms is estimated with a linear-regression model. The explanatory variables are individuals' estimated preferences for collective objectives (obtained from the equations in Table 2), measures of direct self-interest, and measures of local economies.⁷ (The survey included questions about respondents' average tax rates and their use of specific deductions, from

Table 5. Model of Attitudes Towards Specific Tax Policies

Variable	Corporate Dividend	Kemp-Roth	Carter Tax Cut	Capital Gains	Index Taxes	Tuition Credits	Mortgage Deduction	Gax Tax Deduction
Collective-objective Preferences								
Redistribution	-.17	.24**	.34**	-.43**	-.02	-.21*	-.42**	-.26**
Business growth	.22	.03	.34*	-.07	.17	.01	.06	.18
Inflation	.38*	.12	.11	.42	-.93**	-.03	-.09	.22
Services	.06	-.21	-.07	-.13	-.62	.23	-.08	-.20
Personal Variables								
Own stock ^a	.24**			.22*				
Only salary income ^a			.42**					
Own home ^a				-.15			.22**	
Own × housing price change ^a				.54				
Deduct mortgage interest ^a							.19**	
Deduct gas tax ^a								.47**
Child < 13						.02		
Child 13-18						.00		
Child > 18						.08**		
Income and Tax Rates								
Tax rate × income	-.04	-.02	.03	-.02	.27**	.05	-.07	.01
Tax rate × income > \$25,000			-.44**					
Economic Context Measures								
Population change ^b	.18**	.08*	-.07	.31**	.14*	.12*	.02	.07
Per capita income change ^b	.04	-.22*	.40**	-.05	.02	.61**	.01	.08
Unemployment rate change ^b	1.29**	-1.57**	4.22**	1.74**	-3.13**	1.26*	.14	.78
Per capita income ^b	-.01	-.00	.01	.01	.07**	.03	-.01	.00
Partisanship								
Republican	-.05**	-.00	-.03	.03	-.02	.03	.01	-.01
Independent	-.03	.04	-.01	-.03	-.02	.05*	.01	-.04
No party	-.05**	.02	-.05*	-.07**	.04	.02	.02	-.04
Interest and Knowledge of Tax Policy								
No interest	-.00	-.02	-.02	-.03	-.02	-.04	-.03	-.04
No knowledge	-.00	-.04**	-.04*	-.01	-.05**	-.01	-.02	-.05**
Constant	.22	.57	.05	.18	.77	-.23	.91	.45
R ²	.022	.014	.051	.051	.035	.035	.049	.042
Number of Cases = 1,965								

^aVariable multiplied by individual's tax rate.

^bVariable multiplied by (1 - tax rate).

*Coefficient larger than 1.65 times estimated asymptotic standard error.

**Coefficient larger than 1.96 times estimated asymptotic standard error.

which we construct the primary self-interest measures.)

Collective Concerns

Individual redistribution preferences are important determinants of preferences for specific tax changes. The estimated

equations of these preferences are shown in Table 5, with each column being a separate regression equation explaining the variable listed at the top of the column. The coefficients assessing the influence of redistribution preferences are statistically significant at the .05 level in five of the eight equations and their mag-

nitude exceeds .2 in six cases. The coefficients measuring the influence of desires to use taxes to promote business, to increase federal spending, and to control inflation are statistically significant in three equations. Again, we see the dominance of redistribution interests in positions on different tax issues.

Redistribution. Individuals desiring a redistribution of income quite strongly oppose mortgage interest and gasoline-tax deductions, capital-gains exemptions, and tuition tax credits, all parts of the tax code vigorously attacked as favoring wealthier taxpayers. These same individuals favor Carter's proposed tax cuts, which provided greater tax relief for the poor than the wealthy. Surprisingly, they agree with the supply-siders' hypothesis that reduced taxes will increase federal revenues. Overall, the impact of preferences for this fundamental objective is large and in the expected direction for tax issues that were clearly discussed in redistributional terms in 1978.

Other Objectives. Positions on the other public objectives have less systematic influence on evaluations of specific tax changes than do redistribution preferences. Desires to use taxes to promote business activity have their largest influence on support for Carter's tax package. In spite of symbolic attacks on three-martini lunches and other business expenses, Carter's proposal liberally expanded investment tax credits and other allowances designed to stimulate investment by large, capital-intensive firms. Respondents who favor more government spending strongly oppose indexing and mildly oppose other policies that reduce revenue, such as exemptions, deductions, and the Kemp-Roth hypothesis. These people do favor direct tuition tax credits, suggesting that that plan may appear more as an expenditure than a tax policy. The effects of demands to control inflation do not follow any particular pat-

tern. These demands are very strongly related to opposition to indexing, as Keynesian economics would require. Otherwise it is hard, on both statistical and substantive grounds, to reject the null hypothesis that inflation concerns did not effect positions on individual tax changes.

Personal and Local Economic Interests

One should not infer that individual evaluations of specific tax proposals are devoid of self-interest. We will present clear evidence to the contrary. Self-interest attitudes stem from the direct effect of a tax change on individual after-tax incomes by altering tax rates or deductions and from the indirect effect of the tax change on expected gross income through impacts on local economies.

Personal income and tax effects. Self-interest effects are best exemplified by the role of personal deductions. People who own homes and take the mortgage-interest deduction and those who use the gas-tax deduction are far more supportive of these deductions than are people who do not use them. Support for the capital-gains exemption is higher among people who own stocks and who own their own homes. The effect of homeownership increases the size of local housing price increases between 1972 and 1978. The larger the price increase and the larger the potential capital gains for homeowners, the higher the approval for the exemption.⁸ Variables measuring the use of these deductions and exemptions are multiplied by tax rates because marginal tax rates determine the value of any deduction or exemption. Thus, support for each deduction and for capital-gains exemptions equals the appropriate coefficient shown in Table 5 times tax rates (technically, marginal tax rates rather than average tax rates should be used, but this information was not included in the survey). Respondents with college-age children are 8%

more likely than those without to favor the college-tuition tax credit. This term is not multiplied by tax rates because the value of a tax credit does not vary with rates. Lastly, support for Carter's tax plan, which tried to reduce taxes for earned (but not unearned) income, was considerably higher among those who receive all their income from salaries.

Tax rates had their greatest effect on preferences for the proposals that promised to alter rates across the board. Indexing, which decreases tax rates by extending the range of current tax brackets to offset inflation, receives more support as tax rates and incomes rise, as would be expected from the self-interest model. Respondents with larger incomes in higher brackets will gain far more after-tax income than those with low incomes and in low brackets. Carter's highly progressive plan received significantly less support among upper-income people, and this support decreased as tax rates increased. Tax rates and incomes had little or no effect on evaluations of the other tax proposals, which did not attempt to directly alter tax rates.

Aggregate-income effects. Many of the tax measures debated in 1978 were designed to alter investment patterns that will have varied impacts on local economies, thus differentially altering individual incomes. The influence of aggregate economic effects on attitudes is substantial, except for evaluations of the mortgage-interest and gas-tax deductions, which were not debated in aggregate economic terms. Residents in areas with increasing unemployment strongly favor Carter's tax cut, which was advertised as a plan to stimulate stagnant industries. They oppose the capital-gains exemption, indexing, and the Kemp-Roth hypothesis that reducing taxes would increase revenues, all of which are likely to aid growing local economies. Residents of areas with increasing population and, pre-

sumably, future increases in wealth support the capital-gains exemption, indexing, tuition tax credits, and the taxation of corporate profits at the corporation, all of which are likely to increase personal incomes in growing areas.

Partisanship

There is little evidence of partisan differences in tax-policy preferences.⁹ The differences between the preferences of Republicans and Democrats for specific tax policies are .03, or less, in all but one case (Republicans are .05 more likely than Democrats to prefer collecting corporate taxes from individual investors). The largest party effects are between Democrats and those preferring "No party," with the latter respondents more likely to oppose both Carter's tax cut and capital-gains exemptions (the magnitudes of these differences are less than .07). These results suggest there is an absence of partisan cue giving and cue taking on tax policy and that individuals' preferences are much more guided by redistributive preferences and their own economic stake in any change.

Knowledge and Interest

We have also included in the preference equations assessments of individuals' interest in current events and knowledge of tax policy. These variables help correct for any systematic response biases contained in individuals' answers, attributable to cues provided by the questions and to individuals' perceptions of the "right" answer. The lower a person's interest and knowledge, the more likely he or she is to give the expected or correct answer rather than a true preference. As shown in Table 3, the uninterested and less knowledgeable are more likely to give affirmative answers to the redistribution questions and negative responses to questions about support for business. For the

questions about specific policies, as displayed in Table 5, interest has very little effect and lack of knowledge has only small effects. The less knowledgeable are more likely to oppose Carter's cut, the Kemp-Roth hypothesis, indexing, and the gas-tax deduction. These interest and knowledge effects are generally small, with coefficients of .05 or less.

Conclusions

The empirical results of our analysis of tax-policy preferences have broad implications in three different areas, all of which have been alluded to above but warrant additional emphasis and clarification. One point derives from the importance of aggregate economic variations on individual preferences. As mentioned earlier, this result has methodological and substantive implications for studies of individual policy preferences. The other two points are substantive, relating to how we think about the development of individual preferences and their possible importance in the political process.

The importance of collective considerations in structuring individual preferences is mentioned again here only to reaffirm a conceptual point made in the introduction. We distinguished collective objectives, or interests, from private considerations on the basis of whether the results affect the welfare of more than one individual. This definition makes it possible to have disagreement and conflict over public interests as well as conflict arising from competing private interests. Our evidence suggests that individuals have differing views about objectives that are public in nature, such as the proper income distribution, and that these views create conflicts about specific public-policy choices. Thus, some of the conflicts confronting our political institutions are those generated by differing evaluations of what collective activities society should

undertake.

The public objective basic to preferences for tax policies is individual views on income redistribution.¹⁰ The central place of redistribution preferences is demonstrated in their effect on other possible tax objectives and on evaluations of specific taxes and possible changes in the tax law. Responses to questions about the importance of fighting unemployment, using taxes to support depressed businesses, the correct purpose of taxes, and even about how dissatisfied people are with their tax payments are all influenced by redistribution views. Some of these attitudes were originally set out as reflecting independent tax objectives, yet these objectives turn out to be primarily derivations of redistribution preferences. As expected, evaluations of particular parts of the tax structure, such as tax cuts, capital-gains exemptions, and certain other deductions are strongly affected by attitudes towards income redistribution. It is not possible, then, to discuss the politics surrounding individual taxes or changes in the tax structure without addressing the redistribution issue.

The central role of redistribution preferences in evaluations of specific tax changes has significant implications for our notion of political debate and for efforts to build support or opposition for policy initiatives. People evaluate policy actions in terms of how those actions are likely to affect public objectives as well as by how they will be affected personally. Appeals to individual economic interests are obviously a part of politics, but among the mass public successful efforts to relate policy to collective values are effective ways to influence public opinion. Republican strategy on tax policy beginning in 1978 clearly recognizes this implication. They increasingly stressed the importance of economic growth as a public objective and the need for tax cuts, reduced inflation, and capital formation incentives to meet this objec-

tive. These efforts to have tax changes evaluated by their impact on investment and economic growth do not show up in our analysis. However, it is quite possible that after Reagan's 1980 campaign and his efforts to pass the 1981 tax bill, a similar analysis would show much greater influence of the growth objective and of the conflict between growth and redistribution posed by Reagan's proposals and subsequent debate.

People's stated preferences for collective, or public, objectives are not merely a rationalization for their own economic interests. The factors influencing individual redistribution preferences, such as education, race, age, local per capita income, and unemployment rate, indicate that redistribution preferences are more than a simple manifestation of an individual's current place in the income distribution. Income is related to redistribution preferences, as expected, though its influence was relatively small. Furthermore, many individuals with equivalent incomes have considerably different views on whether the poor should pay lower taxes and whether income should be transferred from the rich to the poor.

Other important determinants of redistribution preferences and of evaluations of particular tax policies are the characteristics of the respondent's local economy and how it is changing. Comparable individuals with identical incomes living in different economic regions have substantially different views towards redistribution and specific taxes. The substantive implications of this finding is that individuals can identify with collective attributes of society, or at least of local communities, and not focus solely on their own situations and incomes.

The methodological implications of these results warrant future attention. Most work estimating individual preferences and studying their formation relies on survey data, as one must to study individual behavior. However, most of these analyses exclude aggregate eco-

nomic and social considerations, save for the occasional inclusion of region or residence variables (see Wright 1977, for an important exception to this pattern). Our results clearly point to the need to pay more attention to the social and economic environment in which individuals live, as determinants of political preferences.

The importance of the aggregate economic variables in Tables 2 and 5 indicates considerable contextual variation in positions on key elements of the 1978 Tax Revenue Act. These variations have implications for the legislative process. Our findings suggest that some tax issues create considerable conflict between areas and only small differences among individuals living in the same area, for instance, Carter's proposed tax cut and the effort to increase the capital-gains exemption. Other issues elicit the opposite pattern of conflict, such as taxing corporation profits and the legitimacy of the mortgage-interest and gas-tax deductions.

It is only speculation at this point, but it seems reasonable to predict that issues and policies that generate substantial regional preference differences will produce greater amounts of conflict within Congress than policies in which the conflicts are primarily between individuals. Given that congressional representation is based on geography, issues that split each chamber into regional factions would appear to be more divisive than issues that force members to choose among interests in their own constituencies. In terms of tax legislation, this model would predict that Congress would have more difficulty in passing (or rejecting) Carter's tax cut, the Republican effort to increase capital-gains exemptions, and indexing than in deciding on the mortgage-interest and gas-tax deductions, or the issue of how to tax corporate profits. The only way to develop measures of these conflicts, however, and to test the predictions one draws from them, is through the type of individual-preference analysis developed here.

Appendix

In this appendix we derive the expression for individual evaluations of specific tax proposals shown in Equation 8. We begin with a standard individual utility function denoted as U^i . The arguments in this utility function are the collective objectives associated with tax policy and the person's after-tax income, $U^i = U(G, R, r, E, p, I_i)$, where G , R , r , E , and p are the public objectives discussed in the text and I_i is the person's disposable after-tax income. The effects on these public objectives of some alteration in current tax policy, denoted by a , are given by $\delta G/\delta a$, $\delta R/\delta a$, $\delta r/\delta a$, $\delta E/\delta a$, and $\delta p/\delta a$. We will denote these partials by ΔG_a , ΔR_a , Δr_a , ΔE_a , and Δp_a respectively. The expected change in disposable income is

$$\begin{aligned}\delta I_i/\delta a &= \Delta I_{ia} = (1 - t_i)\delta Y_i/\delta a \\ &+ t_i\delta D_i/\delta a - (Y_i - D_i)\delta t_i/\delta a \\ &= (1 - t_i)\Delta Y_{ia} + t_i\Delta D_{ia} \\ &- (Y_i - D_i)\Delta t_{ia}\end{aligned}$$

The marginal utility from a change in each public objective is $\delta U^i/\delta G = U_G^i$, $\delta U^i/\delta R = U_R^i$, $\delta U^i/\delta r = U_r^i$, $\delta U^i/\delta E = U_E^i$, and $\delta U^i/\delta p = U_p^i$ and the marginal utility for disposable income is $\delta U^i/\delta I_i = U_{I_i}^i$. The expected change in utility from the tax alteration is $\delta U^i/\delta a = U_a^i$, or ΔU_a^i . The expression for this total differential is

$$\begin{aligned}\Delta U_a^i &= (U_R^i\Delta R_a + U_G^i\Delta G_a + U_r^i\Delta r_a \\ &+ U_E^i\Delta E_a + U_p^i\Delta p_a) \\ &+ U_{I_i}^i[(1 - t_i)\Delta Y_{ia} + t_i\Delta D_{ia} \\ &- (Y_i - D_i)\Delta t_{ia}]\end{aligned}$$

If $\Delta U_a^i > 0$, the individual is presumed to favor a ; alternatively, if $\Delta U_a^i < 0$, we expect a to be opposed. This expression, minus the a subscripts, is Equation 1 in the text, from which we derived Equation 8 and the empirical estimations.

Notes

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1. The terms relating changes in public objectives to alternative taxes, e.g., ΔG and ΔR , summarize very complex social processes, about which there is uncertainty and debate. This debate is an important element in the political process but not one we address in this paper. When we estimate the operationalized version of Equation 1, not measuring individual differences in how tax changes are perceived to affect collective goals introduces an implicit downward bias into our estimates of the effects of collective preferences on specific policy preferences. For example, if everyone who agrees that taxes should be used to redistribute income divides evenly on whether tax subsidies to distressed business firms redistribute income, we would find no relationship between redistribution attitudes and the likelihood of favoring tax breaks for such enterprises.

2. These data are in Roper Study No. 648, "The American Public and the Income Tax System," conducted in spring 1978 for H and R Block. The data and information about the study were obtained from the Roper Center, University of Connecticut, Storrs. These data have been augmented by merging variables describing the economic and social structure of respondents' counties with the survey responses.

3. As with any data not designed for the specific research at hand, there are some difficulties in matching theoretical concepts with the variables included in the data set. The most obvious case is the use of the question about business tax credits as a proxy for wanting to use taxes to encourage investment and economic growth in the private sector. It would be quite easy to design a better question for discovering individual preferences for this objective, but we believe the present question is adequate for our purposes. We expect individuals who want to use tax policy to promote investment and growth will suggest using business tax credits to reduce unemployment and welfare, while those who want to use taxes for other purposes are more likely to prefer public-service or direct-grant programs.

4. We convert this scale to one ranging from 0 (satisfied) to 1 (extremely angry) to match the scales of the other variables.

5. The statistical fit measures how well the estimated model describes the observed data. The better the fit, the harder it is to reject the null hypothesis that the specified model is the correct one and conversely for a poor fit. The chi-squared statistic is used to evaluate the fit and to estimate how likely it would be to get a worse fit by chance if in fact the estimated model were the correct one. The better the fit, the lower the value of the chi-squared; and for a

given number of degrees of freedom, the greater the likelihood of getting a worse fit simply by chance, and the more likely one is to accept the estimated model as the true model.

6. These results parallel those of Davis and Jackson (1974), who found that, in 1969, support for a negative income tax was strongest in the northern industrial areas and weakest in the South and rural areas, which was completely opposite to the plan's regional redistribution effects.

7. This procedure is an instrumental variables method using ordinary least squares (OLS) regression. It corrects for any stochastic covariance between responses to the different questions and for attenuation attributable to measurement error in the responses. Probit analysis was used on some of the equations and produced no significantly different results. OLS was used to reduce costs and to produce more easily interpreted results.

8. The interaction between housing-price changes and homeownership implies that virtually all homeowners favor the capital-gains tax, given even the smallest housing-price increase within the sample. To see this effect, the two relevant coefficients imply that Support for Capital Gains = $(-.15 + .54\pi_h)$ Own, where π_h is the change in local housing prices between 1972 and 1978. The minimum value of this change in the sample was .31, giving a coefficient on Own of .02. By contrast, the average and maximum housing-price changes were .52 and .68, giving coefficients on homeownership of .13 and .22 respectively.

9. These coefficients may overstate partisan differences because we do not have the necessary information to take into account the reciprocal effects of tax-policy preferences on partisanship.

10. Thurow (1971) makes the point that society's income distribution is a classic public, or collective, good.

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THE DYNAMICS OF STRUCTURAL REALIGNMENT

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Governments render decisions on how resources and values are allocated in a society. In the United States, Congress is the institution in which most of the key allocating decisions are made. To the extent the U.S. political system is integrated, the coalitions that form around the issues debated in Congress should be reflected in the coalitions that support presidential candidates and those that support the major political parties. We formulate a spatial theory of political change in which new ideological cleavages appear in congressional behavior and presidential elections and gradually reorganize the mass party base. The theory leads us explicitly to consider the question of dealignment and to specify conditions under which the parties will lose support from voters.

How and why does the party system in the United States realign? While there have been many attempts to define *realignment* precisely, the essence of any definition is the idea that in a realignment the underlying support for the two major parties changes. Because some change in party support is occurring most of the time, it is almost a matter of taste to say a political system is undergoing realignment at any given moment. However, at certain times the change is obvious and dramatic enough that virtually everyone acknowledges that a realignment has occurred; at other times the arguments are more subtle and the evaluation less consensual.

The three most ambitious contemporary theories of realignment have been offered by Sundquist (1973), Clubb, Flanigan, and Zingale (1980), and Burnham (1970). Sundquist proposes a spatial theory of realignment in which a new issue quite literally realigns the main conflict dimension in the society and causes new electoral coalitions to form. Clubb,

Flanigan, and Zingale emphasize the importance for realignment of a party's continuing policy control. Burnham proposes a *dealignment* theory that focuses on decline in support for the parties in general rather than the changing balance of mass support. We shall formulate a spatial theory of political change in which new ideological cleavages appear in congressional behavior and presidential elections and gradually reorganize the mass party base. Then we shall return to the implications of our theory for the dealignment question.

The Spatial Theory of Realignment

The key to the spatial theory of realignment is that at any given time there is a central dimension of conflict in the society and realignments are generated when this conflict dimension is altered in a way that creates new political coalitions. Figures 1-2 show the basic Sundquist model. Consider the situation depicted in Figure 1.

Figure 1. Basic Spatial-Realignment Model, Panel A

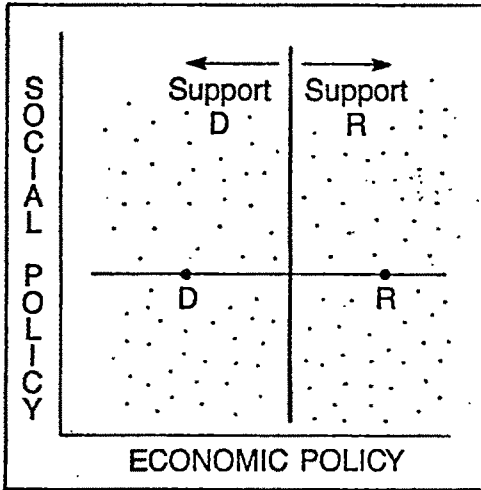
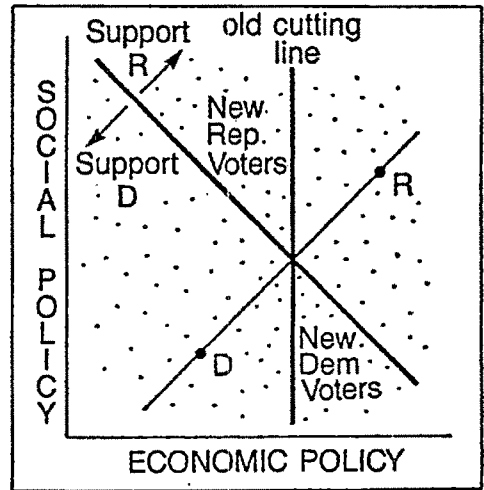


Figure 2. Basic Spatial-Realignment Model, Panel B



Here two candidates, D and R, are competing for the presidency. D represents the issue positions of the Democratic candidate, while R represents the issue positions of the Republican candidate; the dots represent individual voters. The horizontal axis is labeled *economic policy* and the vertical axis is labeled *social policy*. Notice that in Figure 1 the two candidates have identical positions on the vertical dimension, implying that they take the same position on social issues. They differ considerably on the horizontal economic dimension. Hence, the choice between the candidates is based exclusively on economic-policy position. The heavy *cutting line* drawn through the space divides the voters into those closer to the Republican candidate on the right side of the line and those closer to the Democratic candidate on the left side of the line. The line determined by the candidate points represents what we shall call the *active axis* of the election; it represents the central dimension of conflict for the election. In this case the active axis is horizontal because the candidates differ

only with regard to economic issues.

In Figure 2, we illustrate a change in the dimensions of conflict across elections. In this panel the candidates differ in their positions on both the social and the economic dimensions. This leads to several important changes. First, and most critical from a realignment perspective, the *cutting line* dividing the coalitions supporting the two parties has changed. Several individuals who were supporting the Democratic candidate in Figure 1 are now supporting the Republican candidate, and vice-versa. Notice, too, that the *active axis* is reoriented: rather than being horizontal, it now has a slope between the two issue dimensions. In general, the slope of the active axis reflects the relative importance of the underlying dimensions in the election. When only the horizontal dimension is important, the axis is horizontal; when both dimensions are equally important, the axis is oriented exactly between the underlying dimensions. A realignment that follows this general pattern we shall call a *structural realignment* in that it is induced by a basic change in

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Figure 3. Performance-based Shifts in Cutting Line, Panel A

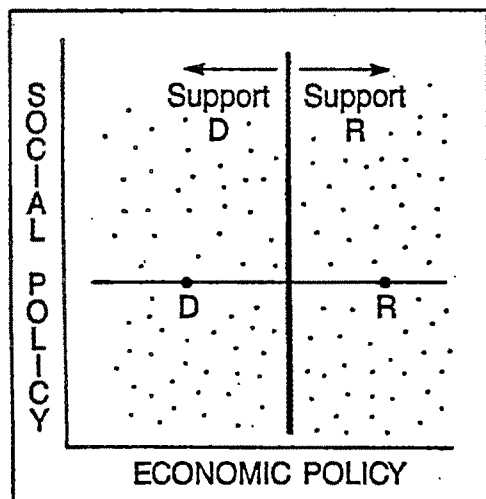
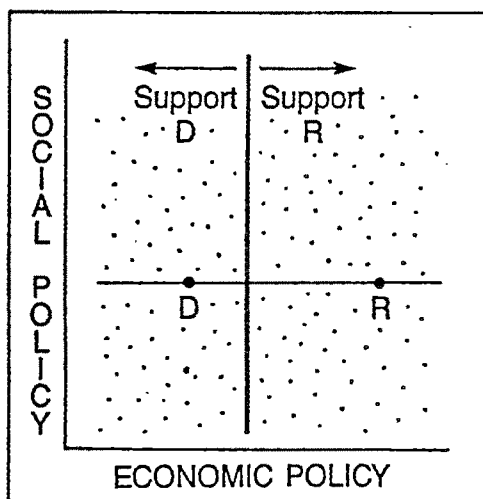


Figure 4. Performance-based Shifts in Cutting Line, Panel B



the structure of conflict—for example, the introduction of social issues to the political agenda.

Structural realignment can be distinguished from realignment rooted in the quality of performance of the incumbent administration. There is accumulated evidence in studies of presidential outcomes that who wins and loses elections rests on how well the incumbent administration has performed. Great success or failure in handling the economy, big scandals, and foreign wars can cause massive shifts in party strength without changing the structure of conflict. The thrust of Clubb, Flanagan, and Zingale's (1980) critique of the Sundquist model is that performance is critical to electoral success.

The impact of performance can be readily illustrated spatially. Figures 3-4 show two elections with identical active axes, that is, with identical conflict dimensions. To keep both figures simple, the conflict is only along the economic left-right dimension. However, the outcome of the election is very different in the two instances. In the first the Demo-

cratic candidate is the clear winner; in the second the Republican candidate wins. The different electoral outcomes in the two panels are determined by the location of the cutting line. We shall call a realignment motivated by movement in the cutting line a *performance realignment*.¹

While both structure and performance can change the distribution of support for the political parties, they are fundamentally different. Structural change flows from the policy debate and concerns the nature of support coalitions; performance change flows from the management of government and concerns electoral success. Thus we see a contrast between the structural question (Who is on which side?) and the performance question (Who is gaining or losing strength across the entire electorate?). Both are politically consequential; yet they are nicely separable. In the spatial context, structure is concerned with axis orientation, while performance is concerned with the placement of cutting lines.

We focus on structural realignment. Our interest in structural change arises

from the importance of structured competition for a political system. If there were no more to democratic control of the polity than throwing the rascals out, there would be no real representation and accountability in the political system, for there would be no direction for policy formulation and action. In democratic theory, representation requires mass control of the broad direction of public policy. Accountability requires that when policies have undesirable consequences voters can change *policies*, as well as leaders, through elections.

Previous work has demonstrated that substantial structure exists for presidential elections (Rabinowitz, Gurian, and MacDonald 1984; Rosenstone 1983; Schneider 1978). Our intent is to reveal the process by which structural change occurs. To do this we shall translate the theoretical structural models that have guided the work of many scholars (e.g., Aldrich 1983; MacRae and Meldrum 1960; Rabinowitz 1973; Sundquist 1973) into an explicit empirical model of change. We shall then observe how closely this process conforms to the real world. The most serious test of the structural model to date by Clubb, Flanigan, and Zingale (1980) has raised considerable doubt as to its empirical viability. Hence, the outcome of this venture is far from certain.

The Process of Structural Change

Structural change requires change in the political agenda. Many factors stimulate agenda change—changes in the external environment, introduction of new actors and interests, and changes in the economic system are some of the most obvious sources of agenda change. Hence, political systems are almost always faced with the potential for structural change. When structural change occurs, we hypothesize that change will be visible in the behavior of elites well before it becomes embedded in the mass party system. We

base this hypothesis on the assumption that the introduction of new issues is normally an elite activity and that once a new issue or new set of issues is raised, it is other elites who are initially sensitive to it. Mass publics are slow to understand, and react to, most issues, with the possible exception of some social issues. Further, even in the event that the mass public is sensitive to the new issue, the issue itself must become embedded in the party system with the two parties taking opposing positions before it becomes possible for the issue to change partisan support.

Our general concern with elites rather than the mass public is justified by the empirical literature from *The American Voter* (Campbell, et al. 1960) on, which has consistently demonstrated the low information level and weak policy interest of most voters. In addition, studies of agenda setting emphasize the role of elites as initiators of public policy (Cobb and Elder 1972; Kingdon 1984). However, in stressing the elite role we are not contending that the mass is irrelevant in determining the political agenda. Rather we maintain that mass involvement is usually in reaction to, and largely stimulated by, the activities of relevant elites. This "elite-first" perspective is consistent with all the major realignment theories. Nonetheless, the principle of elite leadership is important to establish as a base point before elaborating our model of structural change.

A Four-Stage Model

How then do we propose the process evolves from elite activity to mass partisan realignment? A four-stage process seems appropriate to us:

1. A new issue is introduced by relevant elites.
2. The issue causes change in congressional ideological coalitions or in the

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dialogue between presidential candidates.

3. If the issue has changed ideological coalitions in Congress, that change becomes visible in presidential dialogue.
4. The new ideological cleavage becomes embedded in the mass-party system.

In considering this four-step model, we should stress that this is not a description of how a "typical" issue operates, but rather how a *realigning* issue (or set of issues) operates, in the political system.²

The first stage is generic to virtually any political issue; thus, it is not of particular consequence. The second stage is more critical. It implies that the issue is sufficiently powerful to redefine ideological coalitions. Central to the theory we are proposing is the idea that at any given time elites are divided into two basic ideological camps which serve to define the meaning of ideology at that point in time. The meaning of ideological labels is thus allowed to evolve, but the general ideological structure of behavior persists. This view is very similar to Schattschneider's (1960) description of political competition in the United States. Converse's (1964) discussion of the role of ideology for elites and the recent congressional analyses of Poole and Daniels (1985) and Poole and Rosenthal (1985b) are also consistent with the assumption that the behavior of elites is structured along a clear ideological dimension.

The third stage, presidential linkage, is necessary for the issue to have mass impact. The only two institutional settings in the United States that are national in scope and partisan in character are the presidency and the Congress. Therefore, only by appearing in congressional debate or presidential dialogue does an issue have the national forum required to influence the party system. Yet while Congress will often be critical in formulating and debating an issue, congressional debate alone is not sufficiently visible to stimu-

late a mass realignment. We hypothesize that only issues focused by presidential campaigns possess the saliency needed to translate both the partisanship of the conflict and often the actual substance of the issue to the mass public.

Lastly, in the fourth stage, as a consequence of the salience of presidential elections and the centrality of the presidency in the political system, the change becomes manifest in party support at the subpresidential level. Often this process will be slow because local party representatives may present ideological stands that are quite different from presidential candidates, thus blurring the issue message or making it appear idiosyncratic to a particular candidate. But eventually, with sufficient repetition, the ideological cues influence mass party identification. Thus, if our theory is correct, party realignment is the final stage in a process of change that should be visible earlier in other institutional arenas.

The main thrust of this theory is the idea that change in the ideological cleavage among elites becomes manifest first in presidential dialogue and subsequently in the structure of support for the two major parties. Our next task is to translate these dynamics to the spatial theory of realignment. Spatial models usually consider a single active axis as presented earlier. However, according to the theory we propose, at any given time one can imagine three active axes: (1) a congressional axis representing the current ideological cleavage in the Congress, (2) a presidential axis associated with voting in presidential elections (this is the conventional active axis), and (3) a subpresidential axis associated with voting in elections below the presidential level. The subpresidential axis is our best approximation to current partisan support.

A political system in complete equilibrium would have these three axes in exact correspondence. A political system in which there is disparity between the three

Figure 5. Structural Change from One Equilibrium to Another, Panel A

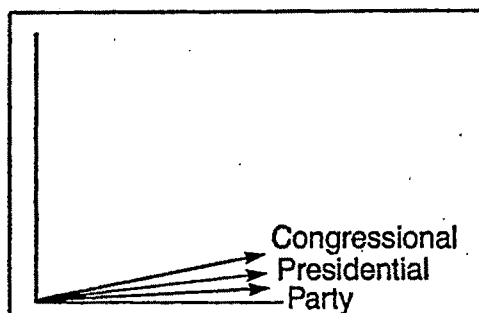


Figure 6. Structural Change from One Equilibrium to Another, Panel B

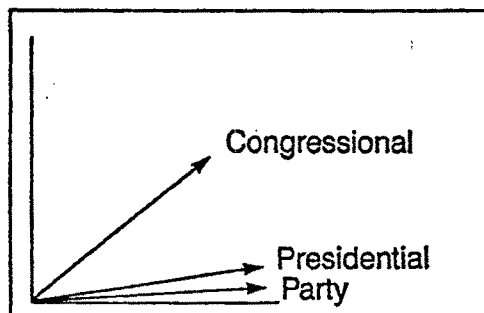


Figure 7. Structural Change from One Equilibrium to Another, Panel C

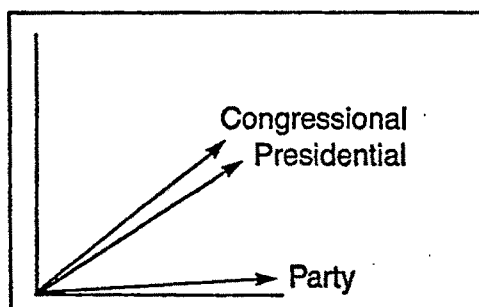
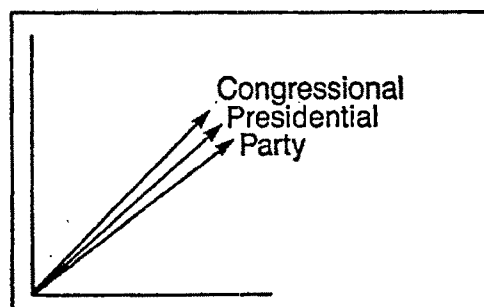


Figure 8. Structural Change from One Equilibrium to Another, Panel D



axes is in political disequilibrium and should have potential for change. Given the central role of agenda change to structural realignment, the leading edge of the realignment should be either the congressional or the presidential axis. Because mass party change depends upon the visibility of presidential elections, movement of the party axis should follow that of the presidential axis in the space. Figures 5-8 illustrate this theory.

The equilibrium condition is illustrated in Figure 5. In that figure all three axes are oriented in roughly the same direction. In Figure 6 the congressional axis has been altered by the introduction of a new element to the political agenda. Figure 7

shows the situation in a subsequent election in which the presidential dialogue reflects the same agenda change, and Figure 8 shows the system in a new equilibrium state in which the party axis has taken the same general orientation as the two preceding axes. We show the congressional axis as the leading edge of the realignment. The theory also allows the initial movement to flow from change in the presidential dialogue. The party axis, however, is never predicted to lead and is always predicted to follow the movement of the presidential axis.

The implications of the theory are striking. First, we are proposing that there is a great deal of overall coherence to the

behavior of the U.S. political system—that the behavior of the Congress, voting in presidential elections, and mass party preferences are all organized in a common spatial-ideological framework. Second, we are proposing that these three political institutions are integrated so that change in any one is intimately related to change in the other two. Third, we are proposing that the two more directly ideological elements—congressional roll-call behavior and presidential elections—drive the transformation of the party system.

Empirical Analysis of the Structural-Change Model

In order to test this theory, we need to use an appropriate analytic unit. Because the analysis is historical in scope, it requires units with stability over time. In the United States, states are the natural units for such analysis. Unlike counties, states are stable over time and are politically meaningful. States choose senators and presidential electors, and all members of the House of Representatives are elected within states. Furthermore, there is considerable variation in political attitudes across state aggregates which tends to reflect political divisions within the society.

Our theory suggests that the behavior of Congress, mass voting in presidential elections, and individual partisanship are linked in a coherent spatial framework. We shall attempt to assess the overall coherence of these three elements by locating axes representing congressional behavior, presidential voting, and partisanship in a common space. We will investigate the period 1920-1984. The selection of this particular period is largely a matter of convenience, but it does have some historical integrity, starting with the end of the First World War and including the Great Depression, the Second World War, and the postwar era.

Data and Measures

The three variables we need to operationalize are *congressional behavior*, *mass voting in presidential elections*, and *partisanship*, in each case aggregated to the state level. The *congressional behavior* score for each state is determined by the roll-call behavior of the state congressional delegation. The first step in calculating the measure is to assess the position of each member of the House and Senate on the principal dimension of conflict in the appropriate chamber during each congressional year or session. These positions are calculated using the Poole and Rosenthal's (1985a) scaling procedure. This method recovers a single value for each congressman, locating that congressman on the main conflict dimension in each year or session.³ The state mean is formed by averaging the scores for all congressmen in the state. These state means are then averaged within a presidential election cycle to obtain a single score for each state for each election cycle between 1920 and 1984. The congressional score for 1940, for instance, is thus the average of the mean positions of the congressional delegations in the 1937-38 and 1939-40 Congresses. The score reflects the ideological position of the state delegation on the issues debated over the four-year period.

Presidential voting is measured as the percentage Democratic of the vote in each presidential election. The spatial theory underlying the model requires that competition take place between two candidates. Hence, in years in which there is a significant third-party vote, that vote is distributed between the two major parties.⁴

The *partisanship* variable uses the David (1972) composite-B measure of party strength in the state. The composite is based on voting in gubernatorial, House, and Senate races, and is simply the average of the three components. In the analysis we use only off- (non-

Table 1. Summary of Principal Component Analysis

Year	Congressional Roll Call			Presidential Voting			Subpresidential Voting		
	Dimension 1	Dimension 2	100 × Community	Dimension 1	Dimension 2	100 × Community	Dimension 1	Dimension 2	100 × Community
1920	.88	-.24	83.2	.89	-.27	86.8	.94	-.18	91.6
1924	.87	-.27	84.0	.89	-.32	90.0	.90	-.32	86.2
1928	.79	-.30	71.8	.80	-.04	64.0	.92	-.21	89.6
1932	.76	-.31	66.8	.88	-.31	87.6	.92	-.16	87.2
1936	.63	-.34	51.7	.90	-.26	88.3	.90	-.16	83.3
1940	.76	-.11	59.4	.95	-.13	91.5	.94	-.22	93.1
1944	.74	.04	54.6	.95	-.06	89.9	.93	-.19	90.4
1948	.67	.23	50.4	.71	.14	53.1	.96	-.17	94.4
1952	.49	.50	50.1	.85	.26	79.1	.97	-.10	94.6
1956	.72	.43	70.5	.74	.10	56.4	.95	-.08	91.6
1960	.16	.63	42.3	.56	.63	71.1	.93	-.07	87.9
1964	.06	.74	55.6	-.57	.65	75.3	.90	.00	81.6
1968	-.39	.81	80.0	-.16	.88	80.8	.85	.06	72.1
1972	-.44	.79	82.5	-.53	.69	77.1	.74	.15	55.3
1976	-.39	.80	78.8	.63	.50	63.6	.76	.25	63.6
1980	-.22	.80	68.4	.43	.66	61.9	.76	.30	65.9
1984	-.11	.82	67.7	.04	.81	66.5	.65	.30	50.7

presidential-election-) year indexes to avoid any potential contamination with presidential-election results. Thus, we have one measure of party strength for each four-year presidential election cycle calculated on the basis of subpresidential voting in the even year preceding the presidential election.⁵ For example, the party score for 1940 is based on voting in 1938.

Factor Analysis

Our coherence argument suggests that congressional roll-call behavior, presidential voting, and partisanship should be representable in a common spatial structure. In order to test this proposition, we shall perform a factor analysis of these variables over the time frame of the study. The use of factor analysis for this purpose is theoretically appropriate based on the *individual-level* spatial model (Rabinowitz, Gurian, and Macdonald 1984).

Results

There is considerable coherence to U.S. politics over this time frame. The results

of the principal component analysis show a two-dimensional pattern with the first component accounting for over 56% of the variance and the second accounting for over 17%. The two factors together account for over 74% of the variance in the data. A third factor would account for only an additional 5.5% of the variance, while a fourth would account for 3.6%. The factor coefficients and the communalities (percentage of variance explained) for the variables are summarized in Table 1.⁶

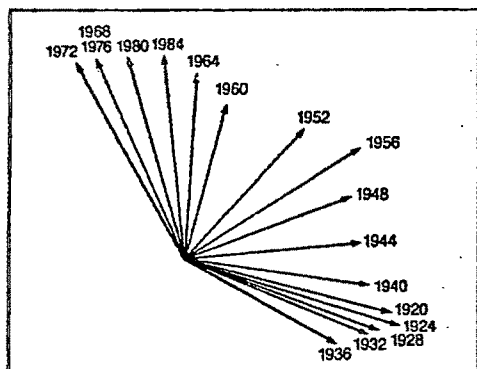
The process of structural realignment is only of interest if considerable structure exists in U.S. politics. The high communalities support that contention. The fact that the policy-making behavior of elites and the voting behavior of the mass fit so well into the same spatial structure is striking confirmation of the integration of these political institutions and is consistent with the ideological structure we hypothesized.

Visual Display

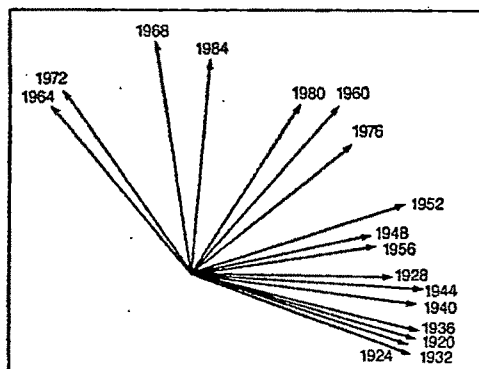
The factor analysis provides information that allows us to display the axes

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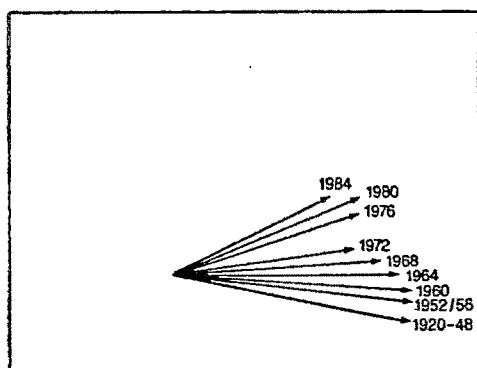
**Figure 9. Axis Orientations:
Congressional Roll Call**



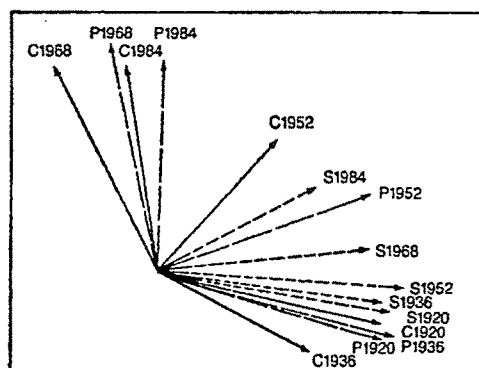
**Figure 10. Axis Orientations:
Presidential Voting**



**Figure 11. Axis Orientations:
Subpresidential Voting**



**Figure 12. Axis Orientations:
Combined**



spatially over time. Such a display has a heuristic advantage which we shall exploit before turning to a dynamic model of change. The visual display of axis movement shows how the axes have changed orientation from 1920 to 1984 and the relative equilibrium and disequilibrium in the system.

In Figures 9-12, we show the position of each set of axes over time. In Figure 9, we show the position of the congressional roll-call axes, in Figure 10 the presidential-election axes, and in Figure 11 the party axes. These three sets of axes are all from the same factor space; there are simply

too many axes with similar orientations to plot them all together. Figure 12 shows the positions of the three sets of axes in the common space for the years 1920, 1936, 1952, 1968, and 1984. These years were selected to include every fourth election cycle starting with 1920. The four figures together give a general sense of how these axes have changed over time. Each set of axes has moved from the lower right towards the upper left. Within that general similarity each has a distinct movement pattern.

The congressional axis, displayed in Figure 9, makes short clockwise moves

from 1920 to 1936 and then a consistent set of counterclockwise moves from 1936 to 1976. The congressional axis then swings back modestly in 1980 and again in 1984. The movement of the axis is always quite gradual.

The path of the presidential axis, shown in Figure 10, is similar to the congressional axis except that the presidential axis makes more erratic swings. In particular, the presidential axis takes two very large jumps, the first of these in a counterclockwise direction from 1960 to 1964 and the second in a clockwise direction from 1972 to 1976.

The party axis, shown in Figure 11, has moved quite a bit less and far more slowly than either the congressional or presidential axis. The movement is always in a counterclockwise direction and is always quite modest. Indeed, the changes are so small between years that it is not possible to display all the axes, so a single axis is drawn to represent the party axes from 1920 to 1948.

The general pattern of movement of the three axes is theoretically pleasing. Even given the high communalities of the factor solution, there was no guarantee that the axes would move in a systematic pattern over time, yet that is what we observe. The smooth and consistent change in the roll-call axis is exactly what we would predict for an institution showing ideological evolution. The somewhat peripatetic movement of the presidential axis is in keeping with the more idiosyncratic candidate dependence of presidential elections. Nevertheless, the congressional and presidential axes move in rough concert and fit nicely with an ideology-driven model of change. The slow but directionally consistent reorientation of the party axis supports the theory that party change follows change in the ideological coalitions.

The equilibrium status of the system has changed considerably over the time frame. As is evident in Figure 12, the

system was in equilibrium in the early period. In both 1920 and 1936 the three axes have similar orientations. This is consistent with Clubb, Flanigan, and Zingale's (1980) and Brady's (1985) assertion that the New Deal realignment was largely performance based.⁷ By 1952 that is no longer the case. The three axes are dispersed, with the congressional axis having moved the farthest in the counterclockwise direction. The 1968 axes are the most dispersed of the set, with both the presidential and congressional axes oriented far differently from the party axis, which has changed little from its earlier position. Thus we see by the mid-1960s a system in severe disequilibrium which, according to our theory, should be ready to realign. By 1984 the positions of the axes are somewhat more in equilibrium than in 1968, but substantial disequilibrium remains, suggesting the potential for further change in the future.

Interpretation

Clearly the general movement of the axes is from the old issues and divisions of the 1920s and 1930s to those of the more modern period. However, a definitive interpretation of the spatial structure in which the axes on the lower right are identified with one critical issue cluster and those on the upper left with another is not possible. Because the change is so consistent over time, any interpretation associated with the time change could conceivably be "correct." With that caveat in place, some attempt at interpretation is appropriate.

The general pattern of movement coincides with the emergence of the civil-rights issue in U.S. national politics. Sinclair (1978) shows that civil-rights issues emerged with the 75th Congress, and it is in this 1936-40 period that we start to see the counterclockwise movement of the roll-call axis. The changing location of axes in the space, however, is

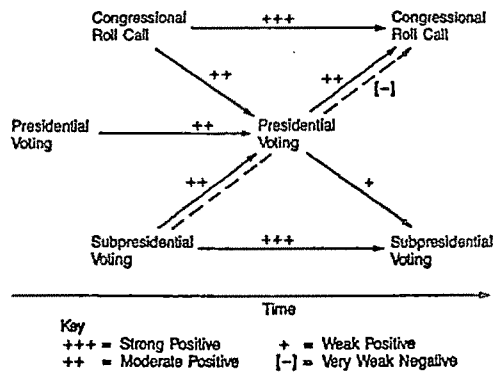
not exclusively a function of civil rights. Voting on economic issues as well as social issues determines placement on each congressional scale, and in most congresses economic votes outnumbered votes on social issues (Poole and Rosenthal 1985b). Interpreting this as a change from economic to civil rights (or social issues more generally) is, therefore, too simplistic. A better explanation is that as the civil-rights issue emerged, there was a gradual change in the ideological coalitions in the society. The role of civil rights was to redefine the ideological coalitions. Once these were redefined, elite division on the new issues, such as the economic-redistribution issues of the Great Society, tended to be consistent with the prevailing ideological cleavage in the society.

Dynamic Analysis of the Structural Model

The factor analysis is useful for demonstrating the general coherence of the U.S. political system and for providing a visual overview of political change. A dynamic analysis of the structural model is necessary, however, to test the theory of structural change we have elaborated. The model we shall analyze is displayed in Figure 13. Our hypotheses are illustrated in the figure by showing the expected magnitude of the various coefficients. The model is a natural analytic extension of the realignment model we discussed earlier, yet it is sufficiently distinct to require some further elaboration.

The three variables in the model are identical to those used in the factor analysis: congressional roll-call behavior, presidential voting, and subpresidential voting. The diagram is drawn with time increasing from left to right. So, for example, the prediction for presidential voting is based on prior presidential voting, the behavior of Congress in the four years preceding the election, and subpresidential voting in the off-year prior to the presidential election.

Figure 13. Hypothesized Relationships Based on the Structural-Change Model



The element driving the structural model of partisan change is change in the ideological debate. We hypothesized that an issue provoking such change could be manifest nationally either in the congressional debate or the debate between presidential candidates. As the issue becomes fully imbedded in the ideological structure, if first manifest in congressional debate, it would subsequently find its way into the presidential debate, and vice-versa. However, we hypothesized that partisan change would occur only when the ideological change was manifest in presidential dialogue, since only then would it be sufficiently visible to impinge on the partisan attitudes of the mass public.

The analytic model reflects this four-stage process. Presidential elections lie at the fulcrum of the model, being influenced by, and influencing, the other variables over time. The congressional behavior and subpresidential voting variables play different roles: ideology motivates change in the political system; partisanship influences voting decisions. Over the long term, we expect ideological structure as reflected in the congressional-roll-call variable to influence partisanship through the visibility of presidential elec-

Table 2. Pooled Time-Series Analysis of Structural-Change Model

Dependent Variable	Independent Variable	Estimate	Standard Error	Adjusted R ²	Auto Correlation
Congressional roll call		—	—	.74	.06
	Lagged congressional	.72	.024	—	—
	Prior presidential	.25	.026	—	—
	Prior subpresidential	-.11	.022	—	—
Presidential voting		—	—	.58	-.06
	Lagged presidential	.42	.035	—	—
	Prior congressional	.30	.030	—	—
	Prior subpresidential	.16	.028	—	—
Subpresidential voting		—	—	.81	-.25
	Lagged subpresidential	.82	.019	—	—
	Prior presidential	.14	.022	—	—
	Prior congressional	-.01*	.021	—	—

*Not statistically significant at the .01 level.

tions. Analytically, this implies an indirect effect from congressional roll calls to subpresidential voting. In contrast, partisanship should not influence congressional coalition structure even indirectly. Therefore we predict a weak, negative direct effect of partisanship on roll calls to countervail the potential indirect effect via presidential voting.

Method

We shall perform a pooled time-series analysis. This means we will combine all the observations on a variable over the entire time frame of the study. The advantage of pooling is that it makes it possible to analyze the time series for all states simultaneously. Hence, each variable will have $48 \times 17 = 816$ cases, one for each state in each time period. (Alaska and Hawaii are omitted from the entire analysis because of excessive missing data.) An example of a case would be Minnesota in 1940. The values of the variables are standardized by election cycle. Standardization is appropriate because the structural model predicts *relative* strength or weakness in a particular election. We have used ordinary-least-squares

(OLS) regression for the estimation of the models in the main body of the text. Supplementary results examining the findings under a variety of methodological controls and supporting the conclusions drawn in the text are presented in the Appendix.

Results

The results reported in Table 2 are nicely consistent with the hypothesized structural-realignment model. Congressional roll-call behavior is a function of both prior roll-call behavior and presidential voting behavior. Both coefficients are substantial, with the lagged effect strong indeed. A weak, but statistically significant, negative effect of partisanship on congressional voting does exist, as predicted.

All three elements contribute to explaining presidential voting behavior. The estimated impact of congressional behavior is twice that of the subpresidential party variable. That the roll-call behavior of Congress proves a signifi-

cantly better predictor of mass presidential voting than the mass public's own subpresidential voting is impressive. This result implies that congressional delegations are representative of their states and U.S. presidential elections quite sensitive to ideological differences between candidates.

The effect of party on presidential voting is weaker than we had anticipated. This could be caused by the presence of the lagged presidential variable in the equation predicting presidential elections. In order to determine if this is the case, we can reestimate the equation eliminating this variable. The results are similar. The subpresidential party coefficient increases to .324, but that for congressional behavior increases as well to .513. Thus, while the revised analysis enhances the absolute value of the party effect, it still leaves party with substantially less impact than ideology. This suggests that party is not as generically important to presidential voting as we had hypothesized. However, the importance of party could well be dependent on conditions influencing the political system at the time of the election; we shall consider that possibility shortly.

Subpresidential voting is the most stable of the variables in the model. This is in keeping with the known stability of partisanship. As we hypothesized, there is a direct effect of presidential voting, and only an indirect effect of congressional behavior, on partisanship. Ideology stimulates changes in mass partisanship through presidential elections and the cues they provide.

Overall, these results are quite consistent with the process we outlined. Change is motivated by the policy dynamic of congressional activity and presidential elections. The evolution of the mass party system is slow and depends upon the new ideological coalitions among elites becoming visible to the electorate through the dynamic of presidential campaigns.

Equilibrium, Disequilibrium, and Dealignment

The concept of equilibrium is important to our understanding of the process of change and its political consequences. Earlier, we defined a political system to be in equilibrium when the three axes representing congressional behavior, voting in presidential elections, and subpresidential voting are oriented in the same direction. Such a structural equilibrium prevailed in U.S. politics from 1920 through 1944. During that time congressional roll-call behavior, mass presidential voting, and subpresidential voting had generally similar orientations. From 1948 on, the axes moved to increasing disequilibrium until 1964, when the three axes were widely separated. Since 1964, the level of disequilibrium has tended to decrease. In 1984 the system was closer to equilibrium than it had been in some time, but was still far from the 1920 to 1944 level of equilibrium.

The concept of equilibrium has particular relevance for the issue of dealignment, the tendency of the public to drift away from the political parties. In his elegant analysis of political change in the twentieth century, Burnham (1970) describes the breakup of the strong party system of the late nineteenth century during the Progressive Era. Progressive reforms, such as the decrease in power of party leaders in the legislature and the advent of the primary as a method of nomination, have generally been reinforced by changes since that time. Wattenberg (1984) stresses the declining relevance of partisanship when candidates can present strong images directly to voters through television. The implication of these explanations of dealignment is that the role of parties will decline consistently over time.

The logic of the structural model implies a different pattern—one in which party will play a more meaningful role

under equilibrium than disequilibrium conditions. When the political system is in equilibrium, parties are in a good position to satisfy their representation and accountability functions. The fundamental role of partisanship in the electoral system is to serve as an efficient guide in selecting candidates. When the system is in equilibrium, voters can come to know the general positions and performance of parties and rely on these images. Information costs associated with voting on the basis of issues are thereby reduced. Party also provides continuity by linking candidates under common labels. The concept of party identification reflects the idea that people become sufficiently attached to one party or the other so as to think of themselves as Democrats or Republicans.

In periods when there is a divergence between the ideological and partisan coalitions—when the political system is in disequilibrium—we would expect the party system to be under stress. The factors that led to the current party structure are no longer operating among elites. Thus, current identifications in the mass may well run contrary to current ideological preferences. This creates an environment in which established partisanship is a less valuable guide to selecting candidates. Rather than images of behavior that a voter can count on, the party images built up over time are inapplicable to current real-world politics. Hence, a system in disequilibrium is a system in which the individual should rely more on ideological cues and less on partisanship.

The structural theory suggests that the party system will gradually respond to ideological pressure and realign. Once the system moves toward a new equilibrium, partisanship should be able to reassert itself. We propose, therefore, that the equilibrium status of the system influences the vitality of the political parties. If this hypothesis is correct, we should find that the tendency of individuals to use party in making voting decisions and to

identify with parties is dependent upon equilibrium level. In particular, we expect that as the level of equilibrium in the system increases, (1) the more useful partisanship is as a voting cue and (2) the more tempting it is to identify with parties. The first hypothesis follows from the fact that party is only a meaningful social-choice mechanism when it conveys information about the issue agenda that will be pursued by party representatives. The closer the system to equilibrium, the more readily citizens can rely on party cues to provide them a handy and meaningful guide when making voting decisions. The second hypothesis follows directly from the first: the more meaningful the party label, the more reasonable it becomes to identify with a political party.

In order to test these hypotheses, an operational measure of equilibrium is necessary. We propose to use the average of the correlations between congressional roll-call voting, voting in presidential elections, and voting in subpresidential elections in any given period as the measure of equilibrium.⁸ A direct relationship exists between this measure and the graphic concept of common orientation of axes. Correlations measure the cosine of the angle between the variables when they are displayed spatially. The cosine will equal 1.0 when the two variables are oriented in exactly the same direction and will equal zero when they are oriented at right angles. A negative correlation means the variables are separated by more than a 90-degree angle. Thus, the average correlation is a measure of the average angular separation between the three variables.

Use of Party as a Cue

We wish to test the extent to which party is used as a voting cue under different conditions of equilibrium. In order to do this, we need to incorporate the equilibrium concept into our model of

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Table 3. Analysis of Interactive Equilibrium Model

Dependent Variable	Independent Variable	Estimate	Standard Error	Adjusted R ²	Auto Correlation
Presidential voting		—	—	.58	-.07
	Lagged presidential	.37	.038	—	—
	Prior congressional	.36	.062	—	—
	Congressional × equilibrium	-.16*	.105	—	—
	Prior subpresidential	.00*	.054	—	—
	Subpresidential × equilibrium	.38	.110	—	—

*Not statistically significant at the .01 level.

presidential voting. We hypothesize that the effect of party should increase as the system moves toward equilibrium, while at the same time the effect of ideology should decrease. These effects should occur because party is an easier cue to use than ideology because it involves little information cost, but it only serves its purpose fully if it conveys meaningful ideological information.

Because the effect of party should increase as a function of equilibrium, we need to include an equilibrium × party term in the model. We predict that this interactive term will have a positive coefficient, signifying that party has increasing impact with rising equilibrium. Similarly, we shall include an equilibrium × ideology term in the model to allow for the possibility that the use of ideology declines when there is less need to pay the information costs associated with ideological voting. We shall analyze an extension of the presidential-voting model presented earlier to which we have added two terms: (1) equilibrium level × subpresidential voting and (2) equilibrium level × congressional roll-call behavior.

The results from the interactive model are displayed in Table 3 and support the hypothesis. Both interactive terms have the expected direction of impact, and the coefficient for party is both statistically significant and substantial. The coefficients are most easily interpreted if we consider the two extreme conditions of

complete equilibrium on the one hand and complete disequilibrium on the other. Under the condition of complete disequilibrium, the effects of both party (subpresidential voting) and ideology (roll-call behavior) are simply the noninteractive coefficients because the interactive terms are equal to zero. In this instance ideology has a coefficient of .36 while party has a coefficient of zero, which implies no effect for party whatsoever. In contrast, under the condition of perfect equilibrium where the equilibrium value is 1.0, the effective coefficient for both party and ideology is the sum of the noninteractive coefficient plus the interactive coefficient. Under the condition of complete equilibrium, the effective ideology coefficient diminishes to .20 (.36 + .16), while that for party jumps to .38 (.00 + .38). The evidence strongly supports the view that when the political system is in equilibrium, party is a substantially more valuable cue to individuals in making their voting decision.

Party Identification

The second equilibrium hypothesis we propose is that people identify more readily with political parties when the system is in equilibrium rather than disequilibrium. This hypothesis depends on the view that people will find identification meaningful when parties provide clear linkages to issues. In order to test the hypothesis, we formulate a model that

**Table 4. Effect of Equilibrium Level on
Percentage Identifying as Independent (1954-1986)**

Variables and Statistics	Estimate	Standardized Estimate	Standard Error
Basic model			
Percentage independent			
Constant	10.04	—	3.43
Prior % independent	.76	.80	.10
Equilibrium level	-8.83	-.29	3.19
Auto correlation	-.32	—	—
Adjusted R ²	.83	—	—
Number of cases	16	—	—
Extended model			
Percentage independent			
Constant	10.15*	—	3.96
Prior % independent	.69	.72	.17
Equilibrium level	-9.42	-.31	3.04
Off-year dummy	2.03*	.17	1.15
Time	.14*	.11	.22
Auto correlation	-.15	—	—
Adjusted R ²	.86	—	—
Number of cases	16	—	—

*Not statistically significant at the .01 level.

predicts changes in the proportion of political independents—those who identify with neither party—as a function of equilibrium level. Our hypothesis is that as the system moves more into a condition of disequilibrium, people will increasingly be drawn to independent status, while as the system moves toward equilibrium, the percentage of independents should decline.

The model we shall analyze is the following: Percentage independent (time t) = $b_0 + b_1$ percentage independent (time $t - 1$) + b_2 equilibrium level. To analyze this model, we require data on the percentage of the electorate who call themselves independents. Good measures of partisanship are available from 1952 to 1984 using the University of Michigan national-election studies. Hence, our time frame is more restricted for this analysis.

The analysis predicts the percentage independent of the total electorate in each on and off election year from 1954 through 1984. We also show in Table 4 the estimation of a model that includes both a dummy variable to distinguish on and off year elections and a time variable that ranges from 1 in 1954 to 16 in 1984. The time variable is included to ensure that the estimated coefficients are not an artifact caused by a general tendency for individuals to identify less with parties over this period.

The results support the importance of equilibrium level in explaining trends in identification over this period. The coefficient for equilibrium level is -8.8, indicating that under conditions of complete disequilibrium there will be approximately 9.0% more independents than under conditions of complete equilib-

rium.⁹ Thus we find that individual decisions to identify with political parties are sensitive to the congruence between partisan and ideological preferences.

Our work suggests that dealignment is in part explicable in simple spatial terms. Much of the classic Burnham (1970) analysis rests on institutional impacts, such as the decline in the power of the Speaker and the loss of party control of nominations. Our analysis suggests that some dealignment is to be expected when the contemporary issue debate is less rooted in the party system, that is, when disequilibrium occurs. The spatial-ideological explanation is based on non-institutional considerations but does not rule out the possibility that relationships exist between institutional and spatial factors. Indeed, some of the institutional revitalization of parties noted by Cotter et al. (1984) and Schlesinger (1985) has occurred as party and ideology have moved back towards equilibrium, suggesting there is some real interplay between institutional and spatial characteristics of the political system.

The relationship between political change, equilibrium level, and partisanship is useful for explicating a paradox in recent political behavior. As the parties have become increasingly easy to identify with issues, partisanship has declined. Thus, it appears that strong issue positions undercut partisanship. Yet theory implies that partisanship should be most excited in times of high issue debate.¹⁰ The analysis we have done resolves the paradox by showing that the effect is exactly as theory suggests *except* that substantial time lags are involved. In the initial period, clear issue stands disequilibrate the system; only later as people are organized around the new ideological cleavage does partisanship become revitalized in the mass. In times of very high issue intensity, such as the Civil War period, the time lags are likely to be substantially reduced, but the general process is likely to be similar.

Conclusion

Governments render decisions as to how resources and values are allocated in a society. In the United States, Congress is the institution in which most of the key allocating decisions are made. To the extent the U.S. political system is integrated, the coalitions that form around the issues debated in the Congress should be reflected in the coalitions that support presidential candidates and those that support the major political parties. Previous work has demonstrated that such structure exists for presidential elections. The work we have done suggests that the structure extends to the behavior of Congress and the behavior of the mass public in its subpresidential voting as well. All three elements of the political system can be nicely represented in a single two-dimensional space from 1920 to 1984. The results show a system with considerable coherence, where the policy decisions made by the congressional elite are closely tied to the electoral system in which that elite operates. Indeed, we have described a system in which change in any component is intimately related to change in the other components.

The most impressive relationship among the three elements in the system is that between congressional behavior and presidential voting. The dynamic of this relationship relies directly on the idea that there is a prevailing ideological climate in which political conflict is structured. Our view is that congressional roll-call behavior is the clearest institutional manifestation of that ideological climate. As such, it is striking to see its strong historical role in foreshadowing presidential-electoral and partisan coalitions.

Presidential elections are visible nationwide contests that can, at least on occasion, present the substance of ideological debate to the mass electorate and, given the centrality of the presidential office, at

times recast that debate. We see evidence for both these roles in our results.

The role of partisanship follows the theory we laid out at the onset. Partisan coalitions change in response to changing ideological coalitions. Partisanship is a derivative effect of the ideological debate in the sense that it initiates no change but only responds to it. As a vehicle for stimulating choice between presidential candidates, partisanship has less weight than we originally anticipated. However, we have seen that the impact of partisanship is enhanced when the political system is in equilibrium.

In studying change, the idea of systemic equilibrium is critical. When ideology separates from partisanship, the political system moves into disequilibrium and a period of realignment is initiated. In our view, dealignment is at least in part a consequence of system disequilibrium. The equilibrium theory predicts both increases and decreases in the importance of partisanship and the vitality of the parties over time. When the political system is in equilibrium, people make greater use of party cues and identify more readily with political parties; under conditions of disequilibrium, partisanship loses meaning and importance.

The structural integrity of U.S. politics appears in stark contrast to the volatility of specific elections and votes. Each election has a unique context of candidates and times; each roll-call vote conjures forth particular interests and motivations. Yet this complex mix of individuals and events is set in an enduring ideological structure that evolves in a very systematic way. We have presented a paradigm for understanding this process of structural change.

Appendix

The analysis of pooled time-series data presents unusual methodological difficul-

ties (Stimson 1985). We have consistently used OLS to estimate the various models. While we have reported standard errors and significance levels, these should be viewed with caution since they are based on the assumptions of no autocorrelation and independence of cases, assumptions not satisfied in our analysis. Furthermore, the inclusion of a lagged dependent variable as a predictor in the equations introduces some danger that estimates may be biased as well as inefficient. The methodological problem is so complex that no "perfect" solution exists. Nevertheless, a reasonable attack on the problem is possible. Our main concern is whether the critical results we have obtained hold up under additional scrutiny. To this end we have performed three additional analyses.

First, we have reestimated the four models using two-stage least squares to see if there is any substantial change. The two-stage procedure involves estimating each lagged dependent variable as a function of the exogenous variables and the exogenous variables lagged (which are the instruments). The estimated variable then replaces the lagged dependent variable in the analysis. The purpose of the two-stage analysis is to see if the potential bias caused by the inclusion of the lagged dependent variable has distorted the estimates. Most commonly, the inclusion of a lagged dependent variable will cause the impact of the variable to be exaggerated at the expense of the other independent variables. The results of the two-stage analysis are reported in the second column of Table 5; the first column reproduces the OLS results reported in the text to facilitate comparisons.

The OLS and two-stage coefficients are quite similar in the presidential-voting and subpresidential-voting models, though the two-stage results are not statistically significant in the presidential-voting models. Because the lack of significance is caused by high collinearity in the

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two-stage model rather than diminution of estimated effects, it is not a matter of concern.¹¹ Multicollinearity occurs frequently in two-stage estimation and is one reason to prefer OLS estimates when the two-stage results are similar. The congressional roll-call model is the only model that shows deterioration in the lagged co-

efficient, but even in this instance there is no change in the substantive interpretation. Thus, the analytic findings are preserved well in the two-stage analysis.

Our second attack on the problem of pooled time-series data is to introduce a dummy variable and an explicit time-dependent function for each state. By in-

Table 5. Summary of Regression Results Using State Dummies and Aggressively Controlling for Time Dependence

Variables and Statistics	Text Result (1)	Two-Stage Estimate (2)	State Dummies (3)	Model 3 + Linear Time (4)	Model 4 + Nonlinear Time (5)	Model 5 + Auto Regression Coefficient (6)
Congressional roll call						
Lagged congressional	.72	.59	.63	.44	.28	—
Prior presidential	.25	.33	.30	.27	.28	.29
Prior subpresidential	-.11	-.11	-.03*	-.05*	.02*	.07*
Autoregression coefficient	—	—	—	—	—	.21
Adjusted R ²	.74	.62	.74	.76	.79	—
Auto correlation	.06	.19	.06	.10	.05	—
Presidential voting						
Lagged presidential	.42	.41*	.30	.09	.05*	—
Prior congressional	.30	.31*	.39	.30	.31	.32
Prior subpresidential	.16	.16*	.00*	-.08*	-.07*	-.07*
Autoregression coefficient	—	—	—	—	—	.04*
Adjusted R ²	.58	.52	.60	.64	.64	—
Auto correlation	-.06	-.05	-.04	.01	.00	—
Subpresidential voting						
Lagged subpresidential	.82	.83	.41	.20	.01*	—
Prior presidential	.14	.13	.09	.07	.07	.07
Prior congressional	-.01*	-.01*	.05*	.02*	.04*	.04*
Autoregression coefficient	—	—	—	—	—	-.00*
Adjusted R ²	.81	.67	.83	.85	.87	—
Auto correlation	-.25	-.26	-.04	-.00	-.01	—
Equilibrium model of presidential voting						
Lagged presidential	.37	.52*	.22	.03*	-.03*	—
Prior congressional	.36	.24*	.47	.42	.45	.44
Prior congressional × equilibrium	-.16*	-.03*	-.24*	-.27*	-.32	-.33
Prior subpresidential	.00*	.03*	-.20	-.28	-.30	-.34
Prior subpresidential × equilibrium	.38	.18*	.53	.52	.61	.70
Autoregression coefficient	—	—	—	—	—	-.06*
Adjusted R ²	.58	.54	.61	.65	.65	—
Auto correlation	-.07	-.17	-.06	-.03	-.04	—

*Not statistically significant at the .01 level.

roducing the dummy variables we will control for case repetition effects. When each state is represented by a distinct dummy variable, we expect a marked reduction in the regression coefficients that depend on state differences consistent over time. Party effects should be most sensitive to this. By introducing a set of time-dependent functions into the models, we will control for general trending effects. If our results follow a well defined trend—even one generated by the process we have outlined, these effects will be suppressed in the analysis. The control for trend should be particularly consequential for the lagged dependent variables since they should follow the same over-time pattern as the dependent variables themselves. We performed three additional regressions: (1) adding a dummy variable for each state; (2) adding the state dummy and a state dummy \times time variable; and (3) adding the state dummy, the state dummy \times time, and a state dummy \times time-squared variable to control for non-linear as well as linear time effects. The results of these analyses are presented in columns 3-5 of Table 5. We do not show the myriad of state coefficients we estimated; they are available from the authors upon request.

The results provide strong support for our principal findings. As expected, the lagged effect of each of the variables is radically diminished with the state and time controls. Nevertheless, there is remarkable consistency for the main effects discussed in the text. The critical coefficients from the analysis of the structural-change model are (1) the effect of presidential voting on congressional roll-call coalitions, (2) the effect of congressional roll-call coalitions on presidential voting, and (3) the effect of presidential voting on subpresidential or party voting. The critical results from the equilibrium analysis are the effects of subpresidential voting and congressional roll-call behavior under the condition of equilib-

rium in comparison to the effects under disequilibrium. These relationships hold up quite nicely.

The third attack on the problem is to perform an autoregressive analysis on the fully controlled model (Model 5).¹² We do this to ensure that the results obtained are meaningful when the autoregressive behavior of the residuals is taken into account in the estimation. The analysis requires the removal of the lagged dependent variable; this is possible in the fully controlled model because the lagged effect should either be incorporated in the time trend or be autoregressive in character. Thus, the models we analyze using this method include the standard predictor variables (except for the lagged dependent variable), the state dummy, the state dummy \times time, and the state dummy \times time-squared variables, as well as a one-lag autoregressive estimate. The results are reported in the last column of Table 5. They are quite similar to those in the previous column, again supporting the general consistency of the findings.

Notes

We are indebted to Keith Poole and Howard Rosenthal for generously sharing their roll-calling results with us; without their cooperation, this research would not have been possible. We also wish to thank Leon Fink, Susan Levine, Duncan MacRae, and the Comparative Politics Discussion Group at the University of North Carolina for helpful comments. A preliminary version of this paper was presented at the 1985 meetings of the American Political Science Association, New Orleans.

1. In a strict sense, cutting-line shifts can also be induced by a candidate taking a relatively extreme position. In general, performance-based shifts seem nonspatial in character; however, recent extensions of the spatial model have attempted to incorporate them. A good introduction to the spatial theory of voting is Enelow and Hinich (1984).

2. Carmines and Stimson (1986) present a psychologically oriented model nicely consistent with the model we propose.

3. The calculation of scores for each of the congressmen from 1919 through 1984 was performed by

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Poole and Rosenthal and shared with us. The calculation was done annually when a sufficient number of roll calls occurred in a year; otherwise the scaling was done by session. Poole and Rosenthal (1985b) present a detailed report of the analysis in which these coefficients were calculated.

4. Elsewhere (Rabinowitz, Gurian, and Macdonald 1984) a detailed description of the division of third-party votes from 1944 to 1980 is presented. Among those elections only 1948 and 1968 had significant third-party voting. In the pre-1944 period, the only election with a substantial third-party vote was 1924, where the progressive candidate, Robert LaFollette, won 16.6% of the total vote. The LaFollette vote was divided on the basis of a regression using the 1916 Democratic vote as the dependent variable and the 1924 Democratic and LaFollette votes as the independent variables. On this basis the LaFollette vote was divided approximately 4 to 1 for the Democratic party. The data source for the presidential vote was ICPSR Study No. 0019, *State Level Election Returns (1824-1972)*; these data were originally collected by Walter Dean Burnham, Jerome M. Clubb, and William H. Flanigan. The source for each of the post-1972 presidential votes was various issues of the *Congressional Quarterly Weekly Report*.

5. A description of the data sources for the David coefficient from 1942 to 1978 appears in Rabinowitz, Gurian, and Macdonald 1984. The coefficients from 1918 through 1938 were obtained from David 1972; the 1982 coefficients were calculated based on reports in the *Congressional Quarterly Weekly Report* and the *Statistical Abstract of the United States* 1984.

6. Each of the eigenvalues for the first four factors exceeds one; given that 51 variables are being analyzed, this result is to be anticipated. The ratios in variance explained across factors (from the first and second through the fourth and fifth) are 3.19, 3.21, 1.57, and 1.37. This pattern is quite consistent with a two-dimensional interpretation.

7. This does not rule out the possibility that some structural change occurred during the New Deal period. Both MacRae and Meldrum (1960) and Erbring, Nie, and Hamburg (1986) find evidence for structural change in fine-grained analyses of Illinois politics. Our own analysis shows somewhat lower levels of equilibrium in 1932 and 1936 than in the early 1920s or the 1940s, but these differences are modest.

8. In the analysis of "party as a cue" reported below we use the average correlation between the congressional, subpresidential, and prior presidential voting variables as the equilibrium measure. This ensures that the equilibrium measure is not a function of current presidential voting. In the analysis of "party identification" we analyze the level of independent identification in both off and on years. Presidential voting is not a variable in the

prediction equations. The off-year correlations are calculated using the immediately prior presidential election, while the on-year correlations use the current on-year presidential election.

9. As a check on the OLS results, we performed a Yule-Walker autoregressive analysis in which the dependent variable is *change* in percentage independent. The predictor variables are equilibrium level, dummy off year, and time. The coefficient for equilibrium level in this model is -8.08 , significant at the .01 level.

10. The theoretical tension surrounding the question of issue clarity and partisan identification is illustrated in the contrast between Pomper (1972) and Nie and Anderson (1974).

11. The collinearity is caused by the fact that the lagged congressional-roll-call and subpresidential-voting variables (which serve as the instruments) are not very different from their unlagged counterparts. This makes the standard errors very high.

12. We performed a Yule-Walker analysis using the SAS autoregression procedure.

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STATE POLITICAL CULTURE AND PUBLIC OPINION

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Do the states of the United States matter (or are they of no political consequence)? Using a data set with over 50 thousand respondents, we demonstrate the influence of state political culture on partisanship and ideology. For individuals, we find that the state of residence is an important predictor of partisan and ideological identification, independent of their demographic characteristics. At the aggregate level, state culture dominates state demography as a source of state-to-state differences in opinion. In general, geographic location may be a more important source of opinion than previously thought. One indication of the importance of state culture is that state effects on partisanship and ideology account for about half of the variance in state voting in recent presidential elections.

Political scientists often assume that political attitudes are shaped by local political culture, the commonly shared and reinforced political values within the local community. But unfortunately, we know little about how geographical location influences political attitudes. *Region*, defined in broad categories like *Northeast*, *South*, *Midwest*, and *West*, seems to have a modest influence on how U.S. citizens think politically. Typically, political scientists use *region* as a surrogate for a common culture, often attributed to the unique historical, economic, or demographic composition of large areas of the nation. *Region* in this sense is used as an explanatory contextual variable in the analysis of individual-level attitudes and behaviors. Yet do regions, as broad geographic categories, adequately account for all geographical variation in political

culture within the United States? Stated differently, have important variations within regions led to unique subregional political cultures? If so, the usual regional definitions mask important differences in political culture within regions.

One intriguing possibility is that the unique political cultures of individual states exert an important influence on political attitudes. We are not alone in entertaining this possibility. A sizeable literature documents apparent differences in state political cultures (Fenton 1957, 1966; Key 1949; Lockard 1959; Patterson 1968). By necessity, though, this literature is largely impressionistic. And the comparative component is implied more than it is demonstrated. The pioneering cultural mapping begun by Daniel Elazar (Elazar 1966; Sharkansky 1970) is forthrightly comparative, but it too is largely impressionistic in its assignment of con-

temporary cultural influences. Comparative analysis of the influence of political culture in the United States has awaited adequate measurement.

We examine the effects of state political culture on the political attitudes of U.S. citizens. This task is made possible by access to an unusually large national-opinion survey. We pool multiple CBS/*New York Times* national-opinion surveys over the period 1976-82. From these surveys, two attitudinal items are available for analysis: party identification and ideological identification. These variables are measured as trichotomies: Democratic, Independent, and Republican; and liberal, moderate, and conservative. This data set is described more fully in Wright, Erikson, and McIver 1985.

Here we analyze only the subset of the CBS/*New York Times* surveys for which a full set of respondent demographic characteristics were ascertained. We examine the differing effects of 48 states, excluding Alaska and Hawaii (no interviews) and Nevada (see Wright, Erikson, and McIver 1985, 480) but with the District of Columbia as an added "state." We have 55,145 respondents for our analysis of the effect of state on party identification and 52,820 respondents for our analysis of the effect of state on ideological identification.

The research that follows is a combination of individual-level and aggregate-level analysis. For the individual-level analysis, we examine geographical units as dummy variables among a larger set of independent variables predicting individual-level responses. For the aggregate-level analysis, we examine the extent to which the observed attitudinal differences across state samples conform to state boundaries, as represented by the state dummy variables from the individual-level analysis, rather than from state-to-state differences in demographic composition.

Admittedly, *political culture* can take on a broader connotation than what we

are able to examine in this paper. For instance, Elazar (1966, 84) defines *political culture* as "the particular pattern or orientation to political action in which each political system is imbedded." State political cultures may be classified in part by attitudes of the public and politicians, the kinds of people involved in state government, and the actions of state governments. Here we examine only one aspect of state political culture: state public opinion as summarized in party identification and ideological identification. Moreover, we define *state political culture* as only that portion of state public opinion that cannot be accounted for by the group characteristics of the state electorate. Our assumption is that estimates of the contextual effects of the state of residence on partisanship and ideology reflect the effects of different state political cultures on these important attitudinal variables.

The Influence of State of Residence on Political Attitudes

Our first task is the estimation of state-level "cultural" effects from survey data. For each of the two dependent variables (party identification and ideological identification), we run four regression equations. First, we include 25 dummy variables representing the available categories on the demographic variables for the CBS/*New York Times* surveys as the independent variables. (The dummy variables represent categories on respondent's age, education, family income, religion, race, gender, and urbanism.) Second, we add three dummies representing the four major regions: Northeast, Midwest, South, and West. Third, we switch to dummies representing a more detailed regional breakdown of eight separate regions: New England, Mid-Atlantic, Great Lakes, Plains, Deep South, Border South, Mountain, and Pacific. Fourth, we

replace the region dummies with separate dummy variables for each of 48 states. At each stage our primary interest is to see how the more complex breakdown of geography improves the explanation of the political attitudes in question.

Once we reach the point where each state becomes a dummy variable, we have 72 independent variables, which, with multiple thousands of cases, can be handled easily. In fact the large data set allows the luxury of including missing data (*don't know, no answer*) for each demographic variable as its own dummy variable.

In Table 1, we show the results of the two regression equations predicting party identification and ideological identification from demographic variables alone. The regression coefficients conform to expectations. (Almost all are highly significant, as we would expect, given the large sample sizes.) Having a high income, being a Protestant, being white, being male, and living in the suburbs or rural areas all appear to result in Republicanism and conservatism. Age is also related to conservatism, but its effect on partisanship is not clear. Education is the one variable that clearly pushes partisanship and ideology in opposite directions. High education appears to promote both Republican partisanship and liberal ideological identification.

Our major interest here is in the consequences of adding dummy variables representing geographic location to the set of independent variables. Table 2 displays the statistical evidence. As can be seen, each further refinement of geographic location does add to the prediction of partisanship and ideology: Four regions add significantly to what demographics alone explain; eight regions add significantly beyond what four regions explain; and states add beyond what eight regions explain. Knowing a person's state, therefore, does add to our ability to forecast a person's partisanship or ideology beyond

what we know from the person's demographic characteristics alone or even beyond what we know from the person's demographics and region together.

Of course, the increments in the adjusted R^2 for each new refinement in the measurement of geography are minuscule. Seemingly it took 10s of thousands of cases to make states collectively a statistically significant set of predictors for political attitudes. This statistical "proof" that the state of residence helps explain political attitudes by itself does not demonstrate the substantive significance of the coefficients for the state dummy variables. Are these estimates of state-to-state differences in political culture large enough and statistically reliable enough to merit detailed attention?

In Table 3, we show the regression coefficients and standard errors for the state dummy variables as predictors of partisanship and ideology, with demographics controlled. Coefficients and standard errors are shown for each of the 48 analyzed states. In place of omitting one state to serve as the base category (the usual procedure in dummy-variable analysis), the intercept is omitted (the two procedures give equivalent equations). *Suppressing* the intercept serves two functions. It gives coefficients representing state effects that are relative to the mean respondent (more precisely, the mean respondent in the base category on each set of demographics) rather than relative to the effect of one arbitrarily chosen base state. Second, it results in standard errors that represent the error in the estimate of the state effect by itself rather than the error in the relative difference between the state effect and the effect of the arbitrarily chosen base state.¹ We will see this to be an important advantage.

The best way to evaluate the magnitudes of the estimated state effects of Table 3 is to compare them with the magnitudes of the effect of the demographic

**Table 1. Regression of Party Identification and Ideological Identification
on Selected Demographic Variables**

Demographic Variables	Party Identification		Ideological Identification	
	Regression Coefficient	Standard Error	Regression Coefficient	Standard Error
Education				
Not high-school graduate				
High-school graduate	.082**	.010	-.038**	.010
Some college	.176**	.010	-.062**	.010
College graduate	.229**	.011	-.091**	.011
Don't know, refused	.016	.050	.007	.056
Income				
Low				
Low middle	.033**	.011	.015	.010
High middle	.048**	.011	.016	.010
High	.145**	.011	.048**	.011
Don't know, refused	.109**	.018	.108**	.018
Age				
18-29				
30-44	-.042**	.009	.115**	.008
45-64	-.037**	.009	.200**	.009
65 and older	.094**	.012	.260**	.011
Don't know, refused	.121	.063	.186*	.067
Race				
White				
Black	-.540**	.013	-.161**	.013
Other minority	-.173**	.021	-.160*	.020
Don't know, refused	.166*	.054	-.091	.054
Religion				
Protestant				
Catholic	-.230**	.008	-.099**	.007
Jewish	-.504**	.019	-.444**	.019
Other religion	-.019	.021	-.061**	.021
Not religious	-.104**	.023	-.171**	.023
Don't know, refused	-.138*	.040	-.050	.041
Gender				
Male				
Female	-.029**	.007	-.038**	.006
Size of place				
Rural				
Large city	-.045**	.010	-.060**	.010
Suburbs	.037**	.008	-.041**	.008
Small city	-.016	.010	-.026*	.009
Don't know, refused	-.159	.011	.122	.110

*Significant at .001.

**Significant at .0001.

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Table 2. Significance of Regions and States as Predictors of Party Identification and Ideological Identification

Variables	Adjusted R ²	Change in Adjusted R ²	F for Change	Degrees of Freedom	Significant at .01?
Predicting party identification					
Demographic only	.0770	—	—	—	—
Demographics plus four regions	.0866	.0096	194.18	3, 55,116	yes
Demographics plus eight regions	.0898	.0032	48.33	4, 55,112	yes
Demographics plus 48 states	.0953	.0055	8.35	40, 55,072	yes
Number of cases = 55,145					
Predicting ideological identification					
Demographics only	.0396	—	—	—	—
Demographics plus four regions	.0421	.0025	46.23	3, 52,791	yes
Demographics plus eight regions	.0425	.0004	5.11	4, 52,787	yes
Demographics plus 48 states	.0444	.0019	2.67	40, 52,747	yes
Number of cases = 52,820					

categories, shown in Table 1. This comparison shows that the effect of living in one state instead of another is often of about the same magnitude as the difference between one demographic category and another. Consider, for example, the apparent effects of living in the relatively liberal and Democratic state of Minnesota rather than the relatively conservative and Republican state of Indiana. Comparing these states' relevant regression coefficients suggests that living in Minnesota instead of Indiana produces a difference of .141 on the three-point party-identification scale and a difference of .031 on the three-point ideological-identification scale. These differences are comparable to the effect of being in the highest rather than the lowest income category (see Table 1): a difference of .149 on party identification and .048 on ideological identification. In other words, the effect of living in the Indiana political culture rather than the Minnesota political cul-

ture is about the same as the effect of having a very high income instead of a very low income.

Our examples of Minnesota effects and Indiana effects are not the most extreme cases. The most Democratic state effect is estimated to be Georgia's (−.252); the most Republican state effect is estimated to be Nebraska's (.238). The coefficients of Table 1 suggest that the difference in party identification produced by the difference between the political cultures of Georgia and Nebraska is almost of the same magnitude as the difference resulting from being Jewish instead of Protestant or of being black instead of white! And remember that the state coefficients are derived from an analysis in which all major demographic variables are controlled. These differences in state effects on partisanship are not a function of the measured state demographics.

Considering ideology, Table 3 identifies West Virginia as the most liberal state

**Table 3. Effects of State Culture on Party Identification
and Ideological Identification**

State	Party Identification		Ideological Identification	
	Regression Coefficient	Standard Error	Regression Coefficient	Standard Error
Alabama	-.233	.028	.065	.028
Arizona	.047	.034	.079	.033
Arkansas	-.232	.033	-.029	.033
California	.019	.018	-.053	.017
Colorado	.057	.035	-.037	.034
Connecticut	.096	.030	-.058	.030
Delaware	.057	.069	-.017	.068
District of Columbia	-.003	.053	-.085	.053
Florida	-.018	.021	.027	.021
Georgia	-.252	.024	-.002	.024
Idaho	.202	.045	.139	.044
Illinois	.088	.020	-.013	.020
Indiana	.071	.024	.003	.023
Iowa	.098	.030	-.039	.030
Kansas	.094	.029	-.056	.028
Kentucky	-.167	.028	-.028	.028
Louisiana	-.242	.029	.079	.028
Maine	.147	.043	-.015	.042
Maryland	-.130	.026	-.032	.026
Massachusetts	-.019	.024	-.109	.024
Michigan	.092	.021	-.041	.020
Minnesota	-.070	.027	-.028	.027
Mississippi	-.171	.039	.054	.039
Missouri	-.021	.024	-.020	.024
Montana	-.009	.052	-.028	.051
Nebraska	.238	.037	-.004	.037
New Hampshire	.231	.047	.013	.046
New Jersey	.097	.023	-.087	.022
New Mexico	-.035	.046	-.011	.045
New York	.161	.019	-.040	.018
North Carolina	-.107	.027	.035	.027
North Dakota	.160	.057	.194	.055
Ohio	.065	.020	-.050	.019
Oklahoma	-.161	.032	.119	.032
Oregon	-.043	.030	-.047	.029
Pennsylvania	.107	.019	-.020	.019
Rhode Island	.025	.054	-.075	.051
South Carolina	-.012	.030	.120	.029
South Dakota	.001	.043	.038	.043
Tennessee	-.046	.026	.013	.026
Texas	-.127	.019	.091	.018
Utah	.153	.042	.169	.041
Vermont	.134	.056	-.115	.056
Virginia	.015	.024	.053	.024
Washington	-.036	.026	-.048	.026
West Virginia	-.126	.034	-.122	.034
Wisconsin	.053	.027	-.023	.026
Wyoming	-.020	.064	-.040	.064

relative to its demographic composition. The most conservative state political culture is that of North Dakota. The magnitude of the difference between these two states exceeds the maximum difference across categories on all demographic variables with the exception of religion. The evident ideological impact of living in West Virginia rather than in North Dakota exceeds by a considerable amount the effect of being Catholic versus being Protestant. But our West Virginia-versus-North Dakota effect does not quite compare in size to the ideological impact of being Jewish versus being Protestant.

Clearly, as estimated by the coefficients for state dummy variables with controls for demographics, the effects of state political culture seem to be reasonably large. But how reliable are these estimates? Each reader may wish to make his or her own assessment, comparing the state estimates with his or her expectations. To us, a few coefficients seem out of line, but most seem plausible. We can do better, however, than to compare estimates with our private intuitions. We can estimate the statistical reliabilities of the state effects.

To ascertain the reliability of the state estimates, we use the standard errors of the state effects, shown in Table 3. Each standard error is equivalent to the sampling error of the sample-based state estimate, but with the effects of demographic variables held constant.² The square of each standard error is the error variance in the estimate of the state's attitudinal effect. We use the estimates of error variance in the calculation of overall statistical reliability for the state dummy variables.

Here, we shift for the first time from the individual level to the aggregate level of analysis. For both partisanship and ideology, we wish to estimate the reliability of the 48 estimates of the effect of state culture represented by the state dummy variables. By the standard statistical

definition, the reliability of each variable is the ratio of true variance to observed variance. True variance cannot be estimated directly. But because the observed or total variance is the sum of the true variance plus the error variance, we can estimate the true variance by subtracting the error variance from the observed variance.

At the practical level, our steps are as follows: first, we square the standard errors for the state variables and obtain their mean, which serves as the estimated error variance for the state coefficients; second, we compute the observed variance as the variance of the state coefficients; third, we subtract the error variance from the observed variance to estimate the true variance across states in cultural differences; and fourth, we divide the estimated true variance by the observed total variance to estimate the reliability.

For party identification, the estimated reliability of the dummy-variable coefficients is .91. Thus, our estimates of state effects on partisanship with demographic effects removed can be accepted with considerable confidence. For ideology, the estimated reliability of the state effects is a somewhat lower value of .73. Thus, about one-fourth of the variance in these ideology coefficients can be attributed to sampling error.³

Let us review our findings so far. First, states make a statistically significant contribution to the prediction of partisanship and ideology beyond what demographics plus a detailed breakdown of region can do. Second, the estimates of specific state effects range sufficiently to compare in magnitude with the measurable effects of standard respondent demographic variables such as race, religion, income, education, and age. Finally, these coefficients for estimated state effects are quite reliable. This means that the state estimates accurately measure state-to-state differences in individual responses to

survey items about partisan and ideological identification, adjusted for the effects of the included demographic variables.

Culture versus Demography as Sources of State Opinion

So far we have examined the effects of state residence upon individual attitudes and how these effects compare with the effects of standard demographic variables upon individual attitudes. In this section, the state rather than the individual respondent is the unit of analysis. Here we try to ascertain the extent to which state-level differences in partisanship and ideology are a function of unique state cultures rather than the different group characteristics of the states' residents.

For this analysis, we decompose the observed mean party identification and the observed mean ideological identification of each state into its cultural component and its demographic component. The cultural components are our estimates of the effect of state political culture on individual attitudes: the dummy-variable coefficients of the state's impact on partisanship and ideological identification independent of demography (see Table 3). The demographic components are the average of the state's respondents' predicted responses based on demography, controlling for state effects. More precisely, the demographic components are the average of the responses predicted by the regression coefficients, but with the effects of the state dummy variables subtracted out. Tautologically, the cultural and demographic components of partisanship (and of ideological identification) add up to exactly the states' mean responses.

In Figures 1 and 2, we show the scatterplots of the states' cultural and demographic components, for partisanship and ideology respectively. The horizontal axes represent the estimated effects

of state culture: the farther right the state's position on the horizontal axis, the more Republican or the more conservative the state effect. The vertical axes represent the demographic components of state attitudes: the higher the state's position on the vertical axis, the more Republican or the more conservative is the state in terms of the demographics of its population. States to the right and above the diagonal lines in the two figures are relatively Republican (Figure 1) or relatively conservative (Figure 2). States to the left and below the diagonal line are relatively Democratic (Figure 1) or relatively liberal (Figure 2).

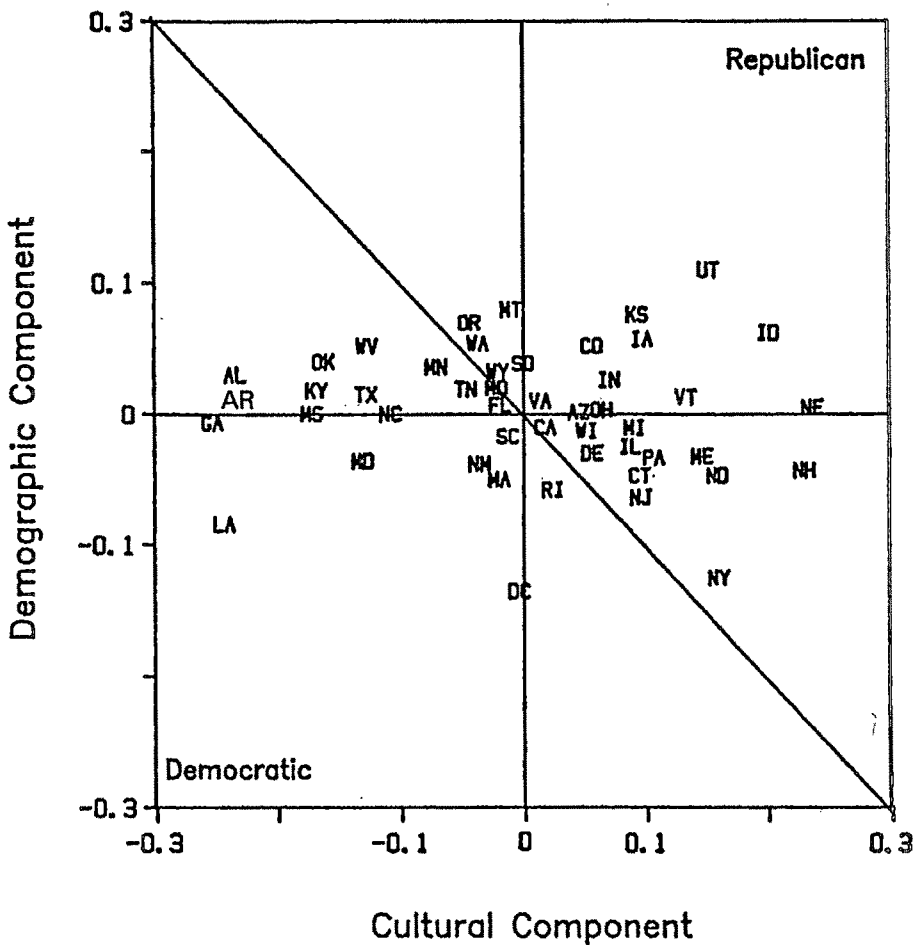
For both partisanship and ideology, the cultural component has more variance than the demographic component. For partisanship, the ratio of variances is .015 to .003. For ideology, the ratio of variances is .005 to .001. Thus the variance attributable to the cultural components of partisanship and ideological identification is five times the variance due to demography.⁴

Adjustment for measurement error modifies these estimates only slightly. We can adjust the two cultural variances, purging our estimates of the error variances from the reliability exercise. Generously, let us assume that the demographic component is measured perfectly, thus allowing a slight inflation of the variance estimate for demography. The resultant ratios still favor culture. For partisanship, the ratio is now .014 to .003, or almost 5 to 1. For ideology, the ratio is .003 to .001, or 3 to 1.

The interpretation of these findings is straightforward and intriguing. Because the cultural components have the higher ratio, most of the variance in state partisanship and state ideology is due to state-to-state differences that cannot be accounted for by the demographic variables measured here. Only a relatively small proportion of the variance in state partisanship and ideology is due to state-to-

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Figure 1. Components of State Party Identification

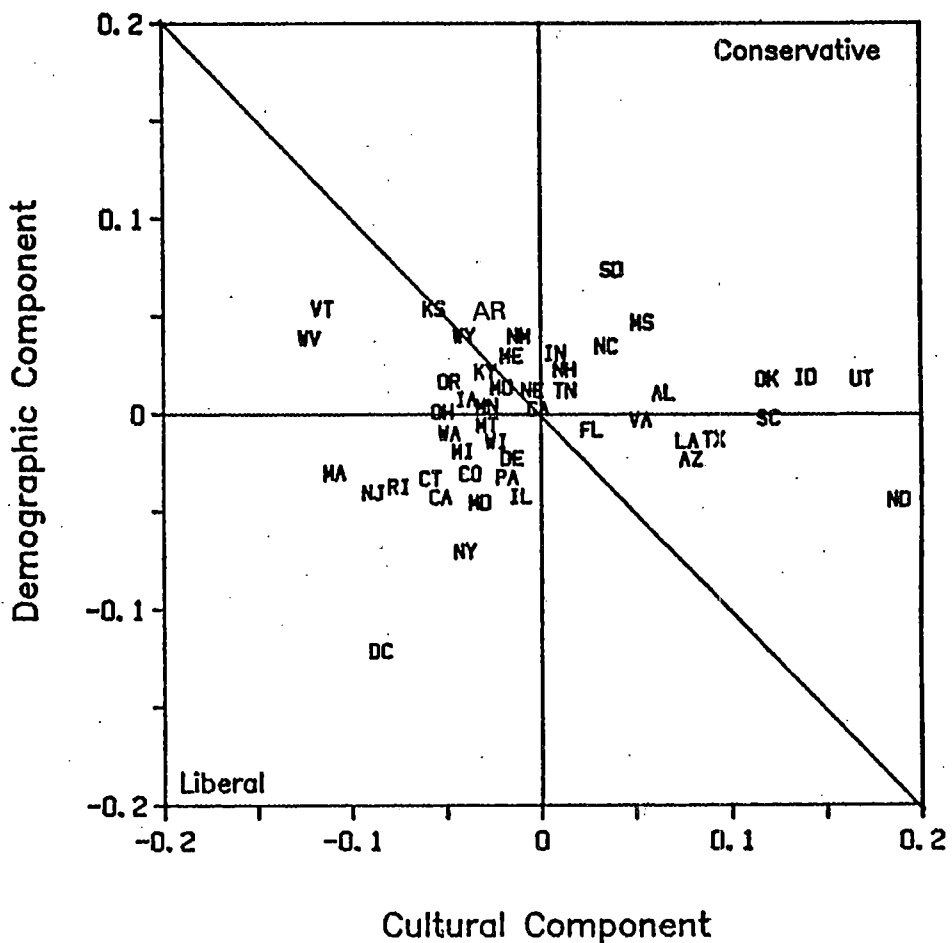


state variation in the demographic characteristics—race, religion, income, education, and so on—that we might think account for state-to-state differences in state attitudes. Put another way, state differences in partisanship and ideology are far more than the sum of their demographic parts.

This result must make us question the validity of the procedure of “simulating” state public opinion from the attitudes of the state’s known demographic groups. Simulation of state opinion from state

demographics has often been employed as a substitute for unavailable state-survey estimates (Pool, Abelson, and Popkin 1965; Weber 1971; Weber et al. 1972). But the simulation of opinion can be misleading if state attitudes are more than the reflection of state demographics. Here we provide ample evidence that the partisanship and ideology of state electorates are not determined exclusively from state demographic compositions but rather are largely the product of the unique political context within each state.

Figure 2. Components of State Ideological Identification



Accounting for State Differences in Political Culture

We have demonstrated that most of the variance in state attitudes in partisanship and ideology can be attributed to the different political cultures of the individual states. But what accounts for this cultural variation? Can we identify patterns that account for state-to-state differences in state effects on partisan and ideological identification?

Several hypotheses can be advanced to account for differing state influences on

partisanship and ideology. Most obviously, we can look for a geographic pattern of common state effects within regions or among geographically similar states. Second, we might expect Elazar's well-known measure of state political culture as *moralist*, *individualist*, or *traditionalist* to be related to our cultural components of partisanship and ideology. Third, we might find a social-context effect, with state demographic characteristics influencing state effects. Finally, we can look for a relationship between the two cultural components themselves: are

the causes of Democratic cultures the same as the causes of liberal cultures?

Region

Earlier, we saw that state effects contribute significantly to the prediction of individual partisanship and ideology beyond what even eight regions can do. But this does not preclude the possibility of regional patterning of the state effects. Referring back to Table 3, one can see some regional patterns. For instance, southern and border states show party effects that are considerably more Democratic than the norm. Southern and interior western states show ideology effects that are more conservative than the average.

The eight-way regional breakdown can account for 59% of the variance in the state coefficients for party identification and 34% of the variance in the state coefficients for ideological identification. Most of this regional predictability is due to the Democratic and conservative South. Excluding Deep South and Border South, only 7% of the variance in the partisan coefficients and only 10% of the variance in the ideological coefficients can be accounted for by region. Therefore, setting aside the cultural distinctiveness of the South, the effect of state cultures on partisanship and ideology does not follow a consistent geographic pattern. Classifying a state as Mid-Atlantic, Mountain, and so on, does not tell us much about the effect of residence in the state upon partisan or ideological identification.

Elazar's Political Culture

Elazar's well-known measure of political culture categorizes regions within states as *moralist*, *individualist* and *traditionalist*. Sharkansky (1970) operationalized Elazar's measure as a state-level index from purely moralistic states to purely traditional states. Sharkansky's measure

of traditionalism correlates quite negatively with our measure of cultural Republicanism ($-.69$) and somewhat positively with our measure of cultural conservatism ($.23$). Closer examination shows the common source of variation in each case to be South versus Non-South regionalism. Southern states are rated highly traditionalistic by Elazar and have very Democratic but conservative state effects. With southern and border states omitted, Sharkansky's measure correlates only $-.00$ and $-.03$ with partisan and ideological state effects, respectively. Clearly, then, our measures of state cultural effects on partisanship and ideology measure something quite different from Elazar's and Sharkansky's codification of state political culture.

"Social-Context" Effects

Studies of the contextual effects of small geographic units like neighborhoods have given special attention to the hypothesis that peoples' attitudes are influenced by the aggregated attitudes of those around them. Rationales include the possibilities that people respond to social norms or are influenced by social interactions. Local attitudes are typically operationalized as the attitudes of the locally predominant social groups. The effect of social context might be examined, for example, by seeing whether neighborhood effects on the vote correlate with the vote predicted from residents' individual characteristics (Huckfeldt 1979, 1984). At the state level, the social-context hypothesis offers an obvious prediction. If state effects reinforce state attitudes that result from individual-citizen characteristics, then the demographic component of state opinion (due to individual characteristics) should be related to the cultural component (due to residence in the state apart from individual effects).

Interestingly, the data offer no support for a social-context effect at the state

level. For party, the demographic and cultural components of state opinion are correlated at .02, and $-.21$ when southern and border states are removed. For ideology, the two components are correlated at .22, but only .02 when southern and border states are removed. Thus, the partisan and ideological leanings that a state obtains from its demographic composition do not seem to affect the directions of the cultural components of state partisanship and ideology. Perhaps surprisingly, although the state of residence appears to have a major impact on political attitudes, these state effects are not influenced by the attitudinal predispositions of the states' demographic groups.

Commonality to Partisan and Ideological Effects?

Although we cannot identify the specific causes of state effects on partisanship or ideology, perhaps the unidentified causes of the two sets of cultural effects are related. If so, the state effects on partisanship and state effects on ideology would correlate with each other. But even this expectation is not borne out by the data. The two are correlated at a surprising $-.04$. But here is an instance where excluding southern and border states boosts the correlation—due to the conservative but Democratic effects of southern states. With southern and border states excluded, the correlation between state effects for partisanship and ideology is .37, suggesting some commonality.

But at best, whether a state's political culture influences opinion in the liberal or the conservative direction is only modestly related to whether the state culture favors the Republican or Democratic party. At the same time, the correlation between the demographic components of partisanship and ideological identification is a rather high .67 for all states and .55

without southern and border states. This reflects the fact that the same variables that predict Democratic party identification generally are the same that predict liberal ideological identification as well.

This quest for causes of the measured state effects ends on a puzzling note. As we saw, indigenous "state" effects, which we attribute to state-to-state differences in political culture, account for far more of the interstate variance of partisanship and ideology than does interstate variation in the demographic compositions of state populations. A state's partisan or ideological bent seems more a function of its political history and development than of the characteristics of its population. But these important differences in political culture are not easily accounted for. Apart from the uniqueness of the South, with its Democratic but conservative culture, states seem to develop relatively conservative or relatively liberal cultures, or relatively Republican or relatively Democratic cultures, without any pattern that we can yet discern.

Context or Statistical Artifact?

This paper is an exercise in "contextual" analysis. As usually defined, *contextual analysis* means that individual-level variables are explained in terms of both individual-level and aggregate-level ("contextual") variables. One problem with contextual analysis is that the estimated effects of the aggregate-level independent variable might actually represent in part the effects of unmeasured individual-level variables (Stipak and Hensler 1982). Thus, if one does not adequately control for relevant individual-level independent variables, one risks exaggerating the effects of the contextual variables.

Here, this worry applies to the possible consequences of the incompleteness of the demographic controls. We have dummy variables for categories of *education, income, race, religion, age, gender, and urbanism*. Are omitted individual-level variables contributing to the apparent cultural effects? Probably they are, but in most instances only at the margins. An extreme illustration is the absence, in the measurement of religion, of a separate dummy variable for *Mormon*. The *Utah* dummy variable has regression coefficients indicating a conservative and a Republican influence of culture in this state. But part of this cultural effect for Utah probably would disappear if we could control for Mormon affiliation. Mormons are both homogeneous in their political attitudes and geographically concentrated, particularly in Utah. Consequently, Mormon religious affiliation may have a significant impact on the attitudes of Utah residents. Most (omitted) demographic variables, however, are less salient, less homogeneous and less concentrated and, consequently, would have a lesser impact.

We have seen earlier that observed standard demographic variables account for only a small portion of the observed state-to-state differences in opinion. If these demographics contribute so little to state differences in aggregate opinion, incorporating additional group characteristics would seemingly make little difference. Taking an empirical approach to this problem, we can see what happens to the estimates of state cultural effects if we add (or subtract) dummy variables that identify other population subgroups.

In our survey data, union membership is measured for about two-thirds of the respondents. We chose not to use this variable in our primary analysis, partly because of the loss of cases. A second reason was that union membership (or its absence) is not necessarily causally prior to opinion. For instance, Democrats and

liberals may be more likely to join unions. Or, Democratic or liberal state electorates could promote pronoun legislation that expands union membership. Either way, union members would be more Democratic and liberal, apart from membership affecting partisanship or ideology. But such concerns need not bother us here, since for current purposes we need only for union membership to be a predictor of partisanship and ideology and for frequencies of union membership to vary across state lines.⁵

Union membership makes a useful test variable to determine the robustness of the analysis against omitted variables. Union membership is a good marker of both partisanship and ideology. When added to the regression equations, union membership shows regression coefficients at about the same magnitudes as the standard demographic variables. (Compare union membership's $-.177$ coefficient for party and $-.084$ coefficient for ideology with the coefficients of Table 1.) Because union membership is unevenly spread across states, with heavier clustering where the Democratic party and political liberalism thrive, possibly some of our cultural effects for party and ideology can be attributed to union membership instead.

Happily for our enterprise of estimating the effects of state culture, our cultural estimates survive this threat. For both party and ideology, the 48 state-dummy-variable coefficients computed without a union membership control correlate almost perfectly ($.99+$) with a new set of state coefficients computed with union membership as an additional control. Moreover, the variances of the state coefficients decline only by about 1%.

This result suggests that our state effects are not seriously contaminated or underestimated by the omission of additional demographic variables that contribute to partisan and ideological attitudes.

Table 4. Predicting State Presidential Voting from the Demographic and Cultural Components of State Partisanship and Ideology

Components of Partisanship and Ideology	1976 Republican Vote		1980 Republican Vote		1984 Republican Vote	
	All States	North Only	All States	North Only	All States	North Only
Mean state opinion						
Partisanship	.75**	.56**	.50**	.27*	.32**	.41
Ideology	.22*	.29	.63**	.65**	.70**	.48*
Adjusted R ²	.60	.57	.64	.66	.58	.60
Demographic components						
Partisanship	.32	.32	.42*	.40	.20	.41
Ideology	-.11	.18	.00	.11	.25	.30
Adjusted R ²	.03	.14	.13	.16	.12	.14
Cultural components						
Partisanship	.69**	.36*	.40**	.11	.24*	.30
Ideology	.25*	.39*	.62**	.71**	.63**	.50*
Adjusted R ²	.51	.34	.51	.55	.42	.42
Number of cases	47	31	47	31	47	31

*Beta coefficient significant at .05.

**Beta coefficient significant at .001.

State Political Culture and the Presidential Vote

State-level presidential voting is probably the most visible manifestation of state public opinion. Therefore, one test of the importance of our estimates of state effects on partisanship and ideology is to observe their relationship to presidential voting. Does state presidential voting vary as a function of state partisanship and ideology? If so, does presidential voting respond to only the demographic component of state partisanship and ideology, or do the state effects attributable to state political culture also make an important contribution?

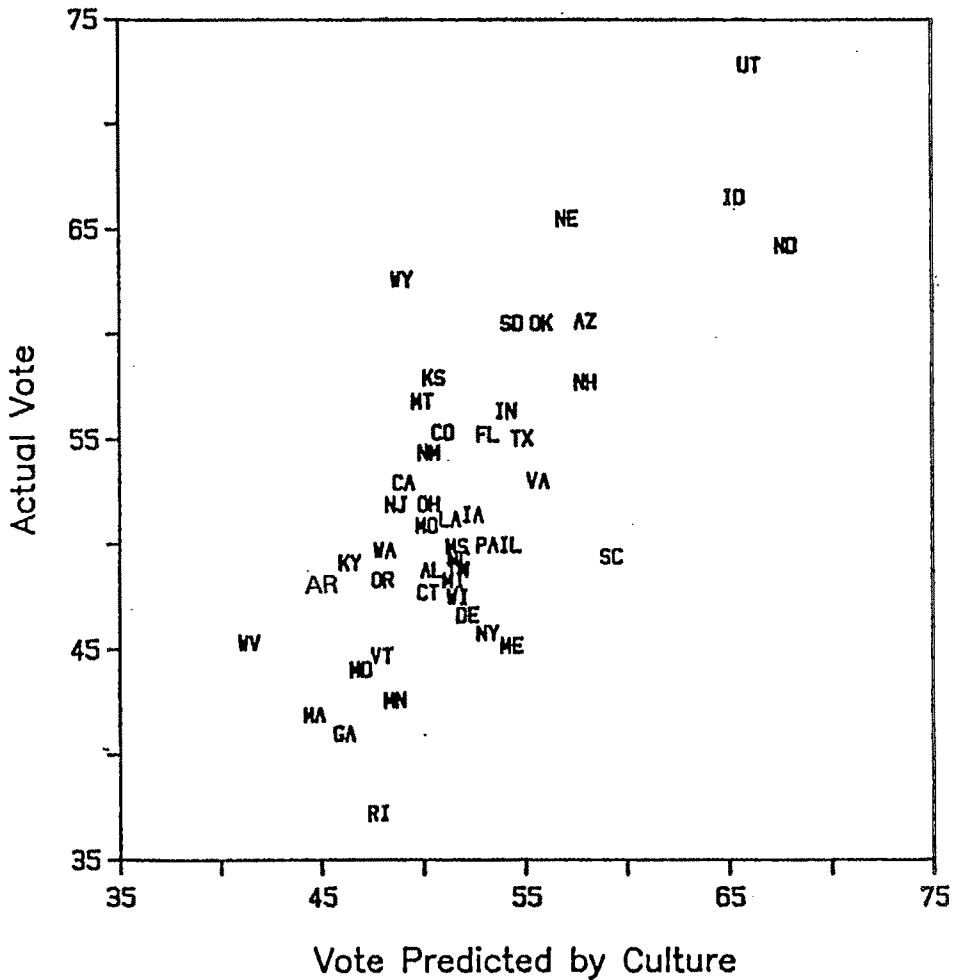
In Table 4, we examine state-level Republican voting in the three most recent presidential elections. The first set of equations predicts the vote from mean state opinion on partisanship and ideology. The second set of equations predicts the vote from the components of state

partisanship and ideology determined by citizen demographics. Finally, the third set of equations predicts the vote from the cultural components of state partisanship and ideology—our estimated state effects on individual attitudes. Because the District of Columbia is such an electoral outlier, it is excluded from the analysis of Table 4. Results are shown both for the full set of 47 states and for 31 states, with southern and border states excluded.

The first set of equations demonstrates that state presidential-election returns are quite sensitive to the party identification and ideological identification of the state's electorate. For each election, whether or not we include southern and border states, mean partisanship and ideology together explain well over half the variance in presidential voting. The demographic components of partisanship and ideology alone, however, explain only from 3% to 16% of the variance. Knowing whether a state has a Democratic or Republican—or

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Figure 3. The Impact of Political Culture on Presidential Voting: State Support for Reagan—1980



liberal or conservative—demographic profile adds only slightly to one's ability to predict its presidential voting. The cultural components of partisanship and ideology—estimated from our dummy-variable coefficients—permit by far the greater prediction of presidential voting. Cultural components account for 51% of the variance in the vote in 1976 and 1980 and 42% in 1984. Comparable figures without southern and border states are 34%, 55%, and 42% respectively. In-

terestingly, in the two Reagan elections, state effects on ideology appear to be far more important than state effects on partisanship. This is consistent with other evidence (Rabinowitz, Gurian, and McDonald, 1984) of the increasing role of ideology in the political geography of U.S. presidential elections.

In Figure 3, we illustrate the predictive ability of the cultural components for 1980. This figure shows the bivariate correlation between the 1980 Reagan vote

and the 1980 Reagan vote predicted from the cultural components of partisanship and ideology. In the 1980 election, over half the variance in state presidential voting can be accounted for by state effects on individual partisan and ideological attitudes, with the effects of individual demography removed.

The results shown in Table 4 and illustrated in Figure 3 are the culmination of this research as well as a justification for further exploration into the meaning and impact of state political culture. State cultures modestly influence individual partisanship and ideology. Apart from southern regionalism, however, variations in these state effects cannot be accounted for easily. One's state of residence contributes somewhat to one's political attitudes, but it is not clear why this is so. We have discovered that most of the variation in state partisanship and ideology is due to these unique state effects rather than state differences in demography. Finally, we see that these unique state effects can account for half the variance in state presidential voting. These results reveal both the importance and the mystery of state political cultures.

Conclusions

We have presented the first detailed examination of the effects of different state political cultures on political attitudes in the United States. For each state, we report reliable estimates of the influence of the state political culture on the partisanship and ideology of its individual citizens.

At the individual level of analysis, the state of residence produces at least as much variation in partisanship and ideology as does the typical demographic variable. With state as the unit of analysis, we find that most of the variance in state-level partisanship and ideology is due to state-level differences in political

culture rather than the demographic characteristics of residents of different states. These state effects are important politically, as indicated by the fact that they explain about half of the variance in state presidential voting.

The findings we have presented raise several issues for further investigation. For instance, at what point in a person's life does the state political culture exert its influence? Are the state effects we have measured the result of childhood socialization (assuming most people grow up near where they live as adults) or the effects of subtle influence of the local environment upon the attitudes of adults? Finally, the geographic variation in the effects of state political culture requires further inquiry. Do the effects of geography stop at state lines? Indeed the geographic sources of the variation in political opinions measured in this paper may extend beyond discretely different cultures separated by state boundaries. "State" effects captured by state boundaries may represent in part the sum of the effects of regional variation within states, or even of variation that transcends state lines. In any case, it is clear that the political attitudes of U.S. citizens vary in important ways on the basis of where in the United States they live.

Notes

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1. One caution about replacing the intercept with the base category is that the reported R^2 and F-level for the equation will in general be incorrect.

2. Recall that these standard errors are determined from the form of the equation in which the base state replaces the intercept. This is important.

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These standard errors are equivalent to sampling errors for state means, but with demographics held constant. If a base state were omitted (as with the normal routine of dummy-variable analysis), the standard errors would be equivalent to the standard error for a difference-of-means test comparing the effect of the particular state with the effect of the base state.

3. The same procedure can be used to estimate the reliabilities of the "raw" state-opinion estimates without the demographic controls. That is, we can estimate state opinion from state dummy variables alone and proceed as above. This yields reliability estimates of .93 for the state means on partisanship and .84 for the state means on ideology. Reliability estimates are slightly higher, therefore, for state estimates of opinion that reflect both cultural and demographic sources than for estimates of the cultural sources alone.

4. Although the cultural and the demographic components add up to define the state means, the variances do not add up in similar fashion. The slight covariance of the two components gets in the way.

5. To see why causal ambiguity is of no concern for this test, consider the extreme case, where union membership does not cause attitudes but where being a liberal or a Democrat cause unionism and/or where liberal or Democratic state cultures promote union membership. Union membership at the individual level would still predict ideology and partisanship, so that states (or state samples) with the most union membership would show up as more demographically liberal and Democratic, and by default, less culturally liberal and Democratic.

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TELEVISION NEWS AND CITIZENS' EXPLANATIONS OF NATIONAL AFFAIRS

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Causal beliefs are important ingredients of public opinion. Citizens are able to identify the causes of complex national issues and do so spontaneously. Evidence is presented that individuals' explanations of political issues are significantly influenced by the manner in which television news presentations "frame" these issues. These results are politically consequential, for individuals' explanations of national issues independently affect their assessments of presidential performance.

From Plato to Schumpeter, political theorists have been preoccupied with the ordinary citizen's ability to understand the course of public affairs (for a recent analysis, see Ferejohn 1987). Empirical research on this question suggests that public opinion is far from enlightened. Citizens are only fleetingly acquainted with current events and very few utilize ideological precepts to organize their political beliefs (for a review of research, see Kinder and Sears 1985). The low level of political knowledge and the absence of ideological reasoning has lent credence to charges that popular control of government is illusory (with respect to U.S. public opinion, for example, see Schumpeter 1950; Toqueville 1954).

The acquisition of information and the application of ideological principles, however, are not the only means for making sense of the political world. I will argue that causal beliefs are important ingredients of political knowledge; that citizens are quite capable of explaining complex national issues and that they do so spontaneously; that individuals' causal accounts of national issues are significantly influenced by the manner in which

television news presentations "frame" these issues; and finally, that individuals' explanations of national issues powerfully affect their attitudes toward government.

Explanations as Knowledge

Explanation is an essential ingredient of human knowledge. To explain events or outcomes is to understand them: to transform the "blooming, buzzing confusion" of today's world into orderly and meaningful patterns. Psychological research has demonstrated that causal relationships feature prominently in individuals' perceptions of social phenomena (Nisbett and Ross 1980; Weiner 1985). In fact, causal thinking is so ingrained in the human psyche that we even invent causation where none exists, as in purely random or chance events (see Langer 1975; Wortman 1976).

Explanatory knowledge is important to political thinking for two reasons. First, answers to causal questions abound in popular culture, making the task of explanation relatively inexpensive. One need not devour the pages of the *Wall Street Journal* or study macroeconomics to "know" why there is chronic unem-

ployment. Second and more important, explanatory knowledge is connotative knowledge. To "know" that unemployment occurs because of motivational deficiencies on the part of the unemployed is relevant to our attitudes toward the unemployed and our policy preferences regarding unemployment. In other words, explanatory knowledge is usable knowledge. Simple factual knowledge, on the other hand (e.g., the current rate of unemployment), does not so readily imply political attitudes and preferences. It is not surprising, then, that opinions, attitudes, feelings, and behaviors in a multitude of domains are organized around beliefs about causation (for illustrative research, see Schneider, Hastorf, and Ellsworth 1979, chap. 2). In fact, causal attributions exert such a powerful hold on behavior that "misattribution" techniques have proven effective in treating behavior disorders (see Fiske and Taylor 1984, 36-39), in inducing "prosocial" behaviors (see Schneider, Hastorf, and Ellsworth 1979, 93-95), and even in strengthening the general sense of psychological well being (see Langer and Rodin 1976).

Framing Effects in Political Explanation

When individuals engage in causal reasoning, their thinking processes are rarely systematic or exhaustive (see, for example, Kelley 1973). Virtually all forms of human judgment are infinitely labile, with outcomes hinging on a variety of circumstantial and contextual cues (for illustrative evidence, see Kahneman, Slovic, and Tversky 1982). The term *framing effects* refers to changes in judgment engendered by subtle alterations in the definition of judgment or choice problems. Tversky and Kahneman (among others), in a number of ingenious studies, have shown that choices between risky prospects can be profoundly altered merely by altering the description of the alternatives. Framing the prospects in

terms of possible losses, for example, induces risk-seeking behavior while describing the identical prospects in terms of potential gains makes people risk averse (Kahneman and Tversky 1982, 1984; for illustrations of framing effects in consumer behavior, see Thaler 1980). In short, the invoking of different reference points triggers completely different strategies of choice or judgment.

Political explanation would seem a particularly promising real-world domain for the occurrence of framing effects. Political stimuli are generally remote from everyday life, and ordinary citizens are unlikely to have developed strong commitments to particular explanations; accordingly, one would expect citizens to be highly susceptible to political framing effects.

Because citizens must depend primarily upon the media for information about the political world, there can be no better vehicle for examining political framing effects than media news presentations in general and television news presentations in particular.¹ The evidence reported here indicates that, according to the manner in which television news frames national issues, individuals' explanations of these issues are altered. Moreover, by altering explanations of national issues, the media alter attitudes toward the incumbent president.

Method

Residents of the Three Village area of Brookhaven Township, Suffolk County, New York, participated in a series of studies described to them as investigations of "selective perception" of television newscasts. Individuals responded to newspaper advertisements offering \$10 as payment. This procedure yielded samples that were approximately representative of the local area (see Table 1).

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Procedure

When participants arrived at the Media Research Laboratory, they completed an informed consent form and a short pretest questionnaire probing personal and political background variables. They then watched a 20-minute videotape containing seven news stories described as a representative selection of network news stories broadcast during the past six

months. On average, each viewing session had two participants. The fourth story on the tape represented the experimental manipulation and, depending on the condition to which participants were assigned (randomly), they saw different coverage of poverty, unemployment, or terrorism. With the exception of the treatment story, the videotapes were identical.

Following viewing of the videotape, participants completed a lengthy posttest

Table 1. Demographic and Political Profile of Residents of Brookhaven Township, Suffolk County, New York (in percentages, except *age*)

Demographic and Political Factors	Experimental Participants	1980 NES Sample	1980 Suffolk County ^a
Female	57	57	51
Nonwhite	5	13	8
Median age	37	41	30
High school education	38	63	64
Some college	26	20	18
College graduates	36	17	18
Employed	65	56	58
Unemployed	6	8	6
Retired	4	13	} 36
Housewives	11	18	
Students	11	3	
Blue-collar	19	37	27
White-collar	49	31	48
Professional	30	22	26
Protestant	32	63	—
Catholic	38	23	—
Jewish	27	3	—
Voting in last presidential election	74	62	—
Republican	27	22	37
Democratic	40	41	23
Independent	23	24	23

Note. For purposes of assessing the representativeness of this group, the comparable figures based on respondents in an American National Election Study (NES 1980) are shown alongside, as are the figures for Suffolk County as a whole. As the figures indicate, the major divergence between the experimental participants and the county/national baselines concerns education. Experimental participants were significantly more educated. The distinctiveness of the experimental samples can be attributed to the fact that Brookhaven Township, where the research was conducted, includes a very high proportion of professional and white-collar workers. Compared to the national sample, the experimental group includes significantly higher numbers of professionals and white-collar workers, but this is characteristic of Suffolk County. The same holds for religion; Suffolk County includes large Jewish and Catholic populations. On all other characteristics including party identification, the three groups are similar.

^aSuffolk County data provided by Long Island Regional Planning Board; religion and voting figures not available.

(individually, in separate rooms) that included the key questions probing their explanations for the target problem as well as other questions on their issue opinions, their perceptions of President Reagan's issue positions, their assessments of the president's overall performance, his competence and integrity, and his performance in specific domains. On completing the posttest, participants were paid and debriefed.²

Measures

I relied entirely on unstructured questions to get at participants' causal accounts. These questions, though unwieldy and coding intensive, are relatively non-reactive. Given the paucity of research into citizens' explanations of political issues, it would have been presumptuous for the researcher to define the appropriate causal categories. The approach was to let each individual choose his or her own categories, with no prompts whatsoever.

The posttest began by asking participants "When you hear or read about _____, what kinds of things do you think about? Please list as many thoughts as you have." Responses to this "thought-listing" question ranged widely. Nevertheless, it was possible to separate explanatory statements from other categories with a high degree of reliability.³ Statements were coded as explanatory if they made reference to antecedent causes of the target issue either specifically ("If people would try harder to find work, we would all be better off") or vaguely ("Society screws the poor; the rich get richer, the poor poorer"). Later in the questionnaire, participants were asked to identify what they considered to be "the major causes of _____." The thought-listing method was designed to provide evidence on the spontaneity of causal reasoning; the "most-important-causes"

question identifies the specific content of causal beliefs.

Study 1: Poverty

We established five experimental conditions. In the first condition (*national poverty*), participants watched a news story that documented the increase in poverty nationwide and the significant reductions in the scope of federal social-welfare programs under the Reagan administration. In the second condition (*high unemployment*), participants watched a story that juxtaposed the national unemployment rate with the size of the federal budget deficit. The three remaining conditions all framed poverty in terms of particular instances or victims of economic hardship. The first (*the high cost of heat*) portrayed two families unable to pay their heating bills. The second, (*the homeless*) focused on homeless individuals—two black teenagers living on the streets of New York City, and a white couple forced to live in their car in San Diego.⁴ Finally, the third case-study condition (*unemployed worker*) described the financial difficulties facing the family of an unemployed auto worker in Ohio. These case-study stories were edited so as to be entirely descriptive; the news stories simply presented the predicament of each victim.

Results

The results of the "thought-listing" procedure revealed quite clearly that causal attributions of poverty figure prominently in participants' store of information. Participants listed a total of 322 thoughts, of which 124 (39%) represented explanatory statements. The causes people cited most often fell into five categories—inadequate motivation, poor education and inadequate skills, welfare dependence, the economy in general, and governmental actions/inactions.

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Table 2. Most Important Causes: Poverty

Explanations	Condition				
	National Trends	High Unemployment	Heating Costs	Homeless	Unemployed Worker
Motivation	.10 (3)	.06 (2)	.13 (4)	.23 ^a (14)	.13 (4)
Skills	.14 (4)	.11 (4)	.22 (7)	.39 ^b (22)	.19 (6)
Culture	.00 (0)	.03 (1)	.03 (1)	.07 (4)	.09 (3)
Economy	.31 (9)	.39 ^c (14)	.37 (12)	.18 (11)	.28 (9)
Government	.45 ^d (13)	.42 ^d (15)	.25 (8)	.15 (9)	.31 (10)
Number of cases	14	14	14	29	15

Note. Number of responses shown in parentheses.

^aSignificantly different from *high unemployment* condition, $p < .05$ by two-tailed t-test.

^bSignificantly different from all other conditions, $p < .05$ by two-tailed t-test.

^cSignificantly different from *homeless* condition, $p < .05$ by two-tailed t-test.

^dSignificantly different from *homeless* condition, $p < .05$ by two-tailed t-test.

Even allowing for the possibility that respondents may have thought of these antecedent factors only in a loose causal sense, it is still impressive that explanatory thoughts take precedence over evaluations or feelings. Respondents were much more likely to offer reasons for poverty than affective reactions.

Turning to the "most-important-causes" question, the categories of causes parallel perfectly those that emerged from the thought-listing question (see Table 2). Participants were far from mystified by the question—a total of 189 causes were cited, for an average of 2.2 per person. Virtually every respondent mentioned at least one cause (98%), and their responses were remarkably homogeneous in that five categories accounted for almost all the responses (183 out of 189).

Motivational explanations, which made up 14% of all responses, point to victims as causing their own plight ("laziness," "People prefer handouts to work," etc.). *Cultural* explanations (4%) invoke family background and upbringing as reasons ("Most people are born into poverty," "broken families"). *Inadequate skills* (24%) refer primarily to the lack of education and job skills. The remaining

two categories shift the causal focus away from dispositional or individual characteristics to societal factors. *Economic* explanations (29%) include reference to national economic conditions ("There's unemployment everywhere," "the recession"), to particular sectors of the economy ("There's no construction these days"), the global economy ("competition from overseas"), management/union actions ("Union wages are forcing companies to relocate"), and changes in the nature of work ("high tech"). Finally, *government and society* (24%) is the most politicized of the categories. Included here were references to President Reagan's policies or actions, social-welfare programs, budgetary allocations (particularly cuts in social programs and increases in defense spending), and broad societal trends such as racial discrimination, inequality, the lack of opportunities for people to advance, and so on. (For essentially similar results with quite different indicators and national samples, see Feagin 1975; Goodban 1981; Kluegel and Smith 1986.)

The effects of the experimental manipulations on participants' explanations are summarized in Table 2. Given the small

proportion of responses referring to cultural factors, these were subsumed within the motivational category. Causal beliefs were significantly molded by the manner in which the news framed poverty. After watching accounts of specific homeless people, participants were especially drawn to poor motivation or inadequate skills as causal factors. After watching the news story that documented trends in the national poverty rate and federal spending on social programs, participants were especially drawn to economic and governmental/societal factors. If we combine the two systemic response categories (*economy* and *government*), the proportion of systemic attributions ranges from .33 in the *homeless* condition to .81 in the *high unemployment* condition. Conversely, the proportion of dispositional attributions (*motivation, skills, and culture*) ranges from a low of .17 in the *high unemployment* condition to a high of .69 in the *homeless* condition.

Framing poverty in terms of individual victims did not always draw individuals to dispositional accounts of poverty. The homeless, the unemployed, and those unable to afford heat all represent people facing economic difficulties, yet they elicit very different explanations of poverty. The type of victim makes a difference. While the homeless trigger personal or dispositional attributions, people unable to pay their utility bills elicit a preponderance of economic and societal attributions, with the unemployed worker falling in between, eliciting a relatively even mix of societal and dispositional attributions.

Descriptions of homeless people may trigger dispositional explanations of poverty more frequently because "the homeless" fit U.S. citizens' mental representations of "poor people" to a greater degree than unemployed workers or families living in unheated homes. Alternatively, it may be that the predicament of the homeless, being more extreme than that faced

by the unemployed worker or families without heat, evokes more individualistic explanations. There is evidence in the psychological literature on attribution that more extreme outcomes prompt dispositional or individualistic attributions (e.g., Walster 1966). Because the homeless have lost everything, people reason that there must be something deficient in their character; if not, homelessness could happen to anyone, a rather threatening proposition (see Lerner 1980).

All told, these results drive home the point that explanations of poverty depend considerably upon the particular informational context in which poverty is encountered. When poverty is framed as a societal outcome, people point to societal or governmental explanations; when poverty is framed in terms of particular victims of poverty, particularly the homeless, people point instead to dispositional explanations.

To assess whether these framing effects withstand controls for built-in or chronic differences in causal beliefs, the analysis incorporated individuals' socioeconomic status, partisanship and approval of presidential performance, issue opinions, and degree of political involvement. I computed two indexes corresponding to the number of systemic (*governmental* and *economy*) and dispositional (*motivation, skills, and culture*) causes mentioned and subjected them to a multiple-regression analysis. These results are shown in Table 3.

The effects of the framing manipulations emerged relatively unscathed after taking into account longstanding individual differences in causal beliefs.⁵ These differences themselves were fairly intuitive. Nonwhites and females were more prone to see systemic causes for poverty, while college graduates and protestants were loath to cite systemic factors. Professionals, older people, and college graduates saw more dispositional causes while Catholics cited fewer dispositional causes.

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These results are essentially similar to those reported in analyses of national-survey data (Kluegel and Smith 1986; Gallup Opinion Report, 1985; Lewis and Schneider 1985) and can be interpreted in motivational terms as reflecting the general impetus toward "defensive attribution." While "haves" favor dispositional explanations, "have-nots" lean toward systemic accounts.

Republicans were significantly less systemic in their reasoning than

Democrats or independents. Similarly, participants for whom President Reagan's stock is high were less likely to offer systemic causes. This effect was independent of the feedback from systemic explanations to approval of the president's performance (see Table 4). While individuals who believed that poverty had worsened across the nation were drawn more to systemic causes, individuals who believed that their own economic circumstances had worsened were less prone to

Table 3. Antecedents of Causal Attributions: Poverty

Variables	Systemic Index ^a	Variables	Dispositional Index ^d
Protestants	-.63 (.31)	Professionals	.32 (.23)
College graduates	-.21 (.20)	College graduates	.30 (.20)
Respondents who feel poverty is worsening	.22 (.20)	Age > 40	.28 (.24)
Nonwhites	.71 (.44)	Attentiveness to public affairs ^e	-.13 (.06)
Republicans	-.41 (.29)	Conservative-issue opinions ^f	.13 (.11)
Males	-.40 (.19)	Respondents with worsening economic situation	-.20 (.19)
Exposure to systemic media frames ^b	.31 (.19)	Exposure to systemic media frames	-.25 (.22)
Unemployed/laid off	-.47 (.40)	Exposure to <i>homeless</i> media frame	.45 (.23)
Frequency of TV news watching	.22 (.16)	Catholics	-.36 (.21)
Approval of Reagan's overall performance ^c	.17 (.13)		
Standard error of regression	.79	Adjusted R ²	.30
Number of cases	85	Number of cases	85

^aEntries are unstandardized regression coefficients (2SLS) with standard errors in parentheses.

^bNational trends/high unemployment conditions combined.

^cEndogenous variable; the first-stage equation was as follows: 1.77 - .30 (Democrats) + .64 (integrity ratings) + .47 (competence ratings) + .10 (conservative-issue opinions). The adjusted R² was .63.

^dEntries are unstandardized regression coefficients (OLS).

^eThree-item index based on attentiveness to news about public affairs and frequency of "following what's going on in government."

^fFour-item index based on opinions concerning school prayer, civil rights, and spending for defense and social programs.

Table 4. Poverty Attributions and Evaluations of President Reagan

Predictor	Coefficient
Republican party identification	.33 (.20)
Specific problem performance ^a	.18 (.10)
Trait ratings: competence ^b	.39 (.09)
Trait ratings: integrity ^c	.44 (.11)
Index of systemic attributions ^d	.31 (.12)
Standard error of regression	.66
Number of cases	85

Note. 2SLS estimates. Standard errors shown in parentheses.

^aSummed index of poverty, unemployment, and arms-control performance ratings.

^bSummer index of *experienced* and *provides strong leadership* trait ratings.

^cSummed index of *compassionate*, *fair*, and *honest* trait ratings.

^dEndogenous variable. The first-stage equation was as follows: 1.46 - .40 (no. of dispositional causes) + .47 (exposure to systemic media frames) - .68 (Protestants) + .43 (belief that poverty is worsening nationwide) + .39 (frequency of TV news watching) - .41 (males) - .19 (college graduates). The adjusted R² was .36.

see the poor as contributing to their lot.

In sum, though causal knowledge is embedded in individuals' socioeconomic circumstances and partisan attitudes, media frames affect this knowledge independently of these antecedents. When the media direct individuals' attention to national outcomes, explanations for poverty become predominantly systemic; when the media point to particular instances of poor people, explanations for poverty become predominantly dispositional.

The final analysis assesses whether explanations for poverty spill over to evaluations of the president. Specifically, the question was whether individuals who understand poverty as a societal or governmental outcome are less apt to praise President Reagan's performance. In this analysis the reverse possibility must also be considered, since supporters of President Reagan systematically discount systemic causes when asked to account for poverty (as noted in Table 3). Evalua-

tions of the incumbent president and explanations for some negative outcome (such as poverty) are likely to feed on each other in reciprocal fashion. Fortunately, it is a relatively simple task to estimate these reciprocal effects, for several of the mainstay determinants of presidential popularity turned out to be essentially independent of individuals' explanations for poverty.⁶ Similarly, determinants of causal beliefs (most notably, the effects of the framing manipulations) were unrelated to ratings of Reagan's performance. In short, two-stage least-squares analysis was appropriate since there were effective instruments for both popularity and causal beliefs. The effects of poverty attributions on evaluations of Reagan's overall performance are presented in Table 4.

Systemic explanations of poverty exerted powerful effects on participants' ratings of Reagan's overall performance even after controlling for the effects of

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Table 5. Most Important Causes: Unemployment

Explanations	Condition		
	High Unemployment	Steel Industry	Unemployed Worker
Motivation	.13 (4)	.05 (1)	.12 (3)
Skills	.07 (3)	.05 (1)	.12 (3)
Economy	.66 (21)	.73 (16)	.62 (16)
Government	.14 (4)	.18 (4)	.16 (4)
Number of cases	14	12	14

Note. Number of responses shown in parentheses.

partisanship, ratings of his performance in the specific issue areas of arms control, the budget deficit, and unemployment, assessments of his competence and integrity, and the reverse effects of his overall performance. Individuals who cited more systemic causes were significantly more negative toward President Reagan. This effect was much stronger than the corresponding feedback ($b = .17$, $s. e. = .13$, as reported in Table 3). The number of dispositional causes mentioned proved unrelated to Reagan's popularity rating.

The results in Table 4 provide compelling evidence that individuals use their causal beliefs about national issues as cues for evaluating the president. Individuals drawn to systemic accounts of poverty are also more critical of President Reagan. To the degree systemic explanations for poverty are subject to media framing effects, these effects can indirectly influence the president's standing with the public.

Study 2: Unemployment

Three conditions were established, each providing a different perspective on unemployment. Two of the conditions were taken from the first study—*high unemployment* and *unemployed worker*. The former represents a systemic frame with the focus on the national unemployment rate, while the latter is a case study of a particular victim of unemployment.

In between these two extremes was a third level of coverage describing the serious economic difficulties facing the U.S. steel industry. The expectation was that this presentation would trigger *the economy* as the dominant reference point. (Note that all three conditions featured the identical "lead-in" by the newscaster, who noted the persistence of high unemployment.) In all other respects, the procedures and method were identical to those of Study 1.

Results

As was the case with poverty, causal attributions were offered spontaneously. The 40 subjects listed a total of 110 thoughts on unemployment, of which 40 took the form of causal attributions. Turning to the content of participants' explanations, Table 5 presents the breakdown of responses to the "most-important-causes" question. The identical set of categories obtained in Study 1 accounted for all the causes cited. Among these, *the economy* was predominant, so much so that two of every three causes cited fell into this category. This was the case even in the *unemployed worker* condition, where the proportion of economic causes was .62. This same story elicited a much lower proportion of economic causes when the target issue was poverty (.28). As Table 5 makes plain, the framing manipulations had no impact on the

Table 6. Unemployment Attributions and Evaluations of President Reagan

Predictor	Coefficient
Democratic party identification	-.47 (.26)
Specific problem performance ^a	.66 (.17)
Trait ratings: integrity ^b	.41 (.19)
Respondents who believe unemployment is worsening nationwide	-.56 (.32)
Index of dispositional attributions	.43 (.21)
Adjusted R ²	.62
Number of cases	40

Note. OLS estimates. Standard errors shown in parentheses.

^aSummed index of poverty, unemployment, and arms-control ratings (1 = *very poor*, 5 = *very good*).

^bSummed index of *honest*, *fair*, and *compassionate* ratings.

relative prominence of economic (or any other) causes. In fact, explanations of unemployment stand in marked contrast to explanations of poverty. While the former is understood uniformly as a systemic problem regardless of the news frame, the latter is seen as having a mix of both systemic and dispositional causes, the precise composition of the mix being significantly influenced by media presentations.

Even though participants' causal beliefs were unaffected by the framing manipulation, these beliefs did carry over to evaluations of President Reagan. I combined the *economy* and *governmental/societal* categories to form an index of systemic causes. Similarly the two dispositional categories were summed. The independent effects of these indexes on Reagan's popularity is shown in Table 6.

The more dispositional the attribution, the more positive the rating of President Reagan's overall performance. This effect was uncontaminated by any feedback from the Reagan performance ratings because ratings of Reagan's overall performance had no effect on the number

of either dispositional or systemic causes for unemployment. Respondents who cited more systemic attributions did tend to be less supportive of the president, but this relationship did not quite attain conventional levels of statistical significance.

Study 3: Terrorism

The target issue in this study was terrorism. The stimulus event was the June 1985 hijacking of TWA Flight 847 and the subsequent hostage drama in Beirut. Following the release of the hostages, all three networks aired detailed recapitulations of the crisis. One such story (ABC's) was edited into three very different versions. The first condition (*U.S. role*) viewed the hijacking incident as a protest against the United States. The U.S. role in the region as the principal ally of Israel was noted as was the hijackers' demands that Israel release the Shiites being held prisoners. President Reagan was shown declaring that the U.S. would never concede to terrorists. The second condition

(*local politics*) shifted the focus to the internal politics of Lebanon. The report traced the history of Amal, the Shiite organization holding the hostages, and described its political ideology and its efforts to gain a power base in Beirut. The third condition (*hostages released*) showed the hostages saying goodbye to each other prior to their release. Red Cross officials were interviewed regarding the state of the hostages' health and their treatment during captivity. The hostages were then shown arriving in Damascus. Finally, unlike the earlier studies, a control condition was added in which subjects saw no story on the target issue, that is, subjects watched the identical videotape without the story on the hijacking.

Results

As usual, participants were asked to list the things they thought about when they heard or read about terrorism. The 92 participants mentioned a total of 201 thoughts. Cause and effect were prominent in the thoughts listed. Many participants explicitly raised causal inquiries (e.g., "Why do people do things like this?"). Causal attributions were also frequent, the most common being dispositional—terrorists as fanatics, deranged extremists, and so on. Terrorism was also traced to situational causes including local political disputes, U.S. policy toward the Middle East, and the support provided terrorist groups by radical leaders such as Colonel Qaddafi. Causal inquiries and attributions together accounted for 31% of all thoughts. All told, the distribution of responses to the thought-listing question maintained the pattern established in the first two studies, namely, that causal attributions (and questions) are offered spontaneously when individuals think about political issues.

What did participants consider to be the prime causes of terrorism, and to what degree were their causal attributions

susceptible to framing effects? Three classes of attributions dominated responses to the "most-important-causes" question. *Dispositional* attributions trace terrorism to personal traits, particularly extremism, fanaticism, and a craving for recognition. *Situational* attributions cover characteristics of the immediate situation and range from general political instability, to governmental oppression, to poverty and injustice. *Policy* attributions encompass actions of the U.S. government, including support for Israel and opposition to the Arab or Palestinian cause, Israeli dominance, and concerted efforts (primarily by "communists") to weaken the U.S. These three broad categories accounted for 175 of the 198 causes mentioned.

The framing manipulation was designed to elicit a significantly higher proportion of policy attributions in the first (*U.S. role*) condition, a higher proportion of situational attributions in the second (*local politics*) condition, and, by default, a higher proportion of dispositional attributions in the *hostages released* and control conditions. The reasoning underlying this last prediction takes into account the well-known tendency of individuals to attribute actions to personal characteristics, a tendency that has been dubbed the "fundamental attribution error" (Ross 1977). In short, under conditions of no coverage and no particular media frame (*hostages released*), dispositional attributions will predominate. When the media frame terrorist acts as part of a local power struggle, situational attributions will be encouraged, and finally, when the media frame terrorist acts as protests against the U.S., policy attributions will prevail.

Table 7 provides only partial support for these expectations. Dispositional attributions tended to be greatest in the *U.S. role* condition, followed closely by the condition in which there was no media frame (*hostages released*). More in keeping with expectations, situational attribu-

Table 7. Most Important Causes: Terrorism

Attributions	Condition			
	U.S. Role	Local Situation	Hostages Released	No Coverage
Dispositional	.45 (20)	.29 ^a (11)	.44 (15)	.31 (18)
Situational	.24 (11)	.53 ^b (20)	.35 (12)	.47 (27)
Policy	.31 ^c (14)	.18 (7)	.21 (7)	.22 (13)
Number of cases	19	19	18	36

Note. Number of responses shown in parentheses.

^aSignificantly different from U.S. role condition, $p < .15$ by two-tailed t-test.

^bSignificantly different from U.S. role and hostages released conditions, $p < .10$ by two-tailed t-test.

^cSignificantly different from all other conditions, $p < .16$ by two-tailed t-test.

tions were most frequent in the *local politics* condition (which differed significantly from the *U.S. role* and *hostages released* conditions). Contrary to expectations, individuals given no news of terrorism tended to attribute terrorism to situational rather than dispositional factors. Finally, framing the hijacking as an act of protest against the U.S. did raise the proportion of policy attributions.

A plausible (though obviously post hoc) explanation for the dispositional flavor of participants' attributions of terrorism in the *U.S. role* condition concerns affect. Because this story presented the terrorist act as an expression of opposition to the U.S., it may have prompted a greater outpouring of negative affect. When terrorists are presented as anti-United States, viewers are more likely to be aroused and thereby attribute the actions of terrorists to their personal deficiencies. The posttest questionnaire included a set of questions asking viewers whether the story on the hijacking had "made them feel" a variety of emotions including "disgust," "anger," and "fear." I summed the responses (*yes/no*) to form an index of negative affect. Participants in the *U.S. role* condition were significantly more aroused by the hijacking story. The mean affect score in this condition was 2.11, as opposed to 1.70 for the remaining

conditions, a statistically significant difference ($t = 2.01$, $p < .05$ by one-tailed test). Moreover, the greater the level of negative affect, the more dispositional the causal attribution (the simple correlation (r) between the index of negative affect and the number of dispositional causes mentioned was .17, $p < .10$). In short, the presentation in the first condition unintentionally manipulated participants' hostility toward terrorists, thereby encouraging them to cite negative personal traits as causes of terrorism.

The independent effects of the framing manipulations on participants' attributions are shown in Table 8. In addition to the *U.S. role* frame, the antecedents of policy attributions included political activity (the more active being more inclined to cite policy causes), religion (Catholics being more policy oriented), and evaluations of President Reagan's performance in dealing with terrorism (the more disapproving the evaluation, the greater the number of policy attributions). Note that this last effect is independent of any feedback from policy attributions to evaluations of the president's performance in handling the problem. (The feedback itself is presented in Table 9).

In addition to participants who watched the *local politics* frame, Jews, Protestants, and Democrats were more

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apt to cite situational causes of terrorism. Dispositional attributions were affected by the experimental manipulations alone—individuals in the *U.S. role* and *hostages released* conditions were more dispositional in their explanations for terrorism.

Finally, I turn to the impact of attributions of terrorism on evaluations of President Reagan's performance in handling terrorism (see Table 9). In a later section of the posttest, participants were asked to indicate (on a five-point scale) the degree to which they approved or disapproved of the president's performance in dealing with terrorism (this study did not include

the question on the president's overall performance).

Both policy and dispositional attributions affected ratings of the president's performance, but in opposite directions. The greater the number of policy attributions, the more critical the terrorism performance rating. This effect was substantial, despite having been purged of the reciprocal influence of participants' evaluations of the president's performance. It was also independent of partisanship, issue opinions, race, gender, and a measure of political interest. The number of dispositional attributions also affected participants' ratings of the presi-

Table 8. Antecedents of Terrorism Attributions

Predictor	Policy ^a	Situational ^b	Dispositional ^b
<i>U.S. role</i> condition	.32 (.15)	—	.55 (.16)
<i>Local politics</i> condition	—	.22 (.15)	—
<i>Hostages released</i> condition	—	—	.21 (.16)
Political activity ^c	.10 (.08)	—	—
Protestants	—	.29 (.17)	—
Catholics	.30 (.13)	—	—
Jews	—	-.55 (.31)	—
Democratic party identification	—	.11 (.11)	—
Approval of President Reagan's handling of terrorism ^d	-.16 (.09)	—	—
Standard error of regression	.53	—	—
Adjusted R ²		.41	.42
Number of cases	84	92	92

^a2SLS estimates. Standard errors shown in parentheses.

^bOLS estimates.

^cComposite index based on voting, attending a rally, and contributing money.

^dEndogenous variable; the first-stage equation was as follows: 2.04 - .33 (interest) - .31 (males) + .43 (Republicans) + .67 (whites) + .31 (conservative-issue opinions). The adjusted R² was .34.

**Table 9. Terrorism Attributions and Evaluations of
President Reagan's Handling of Terrorism**

Predictor	Coefficient
Republican-party identification	.33 (.27)
Political interest ^a	-.22 (.10)
Conservative-issue opinions ^b	.31 (.11)
Males	-.26 (.20)
Whites	.71 (.46)
Index of policy attributions ^c	-.63 (.23)
Index of dispositional attributions	.18 (.13)
Standard error of regression	.87
Number of cases	84

Note. 2SLS estimates. Standard errors shown in parentheses.

^aBased on four questions—the degree to which respondents follow public affairs, frequency of discussing politics, attentiveness to newspaper, and attentiveness to TV coverage of public affairs.

^bAs defined in Table 2.

^cEndogenous variable; the first-stage equation was as follows: .37 – .29 (no. of situational attributions) + .24 (U.S. role condition) + .17 (index of political activity) + .32 (Catholics). The adjusted R² was .20.

dent's performance: the more dispositional one's understanding of terrorism, the more positive the rating. This effect, however, was noticeably weaker than that exerted by policy attributions.

To sum up, attributions for terrorism are influenced by the perspectives television news presentations impose on terrorist acts. When these acts are placed in the context of a local political situation, situational attributions predominate; when terrorism is framed as a protest against the U.S., policy and dispositional attributions come to the forefront of individuals' explanations. Finally, as was true for poverty and unemployment, explanations for terrorism are relevant to evaluations of the president, in this case to ratings of the president's performance in regard to terrorism.

Conclusion

People can and do explain the issues and events they encounter in the world of public affairs. These explanations are politically consequential, for they can be and are integrated with evaluations of the incumbent president. The more individuals attribute problems to structural or systemic causes, the more critical they are of President Reagan's performance.

Individuals are quite sensitive to contextual cues when they reason about national affairs. Their explanations of issues like terrorism or poverty are critically dependent upon the particular reference points furnished in media presentations. Whether terrorists are seen as protesting U.S. actions or as attempting to strengthen their political influence has the

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effect of invoking very different attributions of terrorism. Similarly, whether news about poverty takes the form of particular victims or of nationwide outcomes makes a difference to viewers' attributions of poverty.

The exception to this pattern is unemployment, where attributions are heavily systemic regardless of the media frame used. It may be that the term *unemployment* is itself a sort of semantic frame or wording effect that directs individuals to think of the concept as a collective rather than individual outcome. Asking people to explain "why John Smith was laid off" or "why so many Americans are out of work" may elicit a different set of attributions. Alternatively, the sustained prominence of economic problems in the nation's political rhetoric and policy agenda during the past decade may have served to alert individuals to *the economy* as a catchall cause for unemployment.

It is important to qualify these results by noting the short time interval between the experimental manipulation and the elicitation of participants' causal beliefs. The framing effects may wear off rapidly. As yet I have no evidence bearing on the question of persistence. However, the identical experimental design yielded significant delayed effects (up to a week following exposure to the treatment) on participants' responses to an open-ended question concerning "the most important problems facing the nation" (see Iyengar and Kinder 1987, chap. 3).

All told, these results suggest that it is important to document the degree to which particular media frames are more or less dominant in news coverage of current issues. To take the case of poverty, the tendency of the U.S. public to see the causes of poverty as lying within the poor themselves may be due not only to dominant cultural values like individualism and self-reliance (as argued by Feldman 1983; Kluegel and Smith 1986), but also to news coverage of poverty in which images

of poor people predominate. This is in fact the case with television news coverage of poverty, where close-up depictions of personal experience far outnumber depictions of poverty that are societal or nationwide in emphasis. I have examined every story in the *CBS Evening News* from January 1981 through December 1985 that made reference to "welfare," "social programs," "poor people," "hunger," and other such referents of poverty. Ninety-eight stories contained material relevant to poverty, of which 56 (57%) presented a particular victim. The figure is actually higher if 1981 is excluded, since that was the year of the pitched battles between the Reagan administration and the Democratic Congress on appropriations for social programs, thus resulting in an abnormally high number of government-oriented poverty stories. For the 1982-85 period, the percentage of case-study stories is 68. U.S. citizens who watched *CBS Evening News* during this period would, understandably, have been drawn to dispositional accounts of poverty, whatever social and economic values they subscribed to. At this point, I can only offer the pervasiveness of case-study coverage as a plausible cause of public beliefs. I do have evidence, however, that the degree to which particular issues are seen as *national* problems is significantly lowered when issue coverage takes the form of the "particular-victim" scenario. Specifically, Kinder and I found that when network news coverage of both pollution and unemployment focused on individuals or families adversely affected by either issue, viewers were less likely to come away believing that either pollution or unemployment were important national problems (Iyengar and Kinder 1987, chap. 4).

The political implications of the "particular-victim" media frame are unmistakable. Citizens' causal attributions will fix upon dispositional rather than structural factors. This is naturally

beneficial to the incumbent president—people are less likely to treat the administration and its actions as causal agents. President Reagan's concern in 1982, at the height of the recession, that the networks were providing too much coverage of people who had lost their jobs. ("Is it news that some fellow out in South Succotash has just been laid off and that he should be interviewed nationwide?") seems misplaced. In light of the results reported here, the White House should encourage such coverage, for it has the effect of shielding the president from any culpability, deserved or otherwise.

In closing, I hope that this paper serves to promote research into the explanatory or causal side of public opinion. As I have shown, ordinary people are quite capable of explaining national problems. In doing so, however, they are susceptible to media-framing effects. In altering individuals' causal knowledge, news presentations also influence individuals' evaluations of the president. Given what is known about the "imperialism" of causal attributions, this last finding may be but the tip of an iceberg. Researchers interested in economic voting have begun to probe into voters' explanations for their personal economic situation, finding that these attributions serve to politicize personal circumstances. That is, to the degree voters see systemic causes for their own financial difficulties, they are more likely to vote against those in office (see Feldman 1982; Peffley and Williams 1985). How people explain national problems can similarly influence their policy opinions and therefore their candidate preferences, as well as their attitudes toward the political process itself. In sum, research into citizens' political explanations represents an important new thrust for public-opinion research.

Notes

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1. The focus on television news is justified, for it is the most preferred source of public-affairs information for the great majority of the U.S. public (see Bower 1985).

2. In any experimental procedure it is imperative that the researcher guard against the effects of demand characteristics—cues in the experimental situation that suggest to participants what is expected of them. Several such precautions were undertaken including a plausible cover story that disguised the true purpose of the study. In addition, the treatment stories were compiled using studio-quality editing equipment so that participants could not know that the stories had been altered. Finally, the aura of the research laboratory was minimized by inviting participants to bring a spouse, friend, or colleague with them. Most did so and the average session size of two means that a participant typically watched the videotape with someone he or she knew. The concern over experimental demand also dictated the choice of a posttest-only design. Had I asked participants for their explanations of the target issue both before and after exposure to the videotape, this would have provided them with a powerful cue as to the researcher's intent, thereby affecting their posttest responses.

3. Responses fell into four general categories: explanations, prescriptions/remedies, descriptions, and expressions of affect. Using this four-fold classification, two independent coders agreed more than 90% of the time.

4. These were initially separate conditions, as we wished to investigate the effects, if any, of the victim's race. In this particular study, the two conditions elicited responses not significantly different. For purposes of this analysis, therefore, they were merged.

5. In this analysis and those that follow, the remaining attributional categories are included in the equation as control variables. This is because the number of causes falling into a particular category inevitably reduces the number falling into other categories.

6. These included an index of conservative-issue opinions, ratings of the president's competence (the degree to which the terms *experienced* and *provides strong leadership* fit Reagan), and ratings of his integrity (the degree to which *honest*, *fair*, and *compassionate* fit Reagan).

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THREAT ESCALATION AND CRISIS STABILITY: A GAME-THEORETIC ANALYSIS

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We develop a model of crisis stability based on a deterrence game. Players are assumed to be able to choose any level of initial cooperation or non-cooperation; the more cooperative player (if there is one) may then choose to retaliate. In a crisis, players are assumed to have escalated their conflict and desire to stabilize it before it explodes. Players can, by threatening their opponent, stabilize the new status quo. In addition, points of threat escalation are identified at which neither player can prevent the opponent from escalating further without threatening the opponent more severely than before the crisis erupted, thereby heating up an already tense situation. Crisis stabilization is aided by being close to the full-cooperation position, though, paradoxically, both players may benefit from having created a crisis that only escalating threats may resolve. We discuss ways of avoiding threat escalation and the increased risk of war that it entails, especially in conflicts between the superpowers.

Prominent in the lexicon of nuclear strategists is the notion of *crisis stability*. Roughly speaking, this is a state of affairs in which neither side in a two-party conflict has a first-strike advantage that would give it the incentive to preempt the other, especially in a crisis in which tensions are high and distrust is rampant.¹

In this paper we shall interpret this concept broadly to include *any* stabilizing forces—not just the lack of a first-strike advantage—in a crisis. For example, if each side can threaten devastating retaliation after a first strike because a substantial portion of its weapons are survivable, this second-strike ability may well deter a first strike and therefore be crisis stabilizing, even though each side can still do better striking first than striking second.

Crisis stability is therefore the struc-

tural feature of conflicts that, even after they have escalated to crisis proportions, permits decision makers to prevent the crisis from exploding. The principal means to avoid an explosion is to ensure that neither side has an incentive to preempt the other in the crisis.²

Nuclear deterrence between the superpowers, as long as it is assured by a second-strike capability, would appear to provide the necessary insurance. What worries the political leaders of each superpower considerably more than a “bolt from the blue” by the other superpower is the possible escalation of a conventional conflict, such as might occur in the Middle East or Western Europe, into a crisis that involves serious threats to their allies or even their own security. Insofar as the ability to stabilize such a conflict lies outside their control, crisis stabilization—at

least on their part—is jeopardized.

Presumably, decision makers in an escalatory situation would prefer to be able to damp down a conflict, or at least contain it, so that a crisis cannot erupt into full-fledged war in which everybody may suffer egregiously. This is an especially frightening problem in a confrontation between nuclear powers. But even in conventional conflicts, such as that between Iran and Iraq today, the human casualties and material damage on both sides may be horrendous.

The ability to stabilize a crisis or contain a conflict, however, may be a mixed blessing. For once it is recognized by the antagonists that disaster may be escapable, they may be more willing to take risks that they would avoid if escape from disaster were impossible (Brams and Kilgour 1987c).

Put another way, crisis stability may encourage provocative behavior, whereas an inability to stabilize crises may induce more cautious choices. In the Cuban missile crisis of October 1962, for example, the leaders of the superpowers assiduously eschewed making explicit nuclear threats as the crisis heightened (Morgan 1986, 178), perhaps in part because each side's second-strike capability at the time was not as great as it is today (notwithstanding the greater vulnerability today of each side's land-based ballistic missiles). In the absence of stabilizing forces, the situation was already extremely delicate, so neither side wanted to do anything to provoke the other into preempting with nuclear weapons.

On balance, however, crisis stability is surely desirable; while it affords each side greater leeway to be provocative, which itself may be upsetting, it makes escalation of a crisis decidedly less likely. Because stability is an eminently game-theoretic concept, it is appropriate to model crisis stability using a game. Accordingly, we shall begin our analysis by setting up a threat game (TG), based

on chicken, to model deterrence. The so-called deterrence equilibrium in this game, as we shall show, may be difficult to sustain if a crisis pushes the players into bellicose postures.

How they can prevent their positions from deteriorating further in a crisis will be one theme of our analysis.³ Specifically, after describing the rules of TG and its equilibrium outcomes, we shall indicate what crisis stabilization entails. We shall then derive threat functions, or lines, showing how the players in a crisis can deter further escalation and then spell out when this task is most trying.

Unfortunately, under certain conditions there are crisis points that cannot be stabilized without an escalation in threats by the players: the threats of reprisal that originally sustained cooperation no longer suffice to stabilize the crisis and thereby to deter preemption past this crisis point. In effect, the players will already have gone too far to make a return to mutual cooperation, or even stabilization, worth their while without threat escalation.

We shall show that a game that requires threat escalation to stabilize near the cooperative outcome is one in which neither player can benefit from conflict in a crisis. In such a game, each player does relatively well at the cooperative outcome, lessening the temptation to defect. However, if the players should drift away from this outcome in a crisis, more dire threats are needed to prevent further deterioration of the crisis.

By contrast, if both players are not particularly advantaged at the cooperative outcome and can do better by escalating their conflict (to some degree), the threats required to restore the status quo do not escalate, tempting each player to foment a (mild) crisis. If the crisis should become severe enough to hurt both players, however, then threat escalation is required to restore the status quo.

We will assess the plausibility of this

game as a model of nuclear deterrence and as a basis for determining when deterrence can be maintained in a crisis. We shall then return to a discussion of the substantive interpretation of this game in light of our findings about crisis stability, particularly in superpower conflict, and conditions for maintaining it. The Cuban missile crisis will be used to illustrate some of the results derived from our game-theoretic model.

The Threat Game

TG is based on the game of chicken that, it has been argued, constitutes an appropriate basic model of deterrence between the superpowers (Brams 1985, chap. 1). This claim has been disputed by Zagare (1985, 1987), who contends that prisoners' dilemma is a better model (but see Brams and Kilgour 1986b and 1988 for an arms-race application of this model).

This dispute, however, is somewhat beside the point because TG adds two significant features to chicken that make it a truly different game (Brams and Kilgour 1987a): (1) the players can make quantitative choices of levels of cooperation (C) or noncooperation (\bar{C}), not just qualitative choices of C or \bar{C} ; (2) once these initial choices, which can be interpreted as levels of nonpreemption or preemption, are made, only the less preemptive player (i.e., the player who chose the lower level of preemption initially, if there was one) can retaliate by choosing a different—and presumably higher—level of noncooperation subsequently. The game terminates whenever the initial levels are the same or, if they are not, after the less preemptive player has retaliated.

To be sure, the rule that allows only the more aggrieved player, who chose a lower level of preemption initially, to retaliate is arbitrary. Our reasoning is that conflict escalates precisely because each player, in turn, is willing and able to raise the ante.

If escalation were unilateral, and one player simply became more and more aggressive without provoking retaliation from the other, then the process would not be two-sided escalation but one-sided aggression.

We are interested in modeling situations in which both players move up the escalation ladder. However, we telescope this process into one in which, after the possible preemption of each player, a single retaliatory countermove is allowed that can "right the balance" for the initially more cooperative player. (Other rounds of escalation could be added, but we think the present simple sequence captures well the process that might trigger further escalation.) To take account of the possibility that this process might have a history that does not start the players off from blissful cooperation, we shall later posit the occurrence of a crisis that transports the players to some less-than-cooperative point and then ask what threats are required to arrest further escalation.

Formally, TG is defined by the following rules:

1. The players do not choose initially between C and \bar{C} but instead choose preemption levels s (row) and t (column) that may range on a continuum between 1 (no preemption) and 0 (maximum preemption). These choices are simultaneous and determine a point on the unit square, defined by vertical coordinate y and horizontal coordinate x (see Figure 1), which we call the *initial position*. It is convenient to assume that before play commences, the *preplay position* of the players is (1, 1), the mutual-cooperation position, but this starting point is not crucial to our analysis.
2. The 2×2 game of chicken is embedded in the unit square so that the four corners of the square correspond to the four "pure" states (r_i, c_j) of chicken,

with r_i giving the payoff of row and c_j the payoff of column (the subscripts i and j give the rankings of payoffs, with 4 being best and 1 being worst):

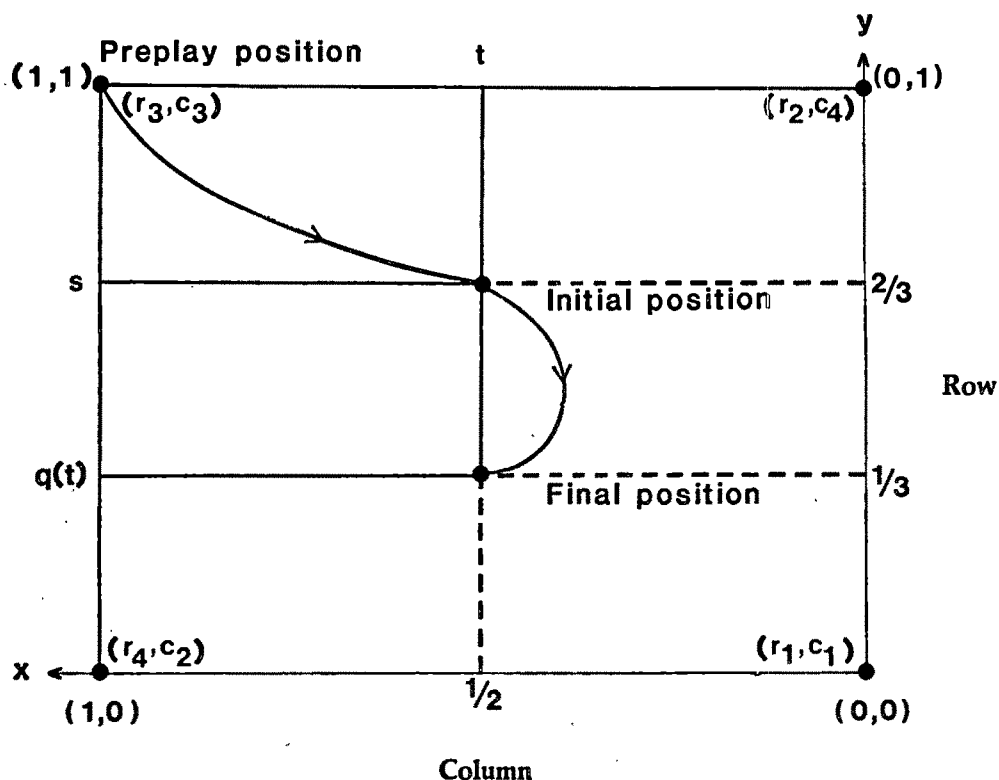
- Both players cooperate (CC)—next best outcome for both players: (r_3, c_3) at upper-left corner in Figure 1.
- One player cooperates and other does not ($C\bar{C}$ and $\bar{C}C$)—best outcome for the player who does not

cooperate and next-worst for the player who does: (r_2, c_4) and (r_4, c_2) at upper-right and lower-left corners, respectively.

- c) Both players do not cooperate ($\bar{C} \bar{C}$)
—worst outcome for both players:
(r_1, c_1) at lower-right corner.

3. In Figure 1, the square is drawn so that the (1, 1) position, corresponding to the CC outcome of chicken, is at the upper left and the (0, 0) position, corre-

Figure 1. The Threat Game



Key:

$$(r_i, c_j) = (\text{payoff to row}, \text{payoff to column})$$
$$r_4, c_4 = \text{best}; r_3, c_3 = \text{next best}; r_2, c_2 = \text{next worst}; r_1, c_1 = \text{worst}$$

s, t = initial strategy choices of row and column, respectively

- $q(t)$ = subsequent strategy choice of row (more cooperative player initially)

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sponding to the $\overline{C}\overline{C}$ outcome of chicken, is at the lower right. (This positioning of outcomes corresponds to the usual one for 2×2 games, but it reverses the ordering of values on the horizontal axis: values of x increase rather than decrease from right to left.) Points of the square not at the four corners represent "mixtures" of two or more outcomes of chicken; the payoffs that these mixtures yield are defined in Rule 5 below, and illustrated subsequently.

4. If $s = t$, the game terminates at point (t, s) in the unit square. If $s > t$, the more cooperative player, row, retaliates against column by changing the level of cooperation, according to the retaliation function $q(t)$, to make the final position of the game $(t, q(t))$. Row's retaliation function thus depends on column's initial choice t ; however, it is the value of s , as compared with t , that determines whether row or column (if either) can retaliate. If $s < t$, column is the player who is more cooperative initially and thereby can retaliate; column's level of cooperation changes from t to $p(s)$, where $p(s)$ is column's retaliation function, and the final position is $(p(s), s)$. The players' retaliation functions are chosen at the same time as their initial choices, s and t . Much of the rest of the paper will be devoted to showing what initial choices and retaliation functions are optimal, in a certain sense, for the players in TG.
5. Payoffs to row (R) and column (C) are defined by both the chicken payoffs at the four corners of the unit square and the actual final position (x, y) on the square:⁴

$$P_R(x, y) = xy r_3 + x(1 - y) r_4 \\ + (1 - x) y r_2 + (1 - x)(1 - y) r_1$$

$$P_C(x, y) = xy c_3 + (1 - x) y c_4 \\ + x(1 - y) c_2 + (1 - x)(1 - y) c_1$$

At the four corners of the unit square, P_R and P_C exactly coincide with the corresponding payoffs of chicken. At any other point on the square, P_R and P_C are averages of the four payoffs at the corners, weighted according to the position of the point relative to the corners.

It is worth noting that the payoff functions are bilinear (linear in each coordinate): in calculating payoffs at (x, y) , the payoffs at each corner are weighted by the product of the distances, parallel to the axes, from (x, y) to the opposite corner.⁵ The definitions imply that the payoffs are continuous functions of the final position (x, y) on the unit square.

As an illustration of this weighting system, consider the coordinates of the final position shown in Figure 1, $x = 1/2$ and $y = 1/3$. The weights of the four corner values of chicken in $P_R(x, y)$ and $P_C(x, y)$ are

$$(1, 1): xy = 1/6 \quad (0, 1): (1 - x)y = 1/6 \\ (1, 0): x(1 - y) = 1/3 \quad (0, 0): (1 - x)(1 - y) = 1/3$$

which sum to 1.

The two lower payoffs in Figure 1, (r_4, c_2) and (r_1, c_1) , which correspond to corners equidistant from $(1/2, 1/3)$, are equally weighted by factors of $1/3$ in P_R and P_C . Similarly, the two upper payoffs, (r_3, c_3) and (r_2, c_4) , which also correspond to corners equidistant from $(1/2, 1/3)$, are equally weighted by factors of $1/6$. Not unreasonably, the upper factors have half as much "pull" as the lower factors, for they correspond to twice the distance from $(1/2, 1/3)$ on the vertical axis.

Different weighting factors would deform the continuous surface generated by our payoff functions. But because these deformations would remain anchored at the four corners of the unit square, they would not alter the basic nature of the Nash (1951) equilibrium results that we shall describe shortly.

An advantage offered by the present

formulation, in addition to mathematical tractability, is that the four weights in P_R and P_C can be interpreted as probabilities because they are nonnegative and sum to 1. Consequently, P_R and P_C may be thought of as expected values, the corner values being the four "pure" states of the game that can arise. As probabilities of being at the four corners, the four factor weights, when multiplied by a player's payoffs at each corner, give this expected payoff at any point of the unit square. It is worth noting that these factor weights are functions of the mixed (i.e., probabilistic) strategies of the players, who can be viewed as choosing strategy mixtures $x/(1-x)$ and $y/(1-y)$.

In a nonprobabilistic interpretation, which we prefer because it leaves unambiguous who initially is the more cooperative player, the unit square of Figure 1 might be considered a board game. The players choose (x, y) coordinates, according to Rules 1-4, that define a point on this board. As given by Rule 5, the payoffs to the players are the heights of the continuous surfaces, above the (x, y) plane, generated by P_R and P_C . A Nash equilibrium then has the property that the player who was more cooperative initially cannot increase his or her payoff by changing the coordinate when the opponent's coordinate is held constant.

TG has several qualitatively different Nash equilibria, but only one equilibrium, which we call the deterrence equilibrium (DE) (Brams and Kilgour 1987a), involves no preemption: $(x, y) = (1, 1)$, with payoffs (r_3, c_3) to row and column. Assume, henceforth, that the payoffs are normalized so that $r_4 = c_4 = 1$, $r_1 = c_1 = 0$, and the intermediate payoffs satisfy the following inequalities: $0 < r_2 < r_3 < 1$ and $0 < c_2 < c_3 < 1$. Then the initial strategies and retaliation functions that render this outcome stable are

$$\begin{aligned} s &= 1, q(t) \leq q_1(t); \\ t &= 1, p(s) \leq p_1(s) \end{aligned}$$

where

$$\begin{aligned} q_1(t) &= \frac{c_3 - tc_2}{1 - t(1 - c_3 + c_2)}, 0 \leq t < 1 \text{ and} \\ p_1(s) &= \frac{r_3 - sr_2}{(1 - s(1 - r_3 + r_2))}, 0 \leq s < 1. \end{aligned}$$

At DE both players never preempt ($s = t = 1$), but row threatens retaliation at level $q(t) \leq q_1(t)$ if preempted at level $t < 1$; and column threatens retaliation at level $p(s) \leq p_1(s)$ if preempted at level $s < 1$. The "1" subscripts on the right sides of each retaliation-function inequality indicate that the preempted player initially chooses $s = y = 1$ (row) or $t = x = 1$ (column). Note that the retaliation levels, $q(t)$ and $p(s)$, row and column threaten are functions of the levels of preemption, t and s , of their opponents; we shall have more to say about this functional dependence later.

Like other Nash equilibria, DE possesses the property of "mutually fulfilled expectations": the choice of $q(t) \leq q_1(t)$ by row and $p(t) \leq p_1(t)$ by column associated with DE ensures that any level of preemption by each player's opponent will be at least as costly (after retaliation) as no preemption. Hence, the players do at least as well, and generally better (if the inequalities are strict), by choosing no preemption initially ($s = t = 1$).

In an earlier model of deterrence (Brams and Kilgour 1985a, 1985b), we did not permit retaliation to be a function of the level of preemption of the more preemptive player. Instead, in a deterrence game (without variable retaliation), we showed that "optimal deterrence" could be achieved by threats of retaliation independent of the actual level of preemption.⁶ The introduction of *variable* retaliation may be viewed as a refinement of the earlier model in that it ties retaliatory threats to the level of preemption rather than assuming that only one threat—sufficient to deter maximal preemption—applies to all situations. In a real-life con-

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text, this refinement is echoed in the abandonment by the United States of "massive retaliation" in favor of "flexible response" or "graduated deterrence" around 1960 because the former doctrine was considered too blunt (and incredible) a retaliatory policy for anything but an all-out Soviet nuclear attack.

Because one player can, by choice of his or her DE strategy, make the compromise outcome at least as attractive for the opponent as preemption, and vice versa, there is a strong reason for both players to settle on DE. Because DE depends fundamentally on threats, however, it is not surprising that it is neither perfect nor subgame-perfect in the sense of Selten 1975.⁷ This is because if deterrence for any reason should fail, it may be irrational to retaliate because retaliation could lead to a worse outcome for the threatener, if obliged to carry out the threat, as well as for the player who preempted and thereby provoked retaliation.

The problem of imperfectness is "solved," at least in theory, if the players can irrevocably precommit themselves to carrying out their threats. In the case of nuclear deterrence between the superpowers, both sides have institutionalized detailed procedures for responding to a nuclear attack that are designed to ensure—so far as possible—that retaliation will occur, even if communication, command, control, and intelligence (C³I) capabilities are damaged by the attack (Bracken 1983).⁸

We shall assume that the rules of TG permit precommitments by the players that are regarded as credible—the threatened player believes that the threatener will in fact retaliate at the level that player says he or she will. We shall now take a closer look at the nature of the retaliatory threats necessary to sustain not only DE but also—should a crisis cause the players' positions on the unit square to deteriorate (reasons why this might happen will be given later)—a position below

and to the right of DE in Figure 1. It is important to bear in mind that while the theoretical solutions we offer suggest how threats might be tailored to provocations in order to render them more credible, we know of no practical procedures for operationally matching actual threats to the real-life preemption they are intended to deter.

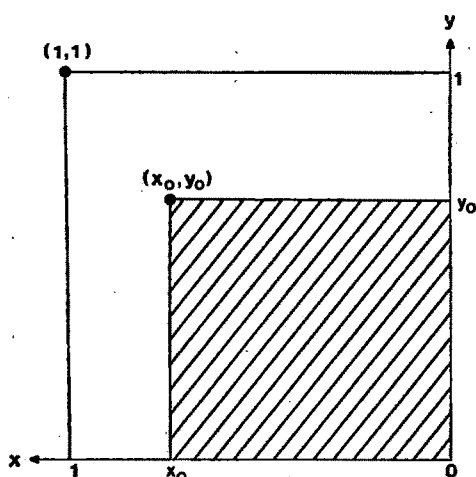
Deterrence in a Crisis

We assumed earlier that the preplay position of the players in TG is (1, 1). A crisis is an event or series of events that changes this position to (x_0, y_0) , where either $x_0 < 1$ or $y_0 < 1$ or both—that is, at least one player has acted so that $(x_0, y_0) \neq (1, 1)$. We assume for now that the new preplay position is near but not at (1, 1), in a sense to be made precise later.

In essence, the crisis has created a situation in which the players find themselves, more or less simultaneously, to be more hostile toward each other than previously, perhaps because one player partially preempted and the other responded with some noncooperative countermove. Indeed, a crisis suggests at least a partial failure of deterrence, which may occur for any of a variety of reasons, including domestic political considerations, misperceptions by the players, lack of communication, and the like. The question we address here is how deterrence can be restored—what kinds of threats are needed to prevent further deterioration of the crisis.

If the players wish to return to DE at (1, 1) eventually, an option for each is to try to do so immediately. By virtue of the fact that DE is a Nash equilibrium, if either player credibly announces his or her DE strategy, the opponent can do no better than to return to that player's own DE strategy. Thus, if column were to announce $t = 1$, $p(s) \leq p_1(s)$, row would

Figure 2. Reduced Game Board at (x_0, y_0)



maximize its payoff by returning to $s = 1$. The desire to protect against possible future preemption by column would then motivate row to threaten retaliation as well, that is, $q(t) \leq q_1(t)$.

One problem with effecting stabilization immediately at DE is that it would necessarily involve threats against the status quo (x_0, y_0) . By assumption, the game is not at $(1, 1)$ after the crisis has occurred, so some intimidation would be required to return to this point. In the middle of a crisis, however, a threat of retaliating against the status quo might indeed be dangerous because it could be interpreted as moving up the escalation ladder. Moreover, to create an incentive for an opponent to return to $(1, 1)$, the threatener would have to change to a cooperative position; this may not be easy to do in a crisis.

It may be preferable for the players to try first to stabilize the status quo (x_0, y_0) , postponing further ameliorative measures for restoring the nonpreemption position at $(1, 1)$ until the crisis atmosphere has cleared. This might be done by indicating a temporary acceptance of the current

position and using presumably milder threats to deter further preemption.

Such a tactic, in forestalling a deepening of the crisis by arresting it at the point where it currently is, affords the players the opportunity to buy more time. Yet it poses a new question: Is it always possible, given a preplay position near but not at full cooperation, for one or both players to stabilize (x_0, y_0) using threats less provocative than those supporting full deterrence at $(1, 1)$?

If the players perceive their problem as one in which the new status quo (x_0, y_0) brought on by the crisis is the best (i.e., most cooperative) position they can hope for in the short term, then we may consider the revised threat game to be a game played on a reduced "game board," comprising those points that lie below and to the right of (x_0, y_0) rather than $(1, 1)$ (see Figure 2). In the Appendix, this short-term deterrence problem is modeled by a reduced threat game, or simply reduced game, wherein the preplay position is assumed to be (x_0, y_0) .

It is shown that, provided (x_0, y_0) is not too far from $(1, 1)$ —specifically, $x_0 > x^* = r_2/(1 - r_3 + r_2)$ and $y_0 > y^* = c_2/(1 - c_3 + c_2)$ —this game has a deterrence equilibrium, analogous to DE, defined by

$$s = y_0, q(t) \leq q_{x_0, y_0}(t)$$

$$t = x_0, p(x) \leq p_{x_0, y_0}(s)$$

where

$$p_{x_0, y_0}(s) = \frac{P_R(x_0, y_0) - sr_2}{1 - s(1 - r_3 + r_2)}, 0 \leq s < y_0$$

and

$$q_{x_0, y_0}(t) = \frac{P_C(x_0, y_0) - tc_2}{1 - t(1 - c_3 + c_2)}, 0 \leq t < x_0$$

It is this reduced deterrence equilibrium (RDE) at (x_0, y_0) , now to be discussed, that will serve as the cornerstone of our model of crisis stabilization.

Crisis Stabilization

We now examine more closely the properties, feasibility, and strategic implications of RDE. A few initial observations help to set the stage. First, notice that $p_{1,1}(s) = p_1(s)$ and, similarly, $q_{1,1}(t) = q_1(t)$. This means that, as (x_0, y_0) approaches $(1, 1)$, RDE at (x_0, y_0) approaches DE, the deterrence equilibrium of the threat game.

Other mathematical properties of RDE in the reduced game are important. Like $p_1(s)$ and $q_1(t)$ in the original game, $p_{x_0, y_0}(s)$ and $q_{x_0, y_0}(t)$ are continuous, strictly increasing, and convex: as the levels of initial cooperation, s and t , increase, the minimal threats of retaliation, $1 - p_{x_0, y_0}(s)$ and $1 - q_{x_0, y_0}(t)$, necessary to deter will decrease at a decreasing rate. In other words, players must be relatively *more* threatening at low levels of initial provocation than at high levels, sometimes threatening even greater retaliation than the provocation at these levels. The nature of such threats is analyzed in detail in Brams and Kilgour 1987a and compared to the findings of Axelrod 1984.

Perhaps the simplest way to understand all these deterrence equilibria is by means of *threat lines*. Each player threatens the other with just enough retaliation (at the threshold) to make the threatened player prefer no preemption to unilateral preemption at any level. The threat line shows how this minimum level of retaliation depends on the level of preemption.

For example, at DE in the threat game, column threatens to punish any level of preemption by row (i.e., any $s < 1$) by cooperating at level no more than $p_1(s)$. Column's threat line (really a curve) at this equilibrium, called the *basic threat line*, is given by $x = p_1(y)$ for $0 \leq y < 1$ and is shown in Figure 3. Similarly, column's threat line supporting the RDE at (x_0, y_0) in the reduced game is given by $x = p_{x_0, y_0}(y)$ for $0 \leq y < y_0$. The upper bound, y_0 , on y means that at RDE,

column does not threaten to retaliate against the status quo y_0 , just as column does not retaliate against $y = 1$ at DE in the original game.

In Figure 3 we illustrate several threat lines of column (in addition to the basic threat line) for several typical RDEs. Row has a corresponding threat line at each RDE; in general, the threat line of row supporting an RDE at (x_0, y_0) is given by $y = q_{x_0, y_0}(x)$ for $0 \leq x < x_0$. Note that the threat lines supporting RDE at (x_0, y_0) always begin at (x_0, y_0) . It is shown in the Appendix that a player's threat lines supporting RDEs at different status quo points (x_0, y_0) never cross—they are either coincident or disjoint, as illustrated by the "parallel" threat lines of Figure 3.

In order that it be rational for the less preemptive player to retaliate against the more preemptive player (Brams and Kilgour 1987a), (x_0, y_0) must be sufficiently close to $(1, 1)$ —that is, satisfy $x_0 > x^*$ and $y_0 > y^*$, where the lower bounds x^* and y^* are as defined in the Deterrence in a

Figure 3. Several Threat Lines of Column

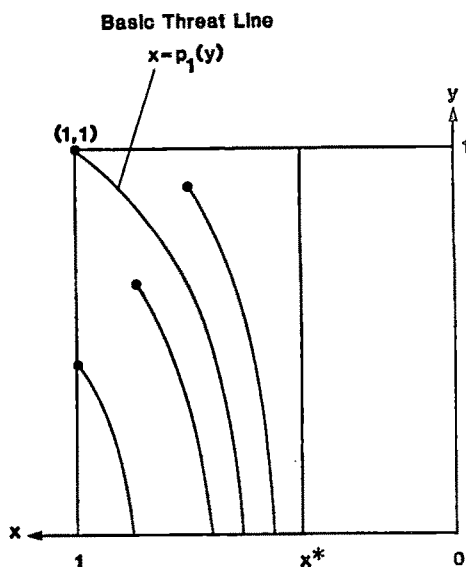
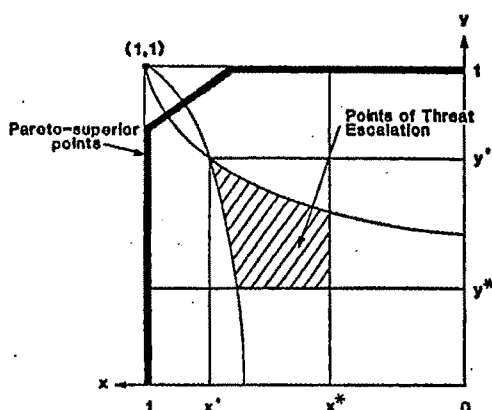


Figure 4. Configuration of Basic Threat Lines in Which Crisis Stabilization Without Threat Escalation Is Always Possible near (1, 1)



Crisis section. At higher levels of preemption, by comparison, retaliation by the less preemptive player sufficient to deter the opponent is irrational in the sense that it hurts the retaliator as well as the opponent. In effect, we consider only whether a crisis can be stabilized at (x_0, y_0) in the reduced game when rational retaliatory threats by both players are possible.

We can now answer the question posed previously. When can one or both players attempt immediate crisis stabilization of a status quo point (x_0, y_0) without threat escalation? We shall consider two cases. In the first case, the basic threat lines of both row and column take the form shown in Figure 4.

Observe that all points in the unit square near (1, 1) are either below column's basic threat line or to the right of row's (or both). Therefore, at least one player—and sometimes both, namely, in the region of overlap between (1, 1) and (x', y') —can stabilize a crisis at (x_0, y_0) with the same threats meant to deter preemption at (1, 1).

The case illustrated in Figure 4 occurs whenever, in the neighborhood of (1, 1), x

$= p_1(y)$ lies to the right of $y = q_1(x)$. It is shown in the Appendix that this takes place if and only if

$$\frac{1 - c_3}{c_3 - c_2} > \frac{r_3 - r_2}{1 - r_3} \quad (1)$$

Notice that for fixed c_2 and c_3 , Inequality 1 holds if r_3 is relatively close to r_2 , and similarly for fixed r_2 and r_3 . In general, Inequality 1 holds when r_3 and c_3 are relatively small—that is, when the players do not value the status quo very highly. For example, if $r_3 = .5$, $c_3 = .6$, and $r_2 = c_2 = .3$, then Inequality 1 is satisfied.

If Inequality 1 does not hold, Figure 5 applies and there are initial points close to (1, 1)—specifically, near the main diagonal where the threat lines diverge—from which neither player can attempt crisis stabilization without escalating its threats. On the other hand, if (x_0, y_0) is sufficiently close to one of the edges of the unit square, this status quo point will be either below column's basic threat line (near the vertical axis), or to the right of row's (near the horizontal axis), and one player can stabilize (x_0, y_0) .

A point of threat escalation is one that is neither below column's basic threat line nor to the right of row's, which in Figure 5 means below and to the right of (1, 1). In Figure 4 there are also points of threat escalation below and to the right of (1, 1). Instead, they are points below and to the right of the interior point of intersection (x', y') of the two basic threat lines.

Actually, all points in the regions where the curves "flare outward" (from (x', y') in Figure 4 and from (1, 1) in Figure 5) are points of threat escalation. It is noteworthy that, whether Figure 4 or Figure 5 applies, crisis stabilization is always facilitated if (x_0, y_0) falls near one of the edges of the unit square. This is because the pre-emptor has more to lose and is thus more vulnerable to threats. Hence, a crisis in

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which both players are more or less equally noncooperative is generally harder to stabilize—except when Inequality 1 holds and the players are close to $(1, 1)$ and therefore “protected” (see Figure 4).

In general, crisis stabilization is enhanced when the advantages of mutual cooperation in the threat game are substantial for the players and, in the reduced game, the status quo at (x_0, y_0) is not too far from $(1, 1)$. In addition, crisis stabilization is abetted if (x_0, y_0) significantly favors one player; this makes the opponent's threat of retaliation potentially more harmful and hence likely to be taken more seriously.

There is one curious aspect to crisis instability in the neighborhood of $(x, y) = (1, 1)$ that suggests why players may have difficulty when the basic threat lines flare out as in Figure 4. As shown in the Appendix, the expected payoffs, $P_R(x_0, y_0)$ and $P_C(x_0, y_0)$, may actually exceed their values of r_3 and c_3 , respectively, when $(x_0, y_0) \neq (1, 1)$. That is, both players may be able to improve their payoffs at points on the game board below and to the right of $(1, 1)$, where the players are less than fully cooperative.

The dark lines shown in Figure 4, along the top and left edges of the square except near $(1, 1)$, indicate the Pareto-superior outcomes of the threat game. (From a Pareto-superior point, neither player can do better unless the other player does worse.) In particular, the Pareto-superior outcomes (on the oblique line) between the two basic threat lines are better for both players than (r_3, c_3) . In fact, all the points in the lens-shaped region between the two basic threat lines are Pareto improvements over (r_3, c_3) , so the players would appear to have good reason to foment crises that drive them into this region and toward the oblique Pareto-superior line, beyond which both players do worse rather than better.

When Inequality 1 is reversed, the oblique, Pareto-superior line of Figure 4

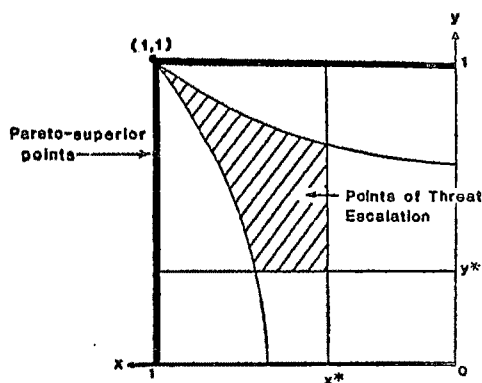
vanishes, resulting in Figure 5 and points of threat escalation near $(1, 1)$. These points correspond to outcomes Pareto-inferior to (r_3, c_3) ; moreover, one must threaten worse reprisals than at $(1, 1)$ to make it rational to stay at them.

The level of threats required in the reduced game to stabilize crises at threat-escalation points always exceeds the basic threats in the threat game that stabilize $(1, 1)$ and make it a deterrence equilibrium. Thus, threats at crisis points $(x_0, y_0) \neq (1, 1)$ but close to $(1, 1)$ in Figure 5 must be more severe than those at $(1, 1)$ to prevent further deterioration of the crisis.

We believe that our theoretical findings accord with reality in certain strategic situations. For instance, as the Soviets were about to install nuclear-armed missiles in Cuba in October 1962, President Kennedy deemed his previous verbal warnings no longer adequate to alter their behavior. Claiming his earlier exhortations had fallen on deaf ears, he escalated the conflict, imposing a blockade and contemplating even stronger actions, such as a possible air strike against the missile sites.

It seems, in such crises, that both sides may initially perceive an advantage in

Figure 5. Configuration of Basic Threat Lines in Which Crisis Stabilization Without Threat Escalation Is Not Possible near $(1, 1)$



raising the stakes, as suggested by Figure 4, but not to the point of an explosion. Although the tension in such crises may be unsettling, to say the least, both sides may well foresee benefits if the lid can be kept on (i.e., if they do not move past the oblique line in Figure 4). Indeed, following the missile crisis and sobered by it, the United States and the Soviet Union quickly reached an agreement on a hot line, which they hoped would defuse future crises, and also signed the Partial Nuclear Test Ban Treaty.

These conciliatory actions surely produced a Pareto improvement in U.S.-Soviet relations (assuming the superpowers had moved past the Pareto-superior line and did better when they stepped back from the abyss) even if this improvement did not occur in the heat of the crisis. As in the application of many theoretical models, our model's "fit" may not be immediate but can be interpreted to occur over time.

Paradoxically, perhaps, a crisis that does, in the end, abate may have stabilizing long-term effects, even though, like the Cuban missile crisis, it may be a terrifying experience through which to live (Kavka 1986, 236-37). In terms of our model, both sides may see a short-term advantage in escalating a conflict, the increased risk of war being offset by the potential benefits of gaining a significant edge over the other side. However, as each side takes actions that bring both closer to the precipice, the dangers become more palpable and each seeks a way to step back.

Thus, while there may be an incentive to depart from the cooperative outcome in the threat game initially, this desire may set in motion a countervailing force: as each side becomes more threatening and conflict becomes imminent, fears arise that induce both sides to try to wind down the crisis. Then, not desiring to be so close to the precipice, each side finds it rational to retreat to $(1, 1)$ —from which,

unfortunately, they will probably be drawn again by the possibility of higher payoffs.

Conclusions

It is important to emphasize that crisis stabilization is always possible in the threat game if either player credibly threatens retaliation at a level given by the deterrence equilibrium of this game. The new question posed in this paper concerns simultaneous departures: If both players should, because of a crisis, find themselves at point $(x_0, y_0) \neq (1, 1)$ of the unit square, can they stabilize this point using milder threats, making it a new status quo from which neither player will have an incentive to depart unilaterally?

The calculations of the threat game can be carried over to this reduced game, with (x_0, y_0) now assumed to be the preplay position. If the goal of the players is first to stabilize (x_0, y_0) and not necessarily to induce a return to $(1, 1)$ immediately, then this goal cannot always be realized without escalating threats at (x_0, y_0) , which may well aggravate a crisis already fraught with the danger of a conflagration.

This goal of temporary stabilization seems a sensible one for the players to aspire to if preventing a further deterioration of their positions in a crisis is a *sine qua non* of eventually easing out of the crisis and moving back to the status quo ante at $(1, 1)$. Numerous crises in international politics have been abated by just such incremental steps.

For example, the eventual settlement of the Cuban missile crisis in October 1962 depended initially on the removal by the Soviets of the missiles they were about to install in Cuba. The United States provided the incentive when it escalated its verbal warnings to a blockade. Once the Soviets promised to remove the missiles, the United States agreed to lift the blockade, after which the crisis subsided.

Our game-theoretic analysis shows

that crisis stabilization may not always be possible, even in the case of small departures from (1, 1), without threat escalation. In general, such escalation is necessary whenever both players do definitely worse in straying from (1, 1).

But this may not always be the case. In fact, by precipitating a crisis, both players may be able to improve their payoffs, perhaps for domestic political reasons. Eventually, however, as the crisis worsens, threats may be escalated to prevent a further deterioration of relations, which may in turn push the two sides back to more conciliatory stances out of fear that their conflict could get out of hand. Yet, the milder threats required to stabilize the cooperative outcome at $(x, y) = (1, 1)$ may tempt the players once again to defy these threats and take risks for the higher payoffs, eventually leading to points of threat escalation where they both suffer.

The inducement to escalate threats in a crisis is heightened if the cooperative outcome is valued highly by the players, giving them less reason to depart initially to try to improve their lot. But even then, a surprising condition can facilitate crisis stabilization: the crisis places the players in asymmetrical positions whereby one player is substantially more antagonistic, or preemptive, than the other at (x_0, y_0) . The more cooperative player is then in a position to reduce his or her threats, indicating (temporary) acceptance of (x_0, y_0) and thereby stabilizing it.

In the Cuban missile crisis, the Soviets initially reaped a significant advantage by their preemption, but the United States could threaten them with strong military reprisal, both of a conventional kind against Cuba and of a nuclear kind against the Soviet Union itself (Trachtenberg 1985). More perilous would be a potential crisis with rapid escalation on both sides and in which neither side could pose an overwhelmingly greater threat against the other, perhaps because the confrontation occurred in neither's

"sphere of influence."

It is perhaps fortunate that most preemptive moves made by the superpowers have been against foes that were perceived to be within their spheres of influence. The greatest danger, according to our model, is when there is sudden and serious escalation on both sides, which seems most likely to occur when the confrontation is outside either's immediate sphere of influence, such as in the Middle East, or even outer space.

Stabilizing such a crisis without threat escalation, much less returning to the status quo ante, may be impossible for rational players. We hope that a recognition that there are threat-escalation points and that there may be no escape from them without aggravating the crisis further, will help to avert such crises.

Appendix

We begin by defining various "deterrence games," specifying the strategy choices available to the players, the rules for determining final outcomes, and the payoff functions. The game board of each game is its set of possible final outcomes. For example, the threat game (TG) has the unit square $[0, 1]^2$ as its game board, and its strategy choices, final outcome, and payoffs are as given in Table A-1— (1_S) , (1_F) , and (1_P) , respectively. Note that r_2 , r_3 , c_2 , and c_3 are constants satisfying $0 < r_2 < r_3 < 1$ and $0 < c_2 < c_3 < 1$.

Fix x_0 and y_0 satisfying $0 < x_0 < 1$ and $0 < y_0 < 1$. When the game board is reduced from the unit square to $[0, x_0] \times [0, y_0]$, the problem faced by the players can be modeled by a reduced game. We define the reduced TG at (x_0, y_0) to be Game 2 of Table A-1. Game 2 is played just as the original game; the only difference is in the "shrinking" of (x, y) by

Table 1. Four Deterrence Games

	Game 1 Threat Game (TG)	Game 2 Reduced TG at (x_0, y_0) Board Reduction	Game 3 Reduced TG at (x_0, y_0) Strategy Transformation
Strategies (S)			
Row	$s \in [0, 1]$ and, if $s > 0$, $q: [0, s] \rightarrow [0, 1]$	same as 1S	$s' \in [0, y_0]$ and, if $s' > 0$, $q': [0, x_0 s' / y_0] \rightarrow [0, 1]$
Column	$t \in [0, 1]$ and, if $t > 0$, $p: [0, t] \rightarrow [0, 1]$		$t' \in [0, x_0]$ and, if $t' > 0$, $p': [0, y_0 t' / x_0] \rightarrow [0, 1]$
Final Outcome (F)	$(x, y) = \begin{cases} (p(s), s) & \text{if } s < t \\ (t', s') & \text{if } s = t \\ (t, q(t)) & \text{if } s > t \end{cases}$	$(x', y') = (x_0 x, y_0 y)$, where (x, y) same as 1F	$(x', y') = \begin{cases} (p'(s'), s') & \text{if } s' < t' \\ (t', s') & \text{if } s' = t' \\ (t', q'(t')) & \text{if } s' > t' \end{cases}$ where $s^* = s' / y_0$ and
Payoffs (P)			
Row	$P_R(x, y) = xy(r_3)$ $+ x(1 - y)(1)$ $+ (1 - x)y(r_2)$ $+ (1 - x)(1 - y)(0)$ $= x + yr_2$ $- xy(1 - r_3 + r_2)$	$P_R(x', y')$	same as 2P
Column	$P_C(x, y) = y + xc_2$ $- xy(1 - c_3 + c_2)$	$P_C(x', y')$, where P_R and P_C same as 1P and (x', y') same as 2F	same as 2P

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factors of x_0 and y_0 in (2_F) to obtain a final outcome that always lies on the reduced game board.

It is possible to think of Game 2, the reduced TG at (x_0, y_0) , as being played entirely within the new game board. This is so because Game 3 is equivalent to Game 2, using the transformations $s' = sy_0$, $t' = tx_0$, $q'(t') = y_0q(t'/x_0)$, and $p'(s') = x_0p(s'/y_0)$. Although Game 3 might be seen as the more natural way to reduce TG to the game board $[0, x_0] \times [0, y_0]$, we shall continue to work with Game 2 for convenience. Similarly, Game 4 is strategically equivalent to Game 2, for the strategy sets and resulting position (x, y) are identical in both, and the payoffs in Game 4 are constant multiples of the payoffs in Game 2. Thus Games 2, 3, and 4 are strategically identical.

We next determine the precise conditions under which Games 1 and 2 are equivalent. First compare the payoff functions of Games 1 and 4 for row only—the situation for column is analogous. From 4P and 2F it is easy to verify that

$$\begin{aligned} Q_R(1, 1) &= [x_0 + y_0r_2 - x_0y_0(1 - r_3 + r_2)]/x_0 \\ Q_R(0, 1) &= y_0r_2/x_0 \\ Q_R(1, 0) &= 1 \\ Q_R(0, 0) &= 0 \end{aligned}$$

Define $r'_2 = Q_R(0, 1)$ and $r'_3 = Q_R(1, 1)$. Then

$$Q_R(x, y) = xy(r'_3) + x(1 - y)(1) + (1 - x)y(r'_2) + (1 - x)(1 - y)(0) \quad (\text{A-1})$$

because the right side of this equation equals

$$\begin{aligned} &\{xy[x_0 + y_0r_2 - x_0y_0(1 - r_3 + r_2)] + x(1 - y)x_0 \\ &\quad + (1 - x)y[y_0r_2]\}/x_0 = [xx_0 + yy_0r_2 \\ &\quad + xx_0yy_0(1 - r_3 + r_2)]/x_0 \\ &= P_R(xx_0, yy_0)/x_0 = Q_R(x, y) \end{aligned}$$

because of 2F and 4P.

Comparison of Equation A-1 with 1P shows that row faces the same strategic problem in Game 4 as in Game 1, pro-

vided that $0 < r'_2 < r'_3 < 1$. Because

$$\begin{aligned} r'_3 - r'_2 &= [x_0 + y_0r_2 - x_0y_0(1 - r_3 + r_2)]/x_0 \\ &\quad - y_0r_2/x_0 = 1 - y_0(1 - r_3 + r_2) > 0 \end{aligned}$$

the games are strategically equivalent for row if and only if $r'_3 < 1$, which is equivalent to

$$x_0 + y_0r_2 - x_0y_0(1 - r_3 + r_2) < x_0$$

Because $y_0 > 0$, this inequality holds if and only if $x_0 > r_2/(1 - r_3 + r_2) = x^*$. Combining with the analogous conditions for column's payoffs, we conclude that Game 1 and Game 2 are strategically equivalent if and only if $x_0 > x^*$ and $y_0 > y^*$.

Now assume that $x_0 > x^*$ and $y_0 > y^*$. Our previous analysis of Game 1 (TG) applies equally to the reduced TGs at (x_0, y_0) because of the strategic equivalence we have demonstrated. For Game 4, the deterrence equilibrium is

$$(\text{DE}) \quad s = 1, q(t) \leq q_1(t) \text{ and } t = 1, p(x) \geq p_1(x)$$

where

$$p_1(s) = \frac{r'_3 - sr'_2}{1 - s(1 - r'_3 + r'_2)}, \quad 0 \leq s < 1,$$

and $q_1(t)$ is similar. Of course the DE is identical in Game 2, for 4S and 2S are the same. The transformation given above shows, after some simplification, that in Game 3 DE becomes

$$\begin{aligned} s' &= y_0, q'(t') \leq q_{x_0, y_0}(t') \text{ and} \\ t' &= x_0, p'(s') \leq p_{x_0, y_0}(s') \end{aligned}$$

where

$$p_{x_0, y_0}(s') = \frac{P_R(x_0, y_0) - s'r_2}{1 - s'(1 - r_3 + r_2)}, \quad 0 \leq s' < y_0$$

and

$$q_{x_0, y_0}(t') = \frac{P_C(x_0, y_0) - t'c_2}{1 - t'(1 - c_3 + c_2)}, \quad 0 \leq t' < x_0$$

Here, $P_R(x_0, y_0)$ and $P_C(x_0, y_0)$ are as given by 1p.

Finally, we show that, near (1, 1), column's basic threat line ($p_1(y)$, y) lies to the right of row's basic threat line (x , $q_1(x)$) if and only if

$$\frac{1 - c_3}{c_3 - c_2} > \frac{r_3 - r_2}{1 - r_3} \quad (\text{A-2})$$

(see Figures 4 and 5). Recall that

$$p_1(y) = \frac{r_3 - yr_2}{1 - y(1 - r_3 + r_2)}, \quad 0 \leq y < 1$$

and

$$q_1(x) = \frac{c_3 - xc_2}{1 - x(1 - c_3 + c_2)}, \quad 0 \leq x < 1$$

Since $\lim_{y \rightarrow 1} p_1(y) = \lim_{x \rightarrow 1} q_1(x) = 1$, both treat lines pass through (1, 1).

We now show that at (1, 1), the slope of (x , $q_1(x)$) is less than that of ($p_1(y)$, y) if and only if Inequality A-2 holds. Because

$$\frac{dq_1}{dx} = \frac{(c_3 - c_2)(1 - c_3)}{[1 - x(1 - c_3 + c_2)]^2}$$

we have that the limiting slope of (x , $q_1(x)$) at $x = 1$ is

$$\left. \frac{dq_1}{dx} \right|_{x=1} = \frac{(c_3 - c_2)(1 - c_3)}{(c_3 - c_2)^2} = \frac{1 - c_3}{c_3 - c_2}$$

Similarly,

$$\left. \frac{dp_1}{dy} \right|_{y=1} = \frac{1 - r_3}{r_3 - r_2}$$

so that the limiting slope of ($p_1(y)$, y) at $y = 1$ is

$$\left(\left. \frac{dp_1}{dy} \right|_{y=1} \right)^{-1} = \frac{r_3 - r_2}{1 - r_3}$$

Comparison of these two slopes now yields inequality A-2.

It is not difficult to verify that any point (x , y) on column's threat line at (x_0 , y_0) satisfies $P_R(x, y) = P_R(x_0, y_0)$. (This shows that threat lines never intersect; they are either coincident or disjoint.) Now a threat line for column can be

drawn through each point of the unit square. It is easy to show that for any point (x_0 , y_0), the threat line separates those outcomes that satisfy $P_R(x, y) > P_R(x_0, y_0)$ from those for which $P_R(x, y) < P_R(x_0, y_0)$. The outcomes preferred by row are always on the side of the threat line containing (1, 0). These facts also apply to column's payoff and row's threat lines.

These observations provide a way to identify Pareto-superior outcomes. If the two threat lines from a point "flare away" (as in Figure 5), the point is Pareto-superior to all those points below and to the right. Otherwise it is not (Figure 4). Proceeding as in the derivation of Inequality A-2 yields the sets of Pareto-superior outcomes shown in Figures 4 and 5.

Notes

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1. We use *preemption* to mean initial attack, the implication being that each side may attack first out of fear of being attacked. This fear, as we shall show, may be well grounded in the threat game, especially when both sides may reap benefits simultaneously from escalation to points away from the cooperative outcome.

2. A more extended discussion of crisis instability can be found in O'Neill 1987, which reviews different efforts to formalize crisis instability and defines axiomatically a game-theoretic index of this concept; see also Intriligator and Brito 1977. For a fascinating alternative game-theoretic model of escalation based on a dollar auction in which the two highest bidders pay, see O'Neill 1986 and Shubik 1971.

3. In a noncrisis context, the "robustness" of deterrent threats has been analyzed in Brams 1985 (pp. 36-43).

4. The route by which the final position is attained from the preplay and initial positions may

social and political history that has brought us to this stand in our crisis of justification. The crisis of justification is not unrelated to the crisis of authority in our time. Perhaps the practical and historical dimensions of this crisis will be dealt with in volume XXIX as *Nomos* revisits the analysis of "authority."

Finally, the volume as a whole succeeds in nothing so much as lowering our expectations and hopes in virtually every arena, whether in the matter of liberalism, fundamental rights, ideal theories of justice, legal authority, objectivity, science, reason, or justification itself. What emerges politically is an exceedingly cautious, analytical, and minimalist defense of liberalism. Long gone are the proclamations of the Enlightenment, the Revolution, the New Deal, and the New Frontier. And the prospects that liberalism might regain its lusty confidence in itself or its foundations appear about as likely as its being saved by a return to justification *sola fide*. By no means is this a criticism of the book or the intelligence contained in it. But it is a sign of just how little we are confidently prepared to justify in our time.

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The Art of Political Manipulation. By William H. Riker (New Haven, CT: Yale University Press, 1986. xi, 152p. \$18.95, cloth; \$6.95, paper).

Among students of politics, William H. Riker is so closely identified with mathematical theories of collective choice that some in our discipline may be surprised at the appearance of a volume of political stories bearing his name. This compilation of twelve tales, spanning the ancient Roman Senate of Pliny the Younger and the contemporary U.S. Senate of Warren Magnuson, is rich with historical detail and personal anecdote. No equations intrude on the lively accounts of Lincoln's rhetorical victory at Freeport or Chauncey DePew's sly defeat of the Seventeenth Amendment, for the emphasis is entirely on the political art of exploiting agendas and rules. If one did not know better, the appearance of this small book might be taken as a sign that the well-known positivist had renounced his faith

in deduction to become a latter-day institutionalist.

But Riker's latest work is no reversion to traditional forms of political analysis; rather, it uses historical settings to illustrate the fundamentals of social-choice theory. The examples of political manipulation assembled here reflect the author's longstanding interest in grounding theory in observation, and many of them have figured in Riker's previous writings on Kenneth Arrow's Impossibility Theorem and Robin Farquharson's exposition of sophisticated voting strategies. While this diverse collection of episodes reveals Riker's extraordinary breadth of knowledge about political behavior, it also offers vivid pictures of how leaders in democratic regimes forge agreements when no underlying consensus exists.

Social-choice theorists like Riker have endeavored to discover lawlike regularities to explain political events, but they have deduced instead a world in perpetual disequilibrium. Under democratic norms of unrestricted preferences and nondictatorial rules, voting cycles are unavoidable, and strategic calculations determine the selection among rival alternatives. This systematic chaos means that political decisions that depend on voting schemes are inherently unpredictable and offer exceptional opportunities for leadership. As Riker observed in *Liberalism against Populism*,

The absence of political equilibria means that outcomes depend not simply on participants' values and constitutional structures, but also on matters such as whether some leader has the skill, energy, and resources to manipulate the agenda, or whether some back-bencher . . . has the imagination and determination to generate a cyclical majority. . . . These are matters of perception and personality and understanding and character. . . . [D]emocratic political outcomes . . . are an amalgamation that often operates quite personally and unfairly, giving special advantages to smarter or bolder or more powerful or more creative or simply luckier participants." (1982, p. 200)

The outgrowth of the indeterminacy Riker sees in democratic regimes is a type of political entrepreneurship he labels *heresthetic*. This is the art of creating successful coalitions by reframing alternatives, so that people are induced or compelled to join without necessarily being persuaded to the leader's point of view. Heresthetic is not scientific, but its

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by shifting ground from justification to justifiability. He then offers a cautious defense of liberal legalism in the face of accusations made by critical legal studies that laws, rights, and judges' powers are riddled with contradiction. This politically sensitive accusation awaits further response until Jeffrey H. Reiman takes it up in the section's last essay, where he too defends legalism. In an essay that deserved to be longer, given its suggestiveness, Amy Gutmann anticipates a defense of liberalism along contextualist, not legalist, lines; and she would have us look beyond judges to the "unity of moral labor" (p. 166) among legislators and judges. One suspects that this is an eighteenth-century use of *moral*, the present political realities of Washington being what they are.

The question of fundamental rights as a constraint on democratic majority rule is taken up by Jeffrie G. Murphy in a deck-clearing operation for later hoped-for constructive efforts. No defense of fundamental rights heretofore offered passes muster with him; and the prospects of there being a long and ambitious list of justified fundamental rights after future efforts appear quite grim. The objectivity of law—so denounced by critical legal studies and by legal realism before that—is defended by Christopher H. Schroeder, but only after he relaxes our expectations about what objectivity is. Martin P. Golding engages in a similar sort of effort, though in his case we are asked to follow the consequences of an analogy between legal and scientific justification, once science is looked at from the postpositivist perspective of Norwood Russell Hanson and others. This analogy, by the way, occasions what is a rarity in a *Nomos* volume: the essays do not quite connect. Baier criticizes related efforts as "moral scientism"; and Murphie observes, not altogether as facetiously as he makes it sound, "One could say that this shows that morality now has a clear bill of health because it is in no worse shape than science. One could also, of course, begin to sign the death certificate on the pretensions of science by arguing that it is in no better shape than morality" (p. 160). What *would* Paul K. Feyerabend be like as chief justice of the Supreme Court?

Political scientist James S. Fishkin gets the biggest rise out of the collected authors, for no fewer than three essays in the third section (and one in the second) respond to his efforts.

He charts seven ethical positions relevant to the justification of the liberal state. Rejecting subjectivism, absolutism, rigorism, and three others, he adopts a "minimal objectivism." Fearing rightly that liberalism is on the eve of self-destruction, he ends with the reasonable if plaintive call for a revision of our moral culture. We should expect (for we will surely receive) less from liberalism by way of what it can justify in absolute terms. Just how we are to secure this moral revision is discussed neither by Fishkin nor by those who take him up on other matters about justification in politics. Barbara Baum Levenbrook counters two of his skeptical arguments about the limits of jurisdiction and the "unforseeability" of future difficulties. A case for subjectivism and against allegations that it undermines liberalism is made by Gerald Gaus.

Four essays by political theorists bring the volume to a close, and collectively make for the most accessible reading. Things take a Kantian (re)turn in Richard Dagger's elegantly written defense of autonomy and a rights-based liberalism. In the face of many recent allegations to the contrary, he contends that no radical individualism need burden liberalism when it is rightly understood. In different essays, J. Roland Pennock and J. Patrick Dobel make out measured defenses of liberal democracy as a political and ethical ideal, thereby illustrating prudential justification. After recognizing the present disquiet, Thomas A. Spragens, Jr. ends his discussion—and the volume—with reminders that justification is an exercise of practical reason that may draw some solace from the Western tradition from which it issues.

As one surveys the whole of *Justification*, its steadfast commitment to analysis stands out, what with its several numbered distinctions, positions, levels, kinds, types, theses, and sorts. It is hard for a reviewer, and I expect it will be for a reader, to do justice to all the arguments because there are so very, very many of them. Attending this abundance, there is a complementary poverty of real practical examples, those in which justification is first called for and then made concrete in the defense of some particular action, law, or public policy. Analysis also tends generally to forget history. In this *Nomos*, the history of political thought is better served (thanks to Pennock, Dobel, and Spragens) than is the

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be relevant but will not be modeled here. Rather, our focus will be on the deterrent threats that can arrest further crisis escalation, however it got started.

5. The four weights so obtained are nonnegative and sum to 1, but they do not constitute barycentric coordinates. This is because they do not provide a unique representation for every point of the square; uniqueness is preserved only if three points (rather than the four corner points in our model) are used to define coordinates in the two-space of the unit square.

6. In related work, Dacey (1987) analyzes within a decision-theoretic framework the use of probabilistic bribes, threats, and tit-for-tat combinations in a number of variable-sum games. Probabilistic variations on tit-for-tat are developed in Brams, Davis, and Straffin 1979 and Brams and Davis 1987 and can also be found in Brams 1985 (chaps. 3, 4) and Brams and Kilgour 1986a, 1987b.

7. See also Shubik 1982 (pp. 265–270) for a recent discussion of perfect equilibria and related concepts.

8. Gauthier (1984) claims that such precommitments are not necessary to deter aggression but that threats that are not credible are empty, and empty threats invite attack. His calculus of deterrence, we believe, is sensible only when the retaliator's threats will assuredly be implemented because of precommitments.

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BUREAUCRATIC POLITICS IN RADICAL MILITARY REGIMES

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Most theories of bureaucratic politics depict state bureaucracies as a conservative force in the political system. Their resistance to radical politics and innovative programs is attributed to certain typical traits of bureaucratic structures and career patterns. I summarize the arguments for bureaucratic conservatism, and then describe how civilian bureaucracies serving military regimes in Japan (1937-45), Peru (1968-75), and Egypt (1952-70) invalidated those arguments by promoting radical policy programs through the three devices of supraministerial bodies, low-ranking ministries, and new specialized agencies. I conclude that middle theories of bureaucratic politics may prove more fruitful than grand theoretical attempts to encompass all bureaucracies in a single set of propositions, and that structural and occupational explanations of bureaucratic behavior need to be modified by a greater appreciation for the role of individual bureaucratic leaders.

While it is commonplace to note the great power of modern state bureaucracies, most scholars agree that bureaucrats use their sizable influence for conservative or moderately reformist ends in relation to the existing social order. Administrative radicalism is said to be blocked by certain inherent features of bureaucratic organizations that foster conservative behavior. Although a few students of public administration have challenged this position, their rebuttal has not suggested that a professional, merit-state bureaucracy might foment revolutionary changes in politics and society.

Unrelated to the issue of inherent bureaucratic tendencies, several case studies of political change have brought the term "revolution from above" into currency (Einaudi 1976; Trimberger 1978). Most often, such revolutions are ascribed to military-bureaucratic regimes that have undertaken radical policy programs at the instigation of administrative elites them-

selves. This contrasts with the more familiar portrayal of revolutions, in which radical policies follow an upheaval of mass violence launching a new elite into power. The role played by military officers in revolutions from above has received serious scholarly attention. For example, it has been analyzed in some cases through the concept of the military's "new professionalism," which describes how a preoccupation with internal security may lead the armed forces to espouse fundamental social change (Stepan 1978, 128-34). Although it is widely recognized that civilian bureaucrats make a critical contribution to governance in military regimes (Feit 1973, 8-13; Nordlinger 1977, 121-22), their role in fomenting revolution from above has received much less study, and its implications for the aforementioned thesis of inherent bureaucratic conservatism have never been fully elaborated.

This article relates the research on radical military regimes to the standard por-

trait of bureaucratic conservatism, describing systematically how civilian bureaucratic institutions have overcome the most often cited obstacles to innovation and become prominent forces for change. It begins with a review of the main arguments for bureaucratic conservatism. It then subjects these arguments to empirical testing in three cases of so-called revolution from above: Japan (1937-45), Peru (1968-75), and Egypt (1952-70). The conclusion is that bureaucratic structures and career patterns need not prevent radical institutional behavior and that some forms of administrative organization have provided strong incentives for innovation.

Several clarifications are in order. First, the focus of attention is not on circumstances in which political figures have been appointed to administrative posts and acted as a *deus ex machina* to herd their subordinates in a new direction. These instances are numerous enough (early-Meiji Japan would be one example), but they disprove nothing about the allegedly inherent conservatism of professional, career civil servants. Defenders of the conservative image of bureaucracy need only respond that political appointees, too, will join the conservative ranks as soon as they begin to feel at home in a bureaucratic establishment (Djilas 1957; Michels 1962). How political revolutionaries give birth to bureaucracy is part of this study, but it is equally concerned with how professional bureaucracies give birth to revolution. Second, the argument is not simply that a few disgruntled ideologues may penetrate the examination system and become administrative officials; this is a hollow truism, given the size of today's state bureaucracies. Rather, the subject is institutional behavior, that is, the process by which professional administrators with revolutionary objectives have transformed their bureaus into radical political actors. Third, while the hypothesis that bureaucratic politics

may vary with the regime context emerges as a logical supposition from this research, there is also ample reason to believe that the findings may be applicable to other regime configurations; the latter possibility, however, has not been systematically explored. Note that although military regimes supply most of the evidence, this factor does not emasculate the brief against theories of bureaucratic conservatism because these theories virtually never specify the regime context as a conditional variable in their calculations. Finally, there is no contention that state bureaucracies in any context are as likely to foment change as they are to guard the status quo. It is probable that all social institutions, bureaucratic or otherwise, tend to be conservative most of the time. Such may be the general bent of human nature. However, most theories of bureaucratic politics assert that bureaucracy is a special case of institutional conservatism. It is argued that certain distinctive traits of bureaucratic organizations lock them into a conservative mold in a way that is not true of other social institutions. It is this argument that the article challenges by demonstrating that its claims have been disconfirmed in a significant number of cases.

The Case for Bureaucratic Conservatism

The concept of revolutionary bureaucracy defies many common axioms of empirical theory purporting to prove that bureaucracy induces conservative policy making. The factors thought to discourage innovation begin with recruitment criteria. Recruitment into the higher civil service is said to exclude political non-conformists (Leemans 1976b, 12). Entrance examinations tend to emphasize technical knowledge or a high general level of education rather than political activism, and where political criteria are

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considered, they are used to weed out those with undesirable affiliations or to confirm that the candidate's ideological commitments resemble those of his or her examiners. Thus, few applicants favoring a sharp departure from existing policy will be admitted. One variation on this theme is that creative people are not attracted to apply for bureaucratic jobs due to recruitment criteria and other conservative aspects of bureaucratic organization (Crozier 1964, 53; LaPiere 1965, 410-12). Another is that recruitment criteria limit successful applicants to the middle and upper classes, which presumably have a stake in defending the status quo (Leemans 1976b, 12; but see Halpern 1963, chap. 4).

A second point is that recruitment criteria and the functional division of labor between bureaucratic offices foster a rational-technical approach to policy problems and a complementary desire to minimize partisan political input into policy making. In the famous phrase of Karl Mannheim, bureaucrats seek "to turn all problems of politics into problems of administration" (1936, 118). The bureaucrat believes that there is one rational "best" solution for each problem and that his or her technical and managerial skills as well as systematic administrative procedures are necessary to identify that solution. Partisan political interference forcing a break with routine in the name of ideology is resisted as an irrational force subversive of effective decision making.

A third factor allegedly promoting conservatism is that the specialization of bureaucratic labors imposes an incremental approach to problem solving and obstructs holistic assessments of system-level crises and reforms (Sharkansky 1971, 269-75). Each office has a limited field of jurisdiction and expertise, giving it but a partial view of the overall policy picture. Bureaucrats are therefore unlikely to advocate major policy shifts that

would transcend the competence and jurisdiction of any particular agency. In the words of Aberbach, Putnam, and Rockman, the bureaucrat's perspective on policy is often "a mile deep and an inch wide" (1981, 114).

A fourth element cultivating a conservative outlook is said to be the importance of seniority in winning promotion (Strauss 1960, 65-67). No bureaucrat will reach a post of substantial authority until and unless that bureaucrat is thoroughly socialized into established institutional norms. Iconoclastic behavior born of youthful exuberance will have minimal impact. As Robert Merton wrote, "The bureaucrat's official life is planned for him in terms of a graded career, through the organizational devices of promotion by seniority, pensions, incremental salaries, etc., all of which are designed to provide incentives for disciplined action and conformity to the official regulations. The official is tacitly expected to and largely does adapt his thoughts, feelings, and actions to the prospect of this career. But *these very devices* which increase the probability of conformance also lead to an over-concern with strict adherence to regulations which induces timidity, conservatism, and technicism" (1952, 367). Due to the seniority principle, the would-be innovator confronts Anthony Downs's law of increasing conservatism: "In every bureau, there is an inherent pressure upon the vast majority of officials to become conservers in the long run" (1967, 99).

A fifth contention is that bureaucrats are conservative because their official functions are hemmed in by legal strictures and supervision (Leemans 1976a, 89-90). In their review of the literature comparing public- and private-sector organizations, Rainey, Backoff, and Levine found a widespread consensus that "government organizations tend to have their purposes, methods, and spheres of operation defined and constrained by law and legally authorized institutions to a

much greater degree [than private organizations]. One effect of these constraints is that public managers have less choice as to entry and withdrawal from various undertakings" (1976, 238). The mechanisms of enforcement are not only internal but also involve accountability to external agencies, such as legislatures, interest groups, the courts, and the press (LaPiere 1965, 405; Roessner 1977, 348).

A sixth trait said to impede innovation is the prevalence of group over individual decision making in bureaucracies. Irving Janis (1982, 174-75) has attributed major bureaucratic blunders to a "groupthink" syndrome that includes the propensity to exclude minority opinions and adverse information that might threaten the preconceptions underlying group consensus. Downs (1967, 180) also notes that a bureaucratic search for new methods to bridge a performance gap is likely to be biased from the outset by the need to achieve consensus behind any new course of action. The room for innovation varies inversely with the number of people who must give their approval. The judgment of William Whyte was even harsher: "People very rarely *think* in groups; they talk together, they exchange information, they adjudicate, they make compromises. But they do not think, they do not create" (1956, 57).

A final impediment to radical policy shifts is the special relationship that frequently develops between bureaucratic agencies and powerful civil interest groups subject to their authority (LaPalombara 1974, 333; Leemans 1976a, 69-71; Lowi 1979, 58-61; Peters 1978, 148-51). These *clientela* connections often find administrators serving as lobbyists for their client groups within the state. Unabrasive and predictable administration requires cooperation from key civil associations, and the bureaucracy's maintenance of close ties with them becomes a vested interest that transforms officials into protectors of the existing social order.

To summarize, the causes of bureaucratic conservatism are said to be (1) recruitment criteria, (2) a rational-technical approach to policy problems, (3) a specialization of functions that leads to incremental policy making, (4) promotion by seniority, (5) legal restraints and accountability, (6) the prevalence of group decision making, and (7) special relationships with powerful interest groups.¹ These arguments are prominent not only in the "new-public-administration" literature of the late 1960s and 1970s but in most standard works on bureaucratic politics. They point to a conclusion that has been restated so many times it has earned the status of a cliché. Michel Crozier, who defined a bureaucratic organization as "*an organization that cannot correct its behavior by learning from its errors*" (1964, 187), found the case for rigidity so persuasive that "one may wonder more about the (very infrequent) innovating decisions than about the reiteration of routinized behavior" (p. 52). Aberbach, Putnam, and Rockman declare, "Prudence, practicality, moderation and avoidance of risk are the preferred traits of a civil servant; only a politician could have termed extremism a virtue and moderation a vice" (1981, 12). And Fritz Morstein-Marx wrote, "The merit bureaucracy operates like a brake; it favors a coherent evolution and discourages excessive swings of the pendulum" (1957, 162). It follows that when bold new policy initiatives are in order, bureaucrats will fade into the background, and politicians will come to the fore. As stated by Mattei Dogan, "During crises, exceptional situations, or dramatic circumstances, the power of top civil servants almost disappears, and politicians—parliamentarians, ministers, or charismatic leaders—assume full powers" (1975, 19-20).

The mainstream position on bureaucratic conservatism has not gone unchallenged, but the claims of the dissenters are

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quite modest in the context of this research. A first counterargument is to attack the excessive occupational determinism of the mainstream position. Modern state bureaucracies comprise a vast complex of institutions. They are not monolithic, and their members are not all cut from the same cloth. "Bureaucrats," Charles Goodsell writes, "don't 'bur'" (1985, 79, 83) but rather engage in very diverse occupations (cf. Waldo 1971, 276). Anthony Downs's (1967, 88-89) description of different bureaucratic types (climbers, conservers, advocates, zealots, statesmen) further underscores the diversity among bureaucratic personnel. A second rejoinder is that the mainstream position is not unambiguously supported by case studies of actual bureaucratic behavior. Both empirical studies of the policy-making process and survey research designed to unearth the attitudes of bureaucrats have sometimes contradicted the conservative image (Blau 1955, chap. 12; Roessner 1977, 354-57; Goodsell 1985, 50-54; Kohn 1971, 465-66). Many canons of the majority view do seem to spring from the abstract logic of organization theory rather than careful empirical testing, and even the logic of bureaucratic career patterns may not be as obvious as it first appears. For example, while some may see the seniority system as logically breeding conformity, others may point to the bureaucrat's job security as (equally logically) facilitating innovation by decreasing the cost of failure (Blau 1955, 188, 199, 208; Kohn 1971, 470; Thompson 1969, 40). Whether either of these propositions is generally valid awaits the systematic gathering of evidence. Finally, many critics have argued that state bureaucracies are no more nor less conservative than businesses in the private sector. Goodsell (1985, 49-50) notes that state agencies face many demands for high performance despite the absence of the market and that private businesses also manifest many traits

typical of official bureaucracies (cf. Kohn 1971, 466).

On careful inspection, the critics of the conservative image of bureaucracy do not so much overturn the mainstream position as provide a healthy corrective for some of its more extreme expressions. To expose exaggerated statements of occupational determinism is not yet to demonstrate that bureaucracies may spawn radical changes, and the critics' empirical evidence for bureaucratic innovation rarely transcends technological improvements. For example, Dwight Waldo's (1971, 274) account of bureaucratic innovations refers to new methods of measurement and data storage, and J. David Roessner (1977, 352) gauges bureaucratic innovation by the speed with which bureaucratic agencies adopt new operational procedures and products invented elsewhere. Even Anthony Downs (1967, 110, 204), whose account of how zealots may promote new policies is pertinent to the pages that follow, offers as evidence of bureaucratic innovation only new weapons technology such as guided missiles and Polaris submarines (see Lewis 1980, chaps. 2, 3). Similarities in the performance of the public and private sectors do not really contradict the conservative portrait of bureaucracy but only demonstrate that there may be little to choose between public and private bureaucracies on this score. None of these criticisms explains how or why bureaucratic institutions might move in radical *political* directions or attempts to explain the phenomenon of administrative radicalism in terms of bureaucratic theory.

The remainder of this article examines how innovative bureaucrats in Japan, Peru, and Egypt managed to transcend the various factors allegedly producing conservative administration. Each case study will document briefly the character of the regime's policies and then describe several specific instances in which civilian bureaucrats took the initiative in intro-

ducing radical programs. The treatment highlights the use of several organizational devices, common to the three cases, that enabled creative administrators to play a decisive role in policy making.

The Ways and Means of Administrative Revolution

Three principal organizational tools have been used to break down the barriers to bureaucratic innovation in radical military-bureaucratic regimes. The most important has been the supraministerial agency, which may take the shape of an ad hoc committee or a large, formal institution. Supraministerial agencies have been uniquely effective in enabling administrators to sidestep nearly all the major hindrances to creative policy making. They are also the most easily organized of the various institutional means to innovation because their establishment may require little tampering with existing structures or change in the law. In fact, they have become a ubiquitous feature of state bureaucracies under all forms of political regime, represented in the industrial democracies by such examples as the Dutch Central Plan Bureau, the Belgian Bureau of Economic Planning, and the United States Office of Management and Budget. Supraministerial agencies played a vital political role in the mobilizational military systems of Japan, Peru, and Egypt by assembling radical bureaucrats from all arms of the administration and turning these otherwise isolated elements into an organized, coherent political bloc within the state.

A second instrument of change has been the low-ranking ministry that emerges from obscurity to promote bold, new policies. In any hierarchical system, it is mainly those near the top who have a vested interest in sustaining the hierarchy; those at the bottom may have more to gain by overturning it, and this is demon-

strated in the case studies by the experience of several minor ministries.

The third vehicle for innovation has been the new agency established with a formal mandate to develop novel policies in a specific area. Administrative reorganization has become a continual process in most states, and for this reason the policy-making tendencies of bureaucracy cannot be adequately discussed without taking into account the input of new agencies. Bureaus of this type tend to be less politically significant than supraministerial bodies, whose responsibilities are more diffuse, but they nonetheless sponsored many fresh policy departures in all three countries under study.

Japan

The organizational landscape of Japanese society was largely redrawn during the 1937-45 period. As a result of state action, all labor unions were dissolved in favor of the Industrial Patriotic Society (Gordon 1985, Notar 1985). Political parties gave way before the bureaucratically managed Imperial Rule Assistance Association, whose subdivisions mobilized almost the entire population by age, occupation, and place of residence (Yokusan Undō Shi Kankokai 1954). Women's groups were absorbed by the Great Japan Women's Association (Fujii 1985), and drastic consolidations were effected in every business sector; the number of banks, for example, was cut from 426 in 1938 to 69 in 1945 (Hadley 1970, 118), and textile mills from 271 to 44 (Johnson 1982, 165). Moreover, these changes were not mere expedients in response to war but were considered part of a permanent transition from the dying era of freedom to the nascent epoch of the national-defense state. Considering the size of the country and its level of development, Japan possessed the most powerful military-bureaucratic regime in history, and the radicalization of the

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bureaucracy was the key to its unprecedented expansion of control over society.

One salient example of a supraministerial agency that initiated structural changes was the Cabinet Information Bureau (CIB), which took charge of state policy toward the mass media. Under its tutelage, Japan's media industries were thoroughly reorganized. The 10 producers of dramatic movies were forcibly reduced to 3; all news-film companies were merged into a single firm under state direction; and some 300 film distributors were fused into a new state-regulated monopoly (Tanaka 1957, 241-84; Uchikawa 1973, 356-65). The CIB took charge of organizing every film program shown in every theatre in Japan. The press underwent similar changes. In 1942, only 64 general daily newspapers remained where there had been nearly 700 six years before, and some 25 thousand magazines were also systematically driven out of business (Uchikawa 1973, 492-97; Ōhara Shakai Mondai Kenkyūjo 1965, 186). By December 1940, the CIB had also asserted complete and direct control over the contents of all radio broadcasts, and thereafter it strove to see that every program, from the news to musical entertainment, would "conform to the purposes of the state" (Uchikawa 1973, 478). How did this institution apparently ensconced in the establishment come to break so cleanly from the conservative mold?

The very interministerial character of the CIB cut through one impediment to innovation, namely, the incrementalism associated with the work of highly specialized offices. As of December 1940, the CIB counted 118 bureaucrats of high rank among 510 full-time employees. Fifty-two of the ranking bureaucrats were permanent CIB officials (most transferred from other agencies), while 24 were on temporary assignment from the Foreign Ministry, 14 from the Home Ministry, 10 each from the army and navy ministries, and eight from the Communications

Ministry (*Senzen no Jōhō Kikō Yōran* 1964, 88,187). One reason supraministerial bodies like the CIB are created is to free policy makers from ministerial blinders and to forge programs that cut across normal jurisdictional boundaries.

As for a rational-technical orientation, CIB officials certainly believed in a rational approach to organization and planning (they were not technocrats), and in fact their educational backgrounds differed little from those of other high civil servants. The top officials (e.g., Kiwao Okumura, Yoshio Miyamoto, Ryūzō Kawamo) had graduated from the law faculty of Tokyo Imperial University and passed the difficult higher-civil-service exams, following the usual elite route to an administrative career. They had entered the bureaucracy in the mid-1920s as employees of various established ministries. However, their commitment to rational administration was complemented (indeed, bolstered) by a radical political ideology that borrowed elements from both Marxist and fascist thought. They believed that an important stage of history revolving around democracy and party politics, capitalism and free markets, and individualism and the profit motive was coming to an end. As they saw it, the irrationalities of a competitive society exemplified by class conflict and the Great Depression could only be rectified by a quantum leap in rational, bureaucratic state control (Miyamoto 1942, 12-13, 18-24; Okumura 1938, 34-43, 113-14). Their rational approach to problem solving therefore posed no checks upon radical policy departures.

Ideological convictions along these lines were an important criterion for entry into the bureau almost from its inception. The CIB had started as an informal bureaucratic discussion group in the early 1930s and evolved into a formal committee in 1935 to supervise the state's news agency, whose creation at that time had been one of the group's recommendations

—a significant innovation in itself. The committee was initially of little consequence, but it soon became a haven for bureaucrats of radical inclinations, who were called “renovationist bureaucrats” (*kakushin kanryō*). By the late 1930s, a record of commitment to radical state-control policies was common to the background of many officials joining the bureau. For example, Kiwao Okumura had framed the seminal legislation to control the electric-power industry in 1935 as a Communications Ministry official, enunciating the widely publicized principle of “private ownership/state management,” and Ryūzō Kawamo (1936) had praised both the Nazi and Soviet broadcasting systems before enlisting in the CIB. Regardless of conservative recruitment criteria for initial entry into a bureaucratic career, the admissions standards used by the bureau to screen prospective candidates from other agencies involved a novel set of political convictions.

The seniority principle was less operative in the CIB’s personnel decisions than it was in the established ministries. In this fairly new organization, there was no upper crust of old hands to block the advancement of younger bureaucrats, and those officials seconded temporarily from other ministries remained on the seniority ladders of their home agencies. Most importantly, the CIB was formally attached to the cabinet, allowing the prime minister to fill the top posts at his discretion; the chief executive did not exercise comparable rights in the more autonomous ministries. Both the civilian Prime Minister Fumimaro Konoe and his successor General Hideki Tōjō used this prerogative to staff the bureau with mid-career “renovationists.” When Kiwao Okumura took effective charge of the CIB as its vice governor in October 1941, he was but 41 years of age.

The projects advocated by the CIB were delayed or modified on several occasions by the presence of legal barriers. Plans to

consolidate publishers, for example, could not be completed until the full cabinet sanctioned this policy in late 1941. However, the bureau’s record of making laws was more impressive than its record of obeying them. As the international crisis deepened in the late 1930s, an overloaded policy agenda and the need for speedy decisions saw more and more legislative matters removed from the purview of the parliament and relegated to the realm of administrative decrees. The State Total Mobilization Law of 1938 was a comprehensive enabling act that empowered the executive to regulate the organization, finances, and operations of almost every civil association in Japan. In practice, all but the most critical decisions pursuant to this law were made in bureaucratic offices. The mobilization law was originally drafted and proposed by the Cabinet Planning Board, another supra-ministerial body whose genealogy and personnel resembled those of the CIB. The planning board was the superagency charged with economic policy. The mobilization law and the Film Law of 1939, another statute replete with enabling clauses, allowed the CIB to execute much of its media-policy revolution through unilateral ordinances that were perfectly legal. Legal restraints were even less of a factor in Peru and Egypt, whose military regimes were launched by means of coups d’état involving a sharper break with the preceding legal systems and the abandonment of meaningful parliamentary institutions altogether.

The CIB also freed bureaucrats of impediments to innovation born of special relationships with interest groups. The CIB did not have long-standing ties with powerful media companies, and this is one reason that the bureau frequently pursued more radical policy goals than the Home Ministry, which exercised traditional jurisdiction over the film industry and the press. For instance, the Home Ministry opposed (on some counts, suc-

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cessfully) the CIB's early plans for consolidating the film industry in order to protect Shōchiku and Tōhō, the two largest producers of dramatic movies.

The Ministry of Commerce and Industry offers an example of the second device used to further innovation, as a relatively weak ministry that quickly changed direction under the military regime. Founded in 1926, the ministry lacked prestige in a country where institutional age is highly prized, and its authority was at first quite limited. Wartime mobilization offered special opportunities to expand its functions, however, and by 1940 "renovationist bureaucrats" like Nobusuke Kishi dominated the ministry and identified themselves with the more radical schemes to extend state control over society. Their policies caused hundreds of thousands of bankruptcies among small-to-medium-size companies and forced the largest firms into "control associations" (*tōseikai*) through which officials attempted to regulate production priorities, access to raw materials, and many other key managerial decisions (Johnson 1982, 143, 162–65). Although the Ministry of Commerce and Industry had extensive connections with the industrial combines, the prospect of new authority served as an important counterweight, inducing officials to pursue many policies opposed by their clients in civil society. While the supraministerial Cabinet Planning Board designed the broad contours of economic mobilization, the ministry was charged with their implementation. The CIB stood in a similar relationship to the Home Ministry and Communications Ministry, which executed many of that bureau's policy plans. In both cases, considerable movement of personnel between the supraministerial planning agencies and the ministries of implementation helped to mitigate (though not eliminate) friction between them.

Regarding the third tool for change, the Welfare Ministry, founded in 1939, dem-

onstrates how a new agency can serve to produce innovations in a particular policy field. The coming era of the national-defense state was deemed to require a coordinated effort by all economic actors. With few exceptions, previous legislation had not set standards for working conditions, and this resulted in costly labor disputes and poor morale and health among workers. The Welfare Ministry quickly filled the policy vacuum with regulations governing such matters as the payment of allowances for overtime, night work, and family size, and the provision of company facilities for dining, health care, and other services (Gordon 1985, 261–97). These measures were complemented by strict controls over wages and labor mobility, the whole package representing a sharp departure from earlier policy.

The profound impact of Japan's military regime stretches beyond the ravages of the Pacific war into many aspects of postwar Japanese society. The CIB's consolidations fixed a national structure of daily newspapers that largely persists to this day. Wartime economic mobilization set critical precedents for Japan's famous industrial policy of the postwar era, and labor mobilization laid important groundwork for the system of enterprise unions (Nakamura 1981, 18; Ōhara Shakai Mondai Kenkyūjo 1965, 60–61). Despite the regime's premature end in defeat, the legacy of the "renovationist bureaucrats" lingers in these and many other aspects of the social system. Scholars rarely analyze measures of administrative reorganization as major events, but they may be every bit as consequential in a bureaucratically dominated regime as a major shift in the party system of a democracy.

Peru

The Peruvian government of General Juan Velasco Alvarado has been perhaps the most studied case of revolution from above. Velasco oversaw a rapid expan-

sion of the state sector involving the nationalization of numerous industries, including the telephone and electric utilities, fish-meal production, mining and petroleum extraction and refining, the railroads, airlines, television stations, newspapers, various import and export activities, banking, and insurance (Lowenthal 1975, 7). Employment in the public sector mushroomed from 270 thousand in 1967 to 455 thousand in 1975, and public expenditures rose from 24% to 46% of the gross national product (Cleaves and Scurrah 1980, 72). Even more impressive was the creation or restructuring of popular organizations in many social sectors, especially those aimed at industrial and agricultural workers and urban squatters. Although military officers were prominent in top administrative posts, the armed forces came into power divided as to the question of revolutionary goals (Pilar Tello 1983, 72-83). In the beginning, conservative officers held many key ministries so that appointments might accord with the military hierarchy and maintain balance between the services. In this circumstance, supraministerial bodies directly under the president played a key role in developing radical policies.

Military officers, career bureaucrats, and civilian outsiders hired into the government all participated in formulating the revolutionary program. While the relative input of each group varied by issue area, one well-documented case that highlights the role of longer-term state administrators was the regime's agrarian reform. The redistribution of land was among the most radical policies during Velasco's tenure, ranking with the Mexican and Cuban land reforms as one of the most comprehensive in Latin American history. Approximately 7.2 million hectares (about 31% of all agricultural land) were turned over to collective enterprises involving some 414 thousand families, and both the large foreign agro-export

companies and the Peruvian landed oligarchy were eradicated in the process (Cleaves and Scurrah 1980, 220-21). The results would have been far less dramatic without the intervention of civilian bureaucrats.

The excellent research of Peter Cleaves and Martin Scurrah (1980, 96-110) describes the evolution of agrarian policy in some detail. In November 1968 the minister of agriculture, General José Benavides, organized a small commission within the ministry comprising bureaucrats and a few individuals from the private sector to draft a new agrarian reform law. The minister was not among the more radical officers in the government, and a majority of the commission favored a reform of moderate proportions. Before the ministry's proposal could be presented to the cabinet, however, two radical administrators on the commission took steps to sabotage it. One was Benjamín Samanez, formerly chief of a regional agrarian office under the civilian government of Fernando Belaunde Terry. The other was Guillermo Figallo, who had worked for Belaunde's National Agrarian Council. These men communicated their discontent privately to members of the president's advisory council, *Comité de Asesoramiento de la Presidencia* (COAP), an agency staffed mainly with radical colonels handpicked by Velasco. COAP permitted the president to accomplish within the army what supraministerial bodies were to achieve within the bureaucracy: the establishment of a radical nucleus. COAP members sympathetic to the dissenters then consulted Eduardo Morán, a bureaucrat employed by the Institute of National Planning, who prepared a statement censuring the proposed reform. When the cabinet met to consider the ministry's recommendations, one COAP officer read Morán's position paper aloud, while another suggested that the matter was too vital to be decided within a single ministry.

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The upshot was the creation of a broader supraministerial commission directly under COAP to reconsider the issue. Including Samanez, Figallo, and Morán among its members, this body was stacked in favor of the radicals. Predictably, the second commission's proposal was a more extreme measure. The civilian bureaucrats not only drafted the new law but also composed supplementary notes for the sponsoring ministers, advising them how to respond to questions that might be raised in the cabinet. The puppeteers themselves were ineligible to attend meetings of the all-military Council of Ministers. The new reform generated its own superagency of supervision, the Directorate for the Promotion and Diffusion of the Agrarian Reform. Its first director was Benjamín Samanez. Another member of the COAP commission, Guillermo Figallo, proposed that a new agrarian judiciary be founded to uproot the influence of large landowners over existing rural courts and protect the interests of the peasantry. He subsequently became the first president of a new Agrarian Court System (Cleaves and Scurrah 1980, 154-55). It is noteworthy that COAP's radical commission and the new judicial system, like Japan's CIB, were organized at the behest of civilian bureaucrats themselves and did not simply materialize as impositions from above.

The agrarian reform was just one example of how a combination of supraministerial bodies and new specialized bureaucracies enabled administrators to take the initiative in radicalizing the regime. The government's unconventional policies toward Peru's urban squatters (estimated at more than 760 thousand in Lima alone) illustrated the same phenomenon (Stepan 1978, 160). A small commission formed directly under the president authored a report leading to the formation of a new-towns development office, Oficina Nacional de Desarrollo

de los Pueblos Jóvenes (ONDEPJOV) (Stepan 1978, 162). This was a supraministerial agency to orchestrate the activities of all government organs operating in the urban slums, and like Japan's cabinet planning bodies it was directly responsible to the prime minister (Dietz 1980, 144). At the same time, a new Ministry of Housing was launched to serve as ONDEPJOV's principal tool of implementation. The result was an unprecedented effort to organize the squatters and extend them land titles and basic public services. In this case, the original presidential commission was dominated by nongovernmental personnel (Collier 1976, 98-99), which renders this instance less damaging to the theory of bureaucratic conservatism than COAP's commission for agrarian reform. The ensuing steps of administrative reorganization, however, were very similar.

In 1972, a grand attempt was made to integrate the various planning agencies operating in high priority sectors of popular mobilization into a single superagency of superagencies, Sistema Nacional de Apoyo a la Movilización Social (SINAMOS). Like the agrarian reform and many other radical programs, the blueprint for SINAMOS was developed by a COAP task force with significant bureaucratic participation (Lowenthal 1975, 38). At the highest level, SINAMOS was led by some of the most radical military and civilian administrators serving the regime (e.g., General Leonidas Rodríguez Figueroa and Carlos Delgado Olivera), and it answered directly to President Velasco (Pilar Tello 1983, 92). The agencies promoting the agrarian reform and the betterment of the squatters were among eight key institutions incorporated into SINAMOS, and its establishment led to even more radical policies on several fronts. However, the effort to create a radical supraministerial body on this scale was not an unqualified success. Most of the agencies incor-

porated into SINAMOS were longstanding subdivisions of established ministries, not creatures of the military regime, and their personnel often brought with them the conservative habits of their former institutions (Delgado Olivera 1973, 256-61). According to Carlos Delgado Olivera, "The problem here was the following: how to structure, on the basis of personnel from typically conservative organisms, an institution of revolutionary objectives" (1973, 260). The mere relocation of conservative organs in SINAMOS did not suffice to resolve this problem, and strenuous efforts were made to restructure the offices in question and retrain their employees. Nonetheless, SINAMOS often suffered from internal dissension. Unlike the other supraministerial bodies discussed above, SINAMOS was also very large, employing some five thousand officials. Its size alone diluted the degree of political selectivity in appointing its personnel. Size was also a handicap in that SINAMOS was not just a planning body but possessed the resources to involve itself in policy implementation, and it thereby threatened the regular bureaucracy more than a simple supervisory agency like ONDEPJOV, which left execution entirely to the ministries. The smaller the size of a radical supraministerial agency, the greater may be its internal cohesion, and the less opposition it will provoke by encroaching upon the budgets and bailiwicks of the established ministries.

The phenomenon of a weak government agency adopting a radical stance and consequently expanding its influence was represented in Peru by the Institute for National Planning (INP). The institute had been installed by an earlier military government in 1962, but it had been neglected during the presidency of Fernando Belaunde (1963-68). The new military regime offered the agency a fresh chance to grow, and it rapidly increased its size, budget, and authority by promot-

ing innovative policies. Adopting novel interpretations, rooted in dependency theory, of Peru's underdevelopment, the planning institute acquired new authority to prioritize development projects and to control the sectoral budgets and investments of many government institutions (Dietz 1980, 11; Cleaves and Scurrah 1980, 71-73). Although the officials of the INP were known to have a strong technocratic background, this did not immunize them against the appeal of radical programs. Indeed, the regime's openness to change offered them the best opportunity they had ever had to plan and organize their country's development. Abraham Lowenthal wrote of the influence of technocrats in the regime: "the best-trained and most self-confident *técnicos* have tended to recommend that the reforms be carried further. The Planning Institute's impact, especially, has been to define the regime's approach in more comprehensive terms, to push for eliminating policy inconsistencies by generalizing the more radical of conflicting approaches. The regime has found itself both pushed and pulled to *profundizar la revolución*" (1975, 40).

It would be an error to portray the motives of the INP's reformers as somehow purely technocratic in the sense of being unrelated to politics. Certainly not all bureaucratic innovations are politically motivated, but policy decisions that cause a major shift of resources and power in the social system are at least partly political by their very nature. Such decisions impose a solution on a grand scale to the fundamental political problem of who gets *what*, *when*, and *how*, and they cannot be made without some determination regarding the basic values of justice and equality. The technocratic and the political cannot be neatly separated at this level of policy making. Nor should one imagine that innovative bureaucrats necessarily pretend to themselves that their actions are purely technocratic in

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character. The same universities that provide future bureaucrats with their training in engineering or law are also frequently hotbeds of political radicalism. Some of Peru's *técnicos* had been student leaders as young men (Lowenthal 1975, 34), and a common denominator among Japan's renovationist bureaucrats was their exposure to Marxist ideas on university campuses in the 1920s (Hashikawa 1965, 264-65). Happily for the officials of the INP, their radical proclivities dovetailed nicely with their career interests under the military regime, earning them more consequential posts than those granted to their more conservative peers. Some were even shifted to other agencies to bring them into step with the regime's revolutionary line; for example, Eduardo Morán was moved to the Ministry of Agriculture after his efforts to promote the agrarian reform, and Carlos Delgado was transferred to the same ministry and then into SIMANOS.

Defenders of the conservative portrait of bureaucracy might dig in their heels at this juncture and counter that there is no proof a *majority* of bureaucrats in these cases took on radical colors. However, because bureaucracies are undemocratic and do not operate by majority vote, this is quite beside the point. To say that most bureaucrats are conservative most of the time is not really saying very much, since this is true of people in virtually all occupations. Bureaucracy, "that gigantic power started up by midgets" as Balzac called it, should not be of interest to political science primarily for the majority opinions of the midgets. The gigantic power in question is the power of institutions. If theories of bureaucratic conservatism have something serious to say, it is not simply that most bureaucrats are normally conservative, but that bureaucratic institutions must behave conservatively due to their inherent characteristics. What is demonstrated in the records of Japan, Peru, and Egypt is that radical

minorities of administrators can redirect the institutional power of the bureaucracy, and they have done so by means of organizational devices so commonplace that hardly a government in the world has failed to make use of them.

Egypt

The governments of Gamal 'Abd al-Nasser introduced a broad range of radical policies in Egypt, especially in the early-to-mid-1960s following the declaration of Arab socialism. Sweeping measures of nationalization imposed state ownership and management of all banking, insurance, foreign trade, shipping, heavy industries, airlines, public utilities, and newspapers, as well as control over department stores, hotels, flour mills, tanneries, construction companies, and other significant manufacturing enterprises (Waterbury 1983, 73-76). Combined with limits on privately held stock in most industrial concerns (Dekmejian 1971, 125), these steps effectively eliminated the class of private capitalists hitherto dominant in the Egyptian economy. Several land reforms accomplished the same task vis-à-vis the country's dominant rural class of large landowners. By 1965, the government had made available through expropriations and land reclamation over 1.2 million acres (approximately 20% of all cultivable land) for distribution to landless peasants, benefitting over three hundred thousand families (Stephens 1971, 365). Iliya Harik has characterized this reform as "the most striking turnover in landholding in the history of private property in Egypt" (1974, 36). Furthermore, the conditions of the peasantry as a whole were improved by policies regulating the status of tenants, agricultural rents, and irrigation, and by the organization of cooperatives providing a wide array of new services (Harik 1974, 37-38). As in Japan and Peru, Egypt's political parties gave way before the regime's own

administered mass organizations (successively, the Liberation Rally, the National Union, and the Arab Socialist Union), and important interest groups such as labor unions and student associations were reorganized into bodies enjoying little spontaneity or autonomy thanks to firm bureaucratic control (Ayubi 1980, 451-56; Binder 1966; Harik 1973).

Egypt's socialist transformation was accompanied by a remarkable increase in the size and complexity of the state. Whereas the Peruvian military had raised the number of ministries from 11 to 17, their number doubled in Egypt (from 15 to 30) during Nasser's rule. Even these figures underestimate the proliferation of new organizational units in the public sector, which grew from several dozen in 1952 to about 600 (excluding village councils) in 1969 (Ayubi 1980, 239). The numbers alone conceal from view a constant process of redistributing functions and reorganizing existing agencies. The state administration expanded from approximately 350 thousand officials in 1952 to 1.2 million in 1970, not counting the personnel of some 370 public companies, the armed forces, and other public-sector organizations outside of the civil service proper (Ayubi 1980, 243). The Egyptian bureaucracy suffers the immense disadvantage of a legal requirement that it must hire all university graduates seeking employment, a guarantee that the administration will absorb countless unfit candidates and inflate perpetually, regardless of its objective needs. The tales of incompetence and corruption that inevitably flow from this circumstance are legion (Ayubi 1980, 280-87; Baker 1978, 70-72; Waterbury 1983, 347-49). Nasser sought to transform the Egyptian bureaucracy in two fundamental ways: the first was to downgrade traditional bureaucrats schooled in the liberal arts and the law and substitute for them a technocratic administration led by engineers; the second was to turn the new technocracy

to bold tasks of economic and political development. The means by which he aimed to achieve these ends closely resembled those employed by his counterparts in Japan and Peru.

The history of planning agencies in Nasser's Egypt exemplifies the use of supraministerial bodies to foster new programs. The first of these was the Permanent Council for the Development of National Production (PCDNP) organized in October 1952, which assembled technocrats from several ministries and the private sector (Ayubi 1980, 227). Most of the regime's early policy initiatives germinated within the council, including the first agrarian reform, the High Dam and Kima fertilizer plant at Aswan, and the Helwan Iron and Steel Complex (Waterbury 1983, 60-61). In the realm of development policy, the PCDNP occupied a position in relation to the Revolutionary Command Council similar to that of COAP to Peru's Council of Ministers, representing forces for change at a time when the supreme executive body was divided among officers of various political tendencies. Unlike COAP, however, the PCDNP was dominated by civilians. While symbolizing Nasser's intent to elevate technocratic personnel in the bureaucracy, the PCDNP also redirected thinking about the state's overall role in society by pioneering economic planning in Egypt and by expanding the public sector through the acquisition of state shares in its developmental projects; it thus involved itself in decisions that were not simply technical but political in kind.

Various supraministerial planning organs served Nasser throughout his 18-year rule, appearing and disappearing unpredictably in a continual process of administrative reorganization that became a trademark of the regime. For starters, the PCDNP was complemented by a High Agency for Planning and Coordination in 1952 to supervise the development of public services. The latter was

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succeeded the following year by the Permanent Council for General Services. Then in 1955 a National Planning Committee was established that in 1957 absorbed both the general services council and the PCDNP. In the same year, however, the National Planning Committee was split into a supervisory High Council for National Planning and an organ to elaborate the details of its designs that retained the name *National Planning Committee*. There followed an independent Institute for National Planning in 1960, and finally a full-fledged Ministry of Planning in 1961, which was reorganized under the baton of the prime minister in 1964 (Abdel-Malek 1968, 109–10; Ayubi 1980, 226–31). And so it went. Clearly, one way to disengage bureaucratic agencies from conservative habits is to prevent the formation of habits altogether.

Needless to say, not all of these measures of restructuring were of equal moment, but occasionally they marked major turning points in government policy. Such was the establishment of the Ministry of Industry in 1956. 'Aziz Sidqi, the career bureaucrat who became the first head of the new ministry, was still in his thirties at the time of his appointment, illustrating once again how administrative reorganization can blunt the seniority principle. Holding a Ph.D. in regional planning from Harvard University, Sidqi had a solidly technocratic background, but this in no way dampened his enthusiasm for innovation (Baker 1978, 178). Sidqi supervised the drafting of Egypt's first five-year development plan, and to oversee its implementation, his ministry was given the power to license the establishment, expansion, or relocation of all industrial enterprises (Waterbury 1983, 70–71). As a young agency, the Ministry of Industry was not inhibited by close ties to private companies, and its policies often reflected an antagonistic attitude toward the private sector.

In the early 1960s, Sidqi was reportedly Nasser's closest collaborator in formulating the socialist decrees that brought most of Egyptian industry under state control, and he made certain that the Ministry of Industry benefited handsomely from the nationalizations (Waterbury 1983, 69–71, 77–80). Some 438 of the public companies to emerge in 1961 from various acts of expropriation were divided among 39 enterprise associations according to economic sector. These associations (*al-mu'assasa al-'amma*, variously translated as "general organizations" or "public organizations") were a new form of state institution standing midway between the regular ministries above and the public enterprises below. Each association exercised legal powers over its member companies, and it in turn was beholden to the legal authority of the ministry charged with its supervision. The Ministry of Industry initially controlled eight of the associations, covering such sectors as foodstuffs, petroleum, electric power, metallurgy, and chemicals and enveloping a total of 168 different companies (Ayubi 1980, 221–25, 236; Waterbury 1983, 80). These administered enterprise associations had their counterparts in Japan's control associations, which differed only by leaving most of the member companies under private ownership.

The Egyptian nationalizations, like the innovative policies pursued by bureaucrats in Japan and Peru, demonstrate how three different interests can harmonize together to produce radical bureaucratic action: (1) a political interest—in this case, a commitment to socialism involving at least the conviction that private capital and the free market did not work for the public good; (2) a rational-technical interest—the desire to plan the nation's developmental course rationally; and (3) an organizational interest—the aim of expanding the size, budget, and authority of one's bureau. Rational-technical inclinations and organizational

concerns, so often thought to breed conservatism, proved to be completely supportive of a new political tendency in the three cases.

Nasser created numerous other agencies that labored to promote an innovative technocratic orientation or novel political commitments. One was the Ministry of National Guidance (renamed several times), which took over the supervision of Egypt's nationalized newspapers (Harik 1974, 131; Rugh 1979, 37-38). Another was the Ministry of Agrarian Reform, organized to take forthright action in a policy field thought to be hampered by the foot-dragging of established agencies. Like so many radical bureaus serving the three military regimes, this one was placed directly under the president and operated independently of any external bureaucratic authority (Harik 1974, 249-50). The Suez Canal and the High Dam at Aswan were also administered by new agencies that enjoyed a special autonomy. In these cases, the innovation desired was efficient management rather than any change of a political nature (the former perhaps more difficult to achieve in the Egyptian context than the latter), and the new bodies were staffed with officials educated in engineering and other technocratic fields. The results were two of the most competently managed organs of the Egyptian government (Ayubi 1980, 273-80). As a rule, however, the agencies engaged in policy innovation in the three countries functioned neither more nor less efficiently than their bureaucracies as a whole.

The creation of new agencies did not usually involve the hiring of new bureaucrats. Although some top administrators were appointed from outside the government in Peru, new agencies in all three countries were generally staffed with bureaucrats already employed by other state institutions; in terms of personnel, the same deck of cards was being reshuffled to produce a new hand. Rather than

serving as an entry point for outsiders, new agencies more often performed as channels through which ambitious career administrators could gain entry into the political elite. In Egypt, both the first minister of industry, 'Aziz Sidqi, and the first head of the High Dam authority, the engineer Sidqi Sulaiman, subsequently rose to the prime ministership, and more than half of the 186 cabinet ministers to serve between 1952 and 1973 had labored in the civil bureaucracy (Putnam 1976, 50). There were many examples in Japan and Peru as well of administrators gaining posts of national prominence due to their roles in innovative agencies.

For a military elite with radical objectives, an alliance with civilian bureaucrats is not a marriage of convenience; it is a marriage of necessity. The officer corps itself is simply unequipped to plan and implement major new programs. In 1961, Egyptian bureaucrats with a military education numbered only three of the 210 top administrators in the Ministry of the High Dam, one of 110 in the Ministry of Planning, four of 262 in the Ministry of Culture and National Guidance, and none of the 88 in the Ministry of Agrarian Reform. In 1967, only 370 (2%) of the government's 17,828 top administrators had military backgrounds (Ayubi 1980, 348, 350). In Peru, where military officers were more numerous in bureaucratic posts than in Egypt or Japan, only about four hundred of the five thousand members of the officer corps held policy-making posts in the civilian administration. Of the 450 thousand employees in the public sector as a whole, less than one-tenth of 1% were soldiers (Cleaves and Scurrah 1980, 84). In all three countries, the military played the critical role of establishing a regime supportive of change, and military officers supplied the impetus for many radical programs, but the civilian bureaucracy was not simply a passive tool following orders. Civilian bureaucrats themselves sponsored some

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of the boldest innovations, and thanks to various forms of administrative reorganization, they fully earned the status of equal partners in the task of revolution from above.

Conclusion

The argument for inherent bureaucratic conservatism is that characteristic administrative structures and career patterns effectively block sharp policy innovations. This argument is contradicted by the record of civilian bureaucratic institutions in Japan, Peru, and Egypt, where bureaus manned by professional career administrators actively instigated radical innovations in public policy. Moreover, these innovations resulted from incentives and measures of reorganization that are found in most modern state bureaucracies and that systematically thwarted the elements commonly alleged to foster conservatism.

Regarding recruitment practices, although conservative standards often prevail for initial entry into the bureaucracy, the creation of supraministerial agencies and new specialized ministries (often on the advice of bureaucrats themselves) allowed for a more selective grouping of officials anxious to promote innovation. The notion that group decision making breeds conformity to existing policies obviously does not apply where entry into the group in question depends upon a prior commitment to change; in that circumstance, group pressures to conform work in favor of innovation. The seniority principle was easily violated by new agencies and those directly under executive authority, which offered rapid advancement to officials in midcareer without regard for an established hierarchy. As for deep-rooted ties to client groups in civil society that might have slowed bureaucratic innovation, such ties had not yet developed in most of the new

agencies and younger ministries that championed policy change. Rational-technical concerns did not prove incompatible with the adoption of radical political ideologies by many key administrators; indeed, it is ironic that rational-technical training should be thought to exclude a broader preoccupation with political values when so many contemporary ideologies call for a planned, organized, and technologically sophisticated transformation of society. The incremental approach to problem solving, said to blind specialized bureaucrats to the consideration of systemic reforms, was no hindrance to supraministerial agencies given broad policy-making responsibilities and drawing their officials from many different fields of specialization. Finally, legal restraints upon bureaucratic action did not pose severe obstacles to change in situations where executive and administrative decrees were either the principal (Japan) or the only (Peru, Egypt) sources of law itself. Respect for internal regulations may even improve the prospects for innovative agencies by allowing radical administrators at the top to control the actions of more conservative subordinates (Blau 1955, 189-91; Crozier 1964, 202-3).

These findings cannot be passed off as anecdotal because the institutional devices that generated new programs in Japan, Peru, and Egypt—supraministerial planning organs, low-ranking agencies, and new specialized ministries—are regular features of state bureaucracies throughout the world. In fact, they have been used to further innovation by all types of political regimes. Examples in a democratic context would include the way that President Richard Nixon's National Security Council changed the basis of U.S. foreign policy (Kissinger 1979, 23-24, 38-48), and the role of the Commissariat du plan in developing France's postwar industrial policy (Shonfield 1969, 128-30). Franklin Roosevelt's

record as a progenitor of new bureaus, launching 195 federal agencies during the New Deal (Strauss 1960, 57), almost rivals that of Nasser. The same devices have also served mobilizational single-party regimes. Examples are the State Planning Committee (*Gosplan*) in the Soviet Union and the Ministry of Propaganda and Reich Chambers of Culture in Nazi Germany, which served as models for Japan's Cabinet Information Bureau. A broader review of bureaucratic systems, rather than showing these cases to be eccentric, would reveal yet other mechanisms capable of unlocking the shackles of conservatism. Ministerial cabinets, for example, also recruit without deference to seniority and can assemble a select crew of administrators devoted to a particular course of change. A supraministerial bureaucratic stratum, such as France's Grands Corps, may have the same potential to promote change as the supraministerial agencies studied above (Crozier 1964, 197-98, 254-55). The cases examined in this article furnish extreme examples of innovation, to be sure, but while the degree of innovation was extraordinary, the means by which innovation was accomplished are simply too familiar to interpret these cases as rarities that somehow confirm the general validity of the conservative argument.

The article points to two general conclusions. The first is that the attempt to formulate a grand theory of bureaucratic politics per se is premature, given our limited knowledge of the subject and its complexity; research might proceed more profitably toward middle-range explanations of how a limited number of bureaucracies of a certain description have functioned. The second conclusion is that theorizing about bureaucracy at any level may be an enterprise fraught with intrinsic limitations.

The experience of administrative radicalism in Japan, Peru, and Egypt points to several promising grounds for middle

theories of bureaucratic politics. One approach would be to examine bureaucratic behavior in different political regimes. Although there has been interesting research on this topic (Caiden 1971, chap. 11; Esman 1966; Fainsod 1963; Heady 1966, chap. 6), the literature on bureaucratic conservatism does not address the significance of regime differences, an obvious deficiency in light of these findings. There is reason to hypothesize that the role of bureaucracies may differ depending upon whether the dominant political elite is the military, a democratic party, or some form of authoritarian party (not to imply that the regime context could be examined without more careful specification). Different ruling elites bring different resources to the task of policy making, and this may leave them more or less dependent upon the bureaucracy and more or less inclined to use it in certain ways.

A second basis for middle theory would be to focus on bureaucracies in particular regions or political cultures (see Crozier 1964, 210-12). Cultural differences do not pose insurmountable obstacles to comparison, but an Achilles' heel of the conservative stereotype has been its relative emphasis on evidence from the Western industrial democracies. Greater attention by theorists to studies of administration in other regions might bring to light more divergent patterns based upon the role of bureaucracy in traditional society, the colonial bureaucratic legacy, or common popular attitudes toward bureaucracy. Any of these factors might affect the likelihood of innovative policy making. In Japan, for example, the traditional legitimacy and prestige associated with bureaucratic governance has emboldened administrators to advocate new programs in many periods of this century. Arguably, innovative bureaucrats are more often blocked by conservative politicians in Japan than the other way around. Bureaucrats might be less inclined to take

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such initiatives in Latin America, however, where administrative action is not accorded the same legitimacy and respect.

A third basis for middle theory would be to examine bureaucratic behavior in times of crisis. Crozier (1964, 196-98, 225-26) and Leemans (1976b, 16-17) both note the important connection between crises and bureaucratic innovation, but most evidence for administrative conservatism from Europe and the United States (including that of Crozier) is taken from a postwar era of unusual stability, when all Western elites, bureaucratic or otherwise, have largely shunned radical change. Data from this setting cannot sustain universal propositions regarding conservative policy making. Comparative studies of how bureaucracies have responded to severe depressions, domestic violence, or foreign wars might point to different conclusions. These, after all, are the events that tend to provoke innovative responses from all segments of society, and bureaucrats should be no exception. If Blau's (1955) study of a New Deal agency is any indication, Western bureaucrats of the crisis-ridden 1930s were much more imaginative than their descendants are portrayed today.

A final ground for middle theory would be to link theoretical statements to particular forms of bureaucratic organization. The traits that distinguish bureaucracy from other types of social organization are not sufficient to press institutional action into a single mold or to impose the same behavioral pattern on all administrators. In practice, bureaucratic organization is not a generic entity but allows of many variations, and they do not all provide the same incentives and disincentives with respect to innovative policy making. Theoreticians sometimes assume that bureaucratic structures are sufficiently alike to be described by a single list of tendencies, but studies focused on supraministerial agencies or younger ministries might produce results

different from those aimed at other forms of bureaucratic organization.

The second conclusion is that all explanations of how organizational structures affect institutional or individual behavior face certain limitations, regardless of the theoretical level of abstraction. No institutional setting offers a definitive explanation for the human behavior that occurs within it, and this is certainly true regarding bureaucracy. Bureaucratic policy making is strongly influenced by external factors such as historical circumstances, cultural values, and the impact of other social institutions. Organizational structures themselves contain a complex potpourri of stimuli that can sway individual behavior and collective action in contradictory ways. As important as it is to understand the impact of these structures, structures alone merely offer opportunities for social action. At every step, the decisions made by human beings as to how they will use those opportunities are critical to the policy-making process, and these decisions are made in large part free of structural determination. Supraministerial agencies may offer different opportunities and incentives to administrators than established ministries, but they are no more a guarantee of innovative policy making than an ordinary ministry is of conservatism. Events in Japan, Peru, and Egypt underscore the crucial role played by the individuals who create and manage these structures. Without an appreciation for the subjective input of men like Kiwao Okumura, Benjamín Samanez, and 'Aziz Sidqi, the action of bureaucratic institutions in these countries is rendered incomprehensible.

In a word, theories of bureaucratic politics must complement structural and occupational variables with a keen appreciation for the role of bureaucratic leadership (Lewis 1980). Too often the significance of individuals is lost in structural studies that reduce the human subject to a function of his or her occupation or insti-

tutional environment, many making no mention of bureaucrats with proper names. When taken to extremes, occupational and structural explanations dehumanize social actors, and they thereby impoverish our understanding of social action, rather than enhance it. As long as the people running state bureaucracies remain all too human, even the best efforts to gauge the effect of organizational structures on public policy will fail to explain many important episodes in the history of bureaucratic politics.

Notes

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1. Another prominent contention is that bureaucratic conservatism is reinforced by the absence of market competition, which many economists consider a vital stimulus to innovation (Roessner 1977, 344-45; Rainey, Backoff, and Levine 1976, 235). This argument is omitted here because the radical changes discussed in the country studies below are not the sort of innovations likely to be stimulated by market competition in any case; therefore they are not relevant to consideration of this variable.

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STACKING THE DECK: BUREAUCRATIC MISSIONS AND POLICY DESIGN

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Empirical studies suggest that mission-oriented bureaucrats bias their design of program alternatives to increase the odds that a superior will choose the kind of program the officials want. However, political executives may anticipate this manipulation and try to reassert control. These struggles are examined in three models. In Model 1 a senior bureaucrat is interested only in missions; the bureaucrat's political superior controls him or her by rejecting inferior proposals and entertaining new options from other policy specialists. Model 2 is a principal-agent analysis. Here the official is interested only in budgets; the official's superior reduces search bias by creating an ex ante incentive scheme. In Model 3 the bureaucrat cares about both budgets and programs; the superior uses both his or her final review authority and ex ante incentives to reduce agenda manipulation. The models' contrasting implications for the political control of bureaucracy are examined.

The air force is dominated by bomber pilots. The corps of engineers is dominated by civil engineers. The navy is dominated by carrier admirals. These agencies tend to develop a sense of mission, an orientation toward a particular means as well as particular ends (Halperin 1974). Indeed, devotion to the means can overwhelm an agency's attachment to the ends: "To say there is not a deeply ingrained prejudice in favor of aircraft among flyers . . . would be a stupid statement for me to make. Of course there is" (General Thomas White, air force chief of staff, quoted in Futrell 1980, 253). This phenomenon, often called goal displacement (Merton 1957), has numerous causes: homogeneous professional training of an agency's leadership (Kanter 1979; Kaufman 1960; McGregor 1974),

genuine conviction that the national interest is well served by carrying out the mission, and career motivation (promotions are easier to obtain if we continue to do what we know how to do). Political superiors may, however, construe an agency's job in broader terms. The corps of engineers is supposed to protect citizens against flood disasters; building dams is only one method for doing that. From the perspective of the secretary of defense, the navy is supposed to strengthen national defense. Deploying aircraft carriers is only a means to that end, and if technological changes in, for example, surface-to-surface missiles render carriers obsolete, then the secretary may want to change the navy's force structure.

The ability of politicians to alter the repertoire of agencies is, however, con-

strained by their lack of technical expertise. The relative merits of strategic bombers versus ICBMs are better known to specialists in the department of defense than to politicians. Indeed, policy alternatives are often generated by experts in the first place (Polsby 1984). Politicians dominate agenda formation in the sense of determining the *problems* that have priority but specialists dominate the generation of *solutions* (Kingdon 1984; Light 1983). Frequently—by no means always—the specialists are bureaucrats.¹

This informational asymmetry gives bureaus an opportunity; their mission orientation provides a motive. When asked to generate policy proposals for review by their political superiors, bureaucrats are tempted to bias the search for alternatives so that their superiors wind up selecting the kind of program the agency wants to pursue: "Thus alleged 'options' are often advocacy in more sophisticated guise, one real choice and two or three straw men" (Destler 1974, 135).² Such manipulation is the stuff of Washington folklore. In general, biased advocacy is the norm: "The sensitivity of military groups to new program needs depends largely upon service doctrine and service interests. The Air Force was active in pushing strategic deterrence and the Army in innovating European defense since each program was closely related to existing service doctrine. All the services were hesitant in pushing continental defense and limited war, however, which were alien to existing service doctrine" (Huntington 1961, 288; see also Bergerson 1980, 64–65; Coulam 1977, 93–96). Such descriptions point up the influence of career bureaucrats, an emphasis often found in public administration and organization theory (Allison 1971, chap. 3; Pfefer 1981, 119–21; Simon, Smithburg, and Thompson 1950, 533–34).

An equally well established research tradition has emphasized how politicians try to reassert their control over the

bureaucracy. (On the White House's efforts to direct the bureaucracy, see, among others, Aberbach and Rockman 1976, Arnold 1976, Hess 1976, Lynn 1981, March and Olsen 1983, Moe 1985, Nathan 1975, Salamon 1981, and Seidman 1970.) One type of control rests on the ability of political executives to review and possibly reject all of a subordinate's options. This is, after all, a superior's formal prerogative. Recognizing this, a bureaucrat may be deterred from proposing an idea that furthers the agency's objectives rather than the superior's. Of course, the threat of a veto must be credible. If a subordinate believes that the superior would be better off accepting the subordinate's preferred alternative than suffering the delay following a rejection of all proposals, the subordinate will consider the threat of rejection a hollow one and rig the agenda anyway.

A political executive can strengthen his or her hand by having more than one subordinate generate options, concurrently or sequentially. Stimulating competition among subordinates was central to Franklin Roosevelt's administrative strategy (Schlesinger 1958, 534–35), and with good reason: threatening to turn down a subordinate is more credible if there are other options waiting in the wings. (For experimental evidence see Sutton, Shaked, and Binmore 1986.) Though simultaneous competition may be rare—an executive must incur the cost of diverting the attention of subordinates from other problems—the sequential strategy may be fairly common.

A second type of control is to reward the bureaucrat for devising options that are useful to the superior. If the politician can evaluate the political worth of an alternative, then even if he or she cannot observe how an agency generates the proposals the politician may be able to induce the agency to keep his or her interests in mind. Investigating such incentive schemes has been an active area of

research in the economics of organization (Arrow 1985). The analysis, known as principal-agent theory, has a simple logic. A superior (the principal) knows that the subordinate (agent) enjoys an informational advantage. For example, the agent may possess technical expertise or the principal may be unable to monitor the agent's actions perfectly. The principal believes the agent may exploit these advantages for the agent's own purposes. Anticipating this, the principal pre-commits him- or herself to a reward scheme *before* the agent acts. The payoffs to the agent are contingent on outcomes the principal can observe. A key task in this research program is to examine the conditions under which a superior can induce a subordinate to choose precisely as the superior would have, had the latter chosen directly.

The ideas underlying principal-agent models have diffused into political science (Moe 1984), perhaps encouraging some scholars to believe that the older concern with bureaucratic influence has been unwarranted. Though politicians may be "mere dilettantes" on technical issues, they are professionals on strategic affairs, and, as we have learned from Neustadt (1960) and others, inducing a subordinate to do as one would wish is a matter of strategy. Hence descriptions of bureaucrats rigging the choice sets of politicians and political appointees may be seriously incomplete, for they focus only on the last steps of the decision process, neglecting how search may be conditioned by incentives created by superiors.

These issues are central to our understanding of policy making. If bureaucrats bias the search for options, we should investigate their policy goals and how they try to attain them. Alternatively, if politicians anticipate these problems and control them, students of the policy process may be able to ignore the bureaucracy, at least insofar as policy generation is concerned. As indicated, scholars disagree

which view is more correct. It seems likely that—under the right conditions—each has merit; the problem is to specify just what those conditions are. To help us figure this out, we develop several models of political control, investigating the effects of both a review process and of incentives. In the first section, we study how a mission-oriented bureaucrat designs alternatives when he knows that in the final review his superior will reject unsatisfactory proposals and consider competing proposals. In the second section, we develop a principal-agent model to analyze how a superior could, in addition to reviewing proposals, use the carrot of bigger budgets to induce the bureaucrat to carry out the search the superior would prefer. (To avoid confusion, we assume the superior is female, the subordinate, male, in all three models.) We initially simplify the analysis by assuming in the first section that the bureaucrat has only programmatic preferences and in the second section that he cares only about budgets. In the third section we consider a more general model in which the bureaucrat is interested in both missions and budgets; as in the second section, the superior uses both types of control strategies. The models' main features are summarized in Table 1.

To keep the focus on substantive issues, we present the model informally in the text. The Appendix presents the mathematical version.

The Basic Framework

The three sections share a common structure. A political superior in the executive branch, such as the president, a department secretary, or other high appointee, instructs a subordinate, such as a bureau chief, to prepare several proposals addressing a problem that concerns the superior. The superior knows that there are two qualitatively different kinds

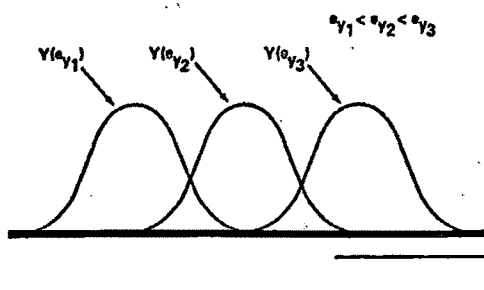
Table 1. The Models' Main Features

Parameters	Model 1	Model 2	Model 3
Bureaucrat's goals	mission only	budget only	missions and budgets
Superior's method of control	final review	final review and budgetary incentives	final review and budgetary incentives

of programs she wants to evaluate: type x (e.g., missiles) and type y (e.g., planes). Indifferent to the programs' technological and administrative characteristics, she cares only about the political benefits they would produce. Not knowing which one would better serve her goals, she instructs the bureau to draft two proposals, one of each type. The bureau has a fixed amount of effort to devote to this task. Effort may be a complex function of person-hours allocated to each design, weighted by the ability of the staff. Let e_x denote the effort the bureau allocates to designing the type- x program and e_y the effort it spends designing the type- y program. This allocation of search effort cannot be observed by the boss.

We assume a simple search technology. Let $X(e_x)$ represent the political benefits that accrue to the boss for a given search allocation, e_x ; similarly for $Y(e_y)$.³ To fit the uncertain quality of planning, we assume that X and Y are random variables. Exactly what the *realized* benefits are, for any given search allocation, depends on a host of idiosyncratic factors that cannot be predicted with certainty.

Figure 1. More Design Time Produces Stochastically Better Proposals



Consequently, the bureaucrat knows the a priori distribution of benefits but will only discover the worth of a specific proposal after he has finished planning.

We assume that thinking improves the quality of solutions: $X(e_x') \geq X(e_x)$ when $e_x' > e_x$, and similarly for Y . Graphically, this means that more design time shifts the distribution of possible proposals to the right (Figure 1). To follow standard terminology, $X(e_x')$ is *stochastically larger* than $X(e_x)$. To avoid pedantic repetition, we shall usually just say that one distribution is better than another.⁴

Once the bureau chief has prepared two proposals, he presents them to the superior. We assume that she knows the political benefits she will reap by implementing either proposal. If only one option is satisfactory, she chooses that one; if both are acceptable she picks the better of the two, denoted $\max(X, Y)$. In either case the decision process ends. If she finds neither acceptable, she turns them both down and invites other specialists—other agencies, her own staffers, outside think tanks, and individual policy entrepreneurs—to propose new alternatives.⁵

Let Z denote the amount of political benefits that would be generated by outside alternatives. Because these outside proposals are not the special province of the agency, we assume that their distribution is known by both the superior and the subordinate. In this second and final stage, the boss chooses the best of the three proposals, denoted $\max(X, Y, Z)$.

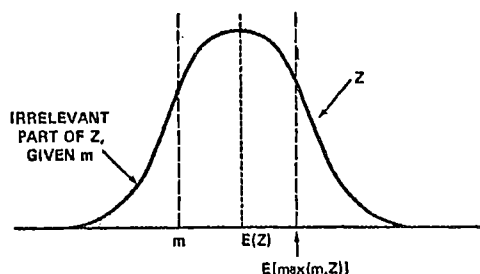
Thus in the basic model each decision maker must make a choice. Consider the

superior's problem first. She must decide whether to accept the better of the bureau's two proposals in the first period or to wait, expecting that other policy specialists will generate something better. Her acceptance level a is a conditional rule of the form, "if $\max(X, Y)$ is above a , accept it; otherwise, defer the selection until the second period."⁶ The level of the optimal acceptance or stopping rule a^* depends on the executive's utility function. We assume her utility is proportional to a proposal's political payoffs. In addition, she prefers sooner to later, discounting the value of future proposals by a parameter δ_s between zero and one.

To see how she would compute a^* , consider a particular value of $\max(X, Y)$ called m . If m is high, other proposals are unlikely to beat it. And if the executive is impatient (a low δ_s), the cost of waiting is great. In such circumstances, she would take m in the first round. If m is poor, the odds are good the outside competition will offer something better. And if she is patient, she does not incur significant delay costs. In such circumstances she would reject m and make her final decision later. In general she will reject m if and only if it is less than the discounted value of what she could get in the second round, $\delta_s E[\max(m, Z)]$ (see Figure 2). Thus, she sets a^* so that she is indifferent between taking $m = a^*$ now and taking the expected value of $\max(m, Z)$ later.

Consequently, the superior's optimal acceptance rule depends only on her knowledge of Z and on her utility function, but not at all on her beliefs about the distributions of X and Y or even on her knowledge of the realized value of the proposals. In game-theoretic terms, the superior has a best response to the bureau's choice that is not contingent on that choice: it is a dominant strategy for her to pick a^* in the above manner.⁷ Thus, there is a fundamental difference between this control strategy of review and the control strategy of incentives

Figure 2. The Expected Value of the "Good Tail" of Z



introduced later. In the former, the superior will make her best move at every point in the process, rejecting a first period option only if she expects to improve upon it by waiting. In contrast, the incentive strategy presumes, in common with most principal-agent models, that the superior can *precommit* to a schedule of rewards before the subordinate makes his move. The second strategy requires precommitment: if the subordinate believed that his superior was not obligated to keep her promise once she inspected the agency's proposals, the incentives would lose their force.

The subordinate's problem is to pick a search strategy, e_y^* , that maximizes his expected utility, given his goals and the prevailing incentives. In the pure mission-oriented model we shall assume that he values the type- y program (planes) over type x (missiles), though either is preferred to letting an outsider win, so $u_y > u_x > u_z$.⁸ For simplicity and with no loss of generality, we set u_z equal to zero. To ensure the two decision makers have conflicting interests we assume that X is better for the superior: if the bureaucrat devotes equal effort to each, then $p(X > b) \geq p(Y > b)$ for every benefit level b .

The bureaucrat prefers having a proposal accepted in the first period to having the same proposal accepted later. Thus $u_{y2} = \delta_b u_{y1}$, where the numerical subscripts denote the period and where

δ_b , the bureaucrat's discount factor, is between zero and one. Similarly $u_{x2} = \delta_b u_{x1}$.

The tensions facing a mission-oriented bureaucrat are now evident. The agency would like to manipulate the superior's agenda so that she will accept a type- y proposal in the first round. However, the bureaucrat knows that x is a better kind of program for the boss, so to increase the odds of beating out the competition he may be forced to spend time designing x s.

In the pure budgetary model the bureaucrat's expected utility depends only on the appropriation he will receive for the accepted program. His search strategy is therefore driven by the budget policy, which the superior designs in advance. The budget policies are described below. In the complex model, the bureaucrat still prefers planes to missiles to losing to an outsider and is also interested in funding. His optimal search strategy therefore must trade off these two goals.

Most principal-agent models focus on either of two kinds of informational asymmetry: the unobserved actions of subordinates (moral hazard) or their unobserved personal characteristics (adverse selection). Our work emphasizes moral hazard. Thus, in all three models, we allow the superior to know the bureaucrat's preferences; similarly, he knows her acceptance level.

Model 1: The Mission-oriented Bureaucrat

In this section we investigate how a purely mission-oriented bureaucrat exploits his expertise in order to win approval of his pet program. First we examine absolute levels of stacking the deck, for example, when search will be extremely rigged. Next we use comparative statics to examine changes in search.

Clearly the bureau chief can control the process if his superior's acceptance level is

low. There are two main reasons why the superior would be so undemanding. In each case the consequence is a rigged agenda.

PROPERTY 1.1. *If the outside competition is sufficiently poor, or the executive sufficiently impatient, the bureau stacks the deck completely by spending all its time designing the y alternative.*

Both conditions make intuitive sense. For the boss, the benefit of further search is the possibility of discovering a superior proposal; the cost, the cost of delay.⁹ If the benefit is too low, or the cost too great, she will stop the process in the first period by accepting the bureau's better alternative. And if the bureau chief is sure he can satisfy his superior in the first round, he prefers to do so with a proposal consistent with his mission orientation.¹⁰

Now we move to the opposite extreme: a mission-oriented bureau that does exactly what its superior wants—more precisely, what the superior *would* want were she completely informed about the quality of the two types of programs. Let us call the search that a completely informed superior would carry out *unbiased* search. (In principal-agent models, the generic term for the choice of a fully informed superior is the "first-best" solution.)

PROPERTY 1.2. *A combination of strong outside competition and a patient superior can induce a mission-oriented bureaucrat to carry out unbiased search.*

In this situation the outside proposals are so promising that the agency has a chance of getting a proposal accepted only if it spends all its time on the superior type of program, x . Spending time on y is futile. In such circumstances the executive's first-best search would be to ignore y completely, which is just what the agency is driven to do.¹¹

What counts here are the *relative* levels

of the outside options, the acceptance threshold, and Y . If the bureaucrat knows that no matter how much time he spends working out the bugs of his mission program it still would be politically dreadful for his superior, then his only chance of getting a proposal accepted is to concentrate on x , even if the outside alternatives are mediocre.

Thus, a mission-oriented bureau tightly constrained by outside forces may do just what a fully informed boss would want. In these circumstances the control strategy of review works as well as could be desired. But Property 1.2 described sufficient conditions for unbiased search, not necessary ones. Can internal controls substitute for external ones? If the bureaucrat is neutral, that is, indifferent as between x and y , and has the same time preference as his superior, then he is trying to maximize the (time-weighted) probability of receiving her approval. Surely this orientation must make him do what she wants? Not necessarily.

PROPERTY 1.3. *Mission neutrality and identical time preferences do not guarantee unbiased search.*

Neutrality is insufficient: whereas the superior likes high values of $\max(X, Y)$, the bureau chief is indifferent between a proposal that just squeaks by and one that far exceeds the acceptance level.

Property 1.2 implied that search is not always biased toward the bureau's pet program. One might think that at least search will never be skewed toward the other type. This too is wrong.

PROPERTY 1.4. *Despite the bureaucrat's preference for the y program, a purely mission-oriented bureau may spend more time designing the x alternative than a fully informed superior would want.*

Consider this example. The superior is patient and will nearly always defer the final program choice until the second

period. This worries the bureau chief who is impatient and craves a first-round decision. In the slim hopes of discovering an outstanding proposal that will induce his superior to look no further, he throws all his organizational resources into investigating the more promising type, x . The patient superior, however, considers a second-round decision nearly as good as a first-round selection. Therefore, unbiased search would approximately maximize the average value of the best of the three proposals. If there were diminishing returns to search, it would be suboptimal to devote all effort to x . The bureau, however, does precisely that.

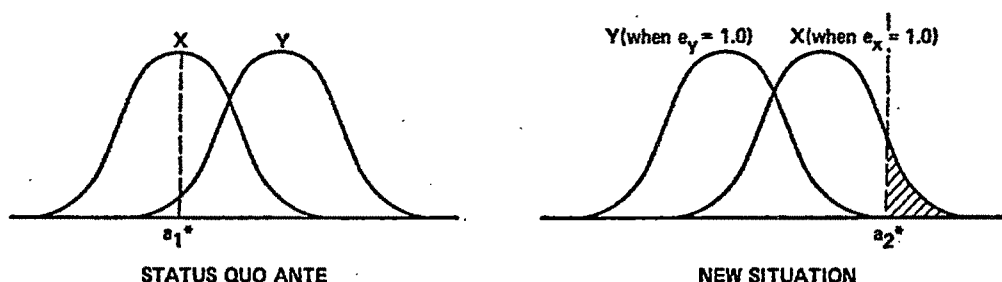
Now we investigate how agenda manipulation shifts in response to changes in the model's parameters, first examining changes in the bureaucrat's preferences and subsequently investigating changes in the strength of the outside competition. To simplify the presentation we explain the results with the aid of figures; derivations are in Section 12 of the Appendix.

Changes in the Bureaucrat's Preferences

Over time, the composition of a bureau's dominant coalition can change (Kanter 1979, 102-8), in turn altering its leader's preferences for different programs. How will a purely mission-oriented chief shift his search pattern as his programmatic preferences change?

Consider an equilibrium before the parametric change. The benefit of paying more attention to y is an increased chance that it will be accepted multiplied by the utility of y . The cost of doing this is a reduced chance that x will be accepted multiplied by its utility. These expected marginal gains and losses were equal at the old optimal e_y^* . If the bureau comes to value y more, the marginal benefits of devoting more effort to y rise while the marginal costs are unchanged. Hence the bureau increases its focus on y . Symmetrically, if the bureau values x more,

Figure 3. Bureaucrat Switches to x



the marginal costs of e_y^* increase while the marginal benefits remain unchanged, so the agency attends less to y .

This result is intuitive. The bureau chief's liking for y over x represents conflict of interest between the superior and subordinate. As this bias falls, conflict diminishes, and search should be less skewed toward the type of program that is inferior from the boss' point of view.¹²

What happens if losing to the outsider becomes less unpleasant, that is, u_z rises? Because this means that the bureaucrat cares less intensely whether the executive chooses x or z , the implicit costs of devoting effort to its mission have fallen. Hence search is skewed more toward his preferred program.

The Effect of Improved Competition

We know from Properties 1.1 and 1.2 that the quality of outside proposals influences the bureau's strategy. In policy formation in the United States, the trend seems to be toward improved competition. Many observers have noted a growth of think tanks and policy entrepreneurs in Washington. This growth probably affects policy: the Reagan administration is reputed to listen to ideas from institutions like the Heritage Foundation.

How will a purely mission-oriented bureau respond if the quality of these competing proposals becomes stochas-

tically better? Most beliefs about the restraining role of competition (e.g., a monopolist's ability to extract consumer surplus for itself) imply that the subordinate would stack the deck less as its competition improves. Further, in this model, improved competition raises the executive's acceptance level, making her more demanding. Since the bureaucrat is less likely to beat his rivals and will be evaluated more stringently, won't he spend more time on the type of program that he knows is superior for the boss?

Not necessarily, as the following pair of examples shows. In the first example the bureaucrat makes the expected move of paying less attention to y ; in the second he pays more attention. In both cases we simplify matters by assuming that the bureau has no chance of beating the outside competition in the second round, which allows us to focus only on the first period. (Alternatively one could assume that the bureaucrat discounts the future completely.) Improved competition still matters, however, because it raises the acceptance level in the first period.

The first example is shown in Figure 3. In the old status quo, the boss's acceptance criterion is moderately demanding. As revealed by the superiority of distribution Y , which occurs only when $e_y > e_x$, the bureau spends more time preparing y than x . The bureau does devote some time to x , possibly due to diminishing marginal returns to search.

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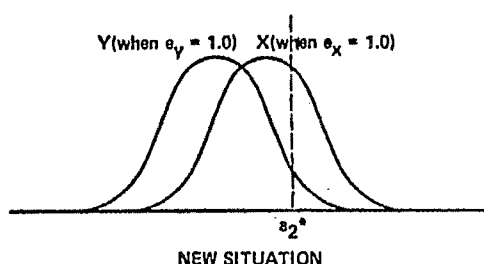
The second picture in Figure 3 depicts the new status quo, in which greatly improved competition has induced the executive to jack up her acceptance threshold by a lot. Now the agency has no choice: to have any chance of getting a proposal accepted it must spend all its time on x , the superior type. As expected, the bureaucrat becomes more docile in response to improved competition and tougher standards.

However, the next example of Figure 4 shows that just the opposite can happen. The status quo ante is the same as the previous example's. Then competition improves and the acceptance level increases, though less dramatically than before. Once again, to have any chance of designing an acceptable alternative, the agency must specialize. But unlike the previous example, y has a chance of being satisfactory, though of course the bureau is more likely to generate an acceptable proposal if it specializes in x . Which path the bureau chief chooses depends on how much he prefers y over x versus the relative chances of getting either one approved. If $u_y \times p(Y \geq a^*) > u_x \times p(X \geq a^*)$, given full specialization in each case, the bureau will focus more on y in response to improved competition.

This example is not bizarre; it is just a circumstance in which the bureaucrat's decision variable has increasing marginal utility and decreasing total utility. Clearly, if the bureau's rivals improve their proposals, the bureau chief's expected utility falls. The world has become a tougher place for the agency: it is less likely to win in either round. But because the administrator can do nothing about this, he focuses on the *relative* values of alternatives. And despite the fall of e_y 's absolute value, its relative value can increase—as it did in the second example.¹³

How does the superior fare in the new environment? There are two answers here. The partial equilibrium answer, based on a fixed search strategy, is that

Figure 4. Bureaucrat Switches to y



she is unambiguously better off. If the second round is needed, she will get $\max(X, Y, Z)$, which can only get better if Z improves. Therefore, in the first period, she can afford to forego comparatively mediocre options that she would previously have approved. Hence, if the bureau does not adjust its search, improved competition must make the superior better off.

The complete equilibrium answer, however, is that the boss is *not* invariably better off. An example is presented in Figure 5. For simplicity, Z is a constant, z . The superior is patient, with an acceptance level just below z . For simplicity, we assume that both X and Y have only three values: for a given allocation of effort, the bureau could generate a poor, a mediocre, or a good proposal of each type of program.

In the old equilibrium (Figure 5a) the bureau spends more time thinking about y , resulting in a small chance of a spectacularly good plan, y_3 . After z improves (Figure 5b) the bureaucrat realizes he can no longer afford to devote so much effort to y . Figure 5c shows the new equilibrium, after the bureau has adjusted its search by spending more time on x , thereby driving x_3 past the new z . The sharp decline of y_3 indicates that there are increasing returns to search from the superior's point of view. Because it is easy to ensure that the fall of y_3 more than offsets the increases of z and x_2 , in this case

improved competition ultimately hurts both the superior and the subordinate.

Of course it is possible that improved competition in the generation of policy alternatives typically enhances the well being of political superiors. If this is so, we suspect that it is because they take steps to ensure that their subordinates' interests are linked with their own. We examine such control strategies next.

Model 2: The Budget-oriented Bureaucrat

We now turn to the other end of the continuum to analyze the behavior of a bureau chief interested only in increasing his agency's appropriations. The basic structure remains the same: the bureau-

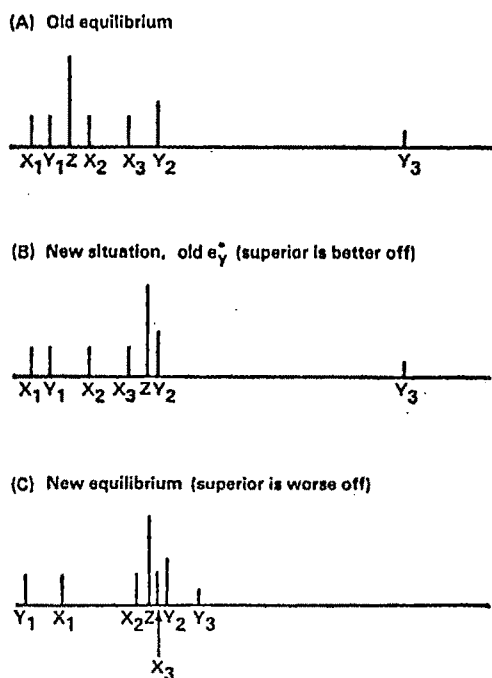
crat generates proposals; the political executive makes the final choice. Now, however, each actor enjoys a larger repertoire of choices. We shall examine in turn three new decision variables of the bureau chief. In addition to allocating effort, he can now (1) make his proposals more or less innovative, (2) reorganize his agency's planning to decrease or increase the correlation between his two proposals, and (3) draw on ideas floating in the "policy primeval soup" (in Kingdon's phrase) to alter correlation between his and outsiders' plans. (When examining Variables 1 and 3, we will suppress the distinction between the agency's two types of proposals, focusing directly on the derived distribution of $\max(X, Y)$.) The superior can design and precommit to incentive schemes. We consider three policies. The first is the simplest: if she accepts one of the agency's options, the bureau will receive a budget to administer that program. The funding grows in proportion to the program's political benefits. To maintain tractability we assume that the bureau chief is risk-neutral in the budget.

This incentive scheme is simple enough to be empirically plausible: all the superior needs to know is that the bureau chief prefers more funds to less. Moreover, because precommitting to an appropriation is a public matter, it is easier than precommitting to an acceptance level.

What behavior will this incentive scheme induce? Because here the bureau chief is interested only in funding and because he knows that his reward will increase in the quality of his better alternative, if the superior selects it, one might think that he will conduct unbiased search. However, consider the following properties.

PROPERTY 2.1. *The winner-take-all incentive policy does not guarantee that a budget-oriented bureaucrat will carry out unbiased search.*

Figure 5. Partial Equilibrium versus Full Equilibrium Analysis



a^* is not drawn ($a^* = Z - \epsilon$)

Problems arise because this winner-take-all scheme makes the bureaucrat ignore the value of outside alternatives to his superior. Consequently he places a lower value on innovative searches that may lose to outsiders, even if such strategies—when they succeed—generate excellent proposals. Thus this incentive policy creates the following conflict:

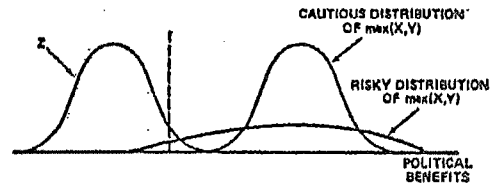
PROPERTY 2.2. *The political executive always prefers riskier search to more cautious search, if the associated distributions have the same mean. Under the winner-take-all scheme, however, the bureaucrat does not always prefer riskier search.*

Note that these preferences do not derive from attitudes toward risk: both actors are risk-neutral. It is the logic of the relationship that drives them to have different views toward innovation. The superior knows that if the agency's proposals are poor, she can defer her decision in the hope that an outside alternative will be superior. Thus, she is protected from downside risk. And, of course, she likes the benign uncertainty of outstanding alternatives. Therefore she always wants the agency to try more innovative policy design (Kohn and Shavell 1974). The bureaucrat, however, is not always protected from downside risk. Indeed, risky search may increase the chance of losing to outside competitors (Figure 6). In general, if prospects look very good for the agency, the bureaucrat will prefer more cautious search; if matters are desperate—little chance of winning in either round—he will prefer bolder search. Thus the logic of the situation can induce bureaucratic caution without assuming that administrators are personally averse to risk.

Implementation Budget Scheme

An intelligent superior might realize that the winner-take-all scheme is undesirable because it drives a wedge

Figure 6. Cautious versus Innovative Search



between her interests and her subordinate's. Alternatively it may be infeasible if the only rivals to the agency are staffers or think-tank analysts who lack the organizational resources for implementation. For either reason, we now consider a second incentive strategy: the executive decides in advance that the agency will administer whichever option she deems best, as well as the associated budget.¹⁴ Thus, even if she picks an outside proposal the agency will implement it.

This scheme does diminish some of the conflicts created by the winner-take-all system: because the bureaucrat is interested only in budgets and will be funded to implement outsiders' proposals, he will not in his own search shy away from innovative ideas just because they run some risk of losing to an outside alternative. Nevertheless, it is not a panacea.

PROPERTY 2.3. *The implementation budget scheme does not ensure that a budget-oriented bureaucrat will carry out unbiased search.*

The remaining problem is conflicting time preferences. It is often asserted that career bureaucrats are more patient than political executives (e.g., Hecl 1977, 143). The latter typically have short stays in Washington and both sides know it. Therefore, in our model, the patient bureaucrat might prefer a second-period choice of a wonderful outside alternative—which the bureau, blessed with a correspondingly large budget, would imple-

ment—to a first-period choice of its own programs (recall that the bureaucrat has no mission preferences here).

PROPERTY 2.4. *The superior always prefers a first-period search yielding a stochastically better distribution of alternatives to one yielding an inferior distribution. The bureaucrat has the same preferences—if he is less patient than his superior. If he is more patient, he will under certain conditions prefer search that generates inferior distributions.*

The superior's preference comes as no surprise. Because the bureaucrat's reward depends on the quality of the accepted alternative, one might think that he too would always prefer superior search. So he does, if he is more anxious than his superior to reach an early resolution. But if he is more patient he may sabotage the first period, hoping to receive much more funding later. The implementation budget scheme makes the bureaucrat internalize the value of outside alternatives; indeed, now he may overvalue them (see Appendix, sec. 10).

Thus far the superior's control methods have been predicated on a genuine diversity among policy specialists: the outside proposals have been assumed independent of the agency's, and (given a search allocation) the agency's proposals have been independent of each other. Because in the real world policy ideas diffuse, experts might think in similar ways. The superior may want genuine pluralism maintained; would the bureaucrat also like this? To examine their possibly conflicting preferences for diversity, we consider two kinds of cognitive interdependence: the first concerns the relation between the agency's proposals; the second, the relation between the agency's plans and everyone else's. We take up each in turn. When considering the first type we continue to assume that the out-

side alternatives are independent of the bureau's.

An agency can organize its planning in several different ways. One office could be charged with the task, or each program could be the responsibility of separate offices. If there are separate units, they could communicate with varying intensity. The more "tightly coupled" the offices, the more correlated (positively interdependent) the alternatives are likely to be. (For a precise definition of *positively interdependent distributions*, see sec. 11 of the Appendix.) Hence, the organization of proposal generation can be a strategic choice: given a fixed allocation of effort, the agency can decide to organize its planning in either a tightly or a loosely coupled way. These choices matter. To focus only on the effects of interdependence, we assume that the *unconditional distributions* of the two plans are unaffected by how tightly coupled they are.

PROPERTY 2.5. *The superior would always prefer the agency to organize its search to reduce positive interdependence between its alternatives. A less patient bureaucrat has the same preferences. However, a more patient subordinate will under certain conditions prefer more correlated search.*

The intuition behind the superior's preference is straightforward. Tightly coupled planning is more likely to produce two good proposals; it is also more likely to generate two bad ones. Because the superior needs only one good option, the disadvantage of two poor alternatives outweighs the advantage of two good ones. Thus she prefers pluralistic policy design. Indeed, a more pluralistic process yields a superior distribution of $\max(X, Y)$ (Bendor 1985, 47; Bhattacharya and Mookherjee 1986), so even a risk-averse superior prefers loosely coupled search.

This superiority of low correlation implies that Property 2.4. obtains. Thus

the bureaucrat, whose budgetary reward is contingent on the outcome, will also generally prefer loosely coupled search, unless he wants to sabotage the first round in order to wait for an outside option.

The second dimension of interdependence concerns the relation between the agency's search and the outsiders'. Typically, policy arenas form communities of common attention. Ideas for new programs diffuse in these networks (Heclo 1978; Kingdon 1984; Polsby 1984). When an agency designs policy proposals, it can draw in varying degrees on this fund of public knowledge. Thus, qualitatively different search strategies produce plans correlated in varying degrees with proposals from outside the agency. Would the superior prefer high or low correlation over time?

One might think that an extension of Property 2.5 would apply immediately: search that is loosely coupled over time is always better than more tightly coupled search. Not so, however: when time is introduced the problem becomes more complex.

PROPERTY 2.6. (a) *In sequential search the superior does not always prefer loosely coupled design strategies and (b) if the superior's stopping problem is classical in that her optimal strategy in the first period is to reject poor alternatives and to accept good ones, then she prefers less correlated search. However, even in this setting there can be conflict: the bureaucrat will under certain conditions prefer more interdependent search.*

The superior might prefer high correlation in the sequential setting—Result (a)—when good options are likely to be followed by much better ones. Consider the following simple example. In the first round, $\max(X, Y)$ will be either poor (m_1) or good (m_2). The two outside alternatives, z_1 and z_2 , are strongly correlated

with m_1 and m_2 respectively. Assuming that policy specialists learn from each other, the second-round proposals generally improve upon the earlier ones: z_1 is a bit better than m_1 , and z_2 is much superior to m_2 . Because z_1 is but a small improvement over m_1 , the superior will accept m_1 if she discounts the future moderately. However, she will reject m_2 in the hopes of doing much better later. Given this pattern of accepting poor first-period proposals and rejecting good ones, if the agency could figure out a way to plan so that its alternatives were even more positively correlated with the later ones, the superior would prefer that the agency do so. (For example, if the alternatives were perfectly correlated, rejecting m_2 would be riskless since it would always be followed by the wonderful z_2 .)

This example requires the counterintuitive acceptance strategy of accepting poor options and rejecting good ones. Yet, though it is counterintuitive, such a strategy can be an optimal stopping rule because, in a search context, alternatives have information value as well as intrinsic value. Poor alternatives are accepted not because they are intrinsically useful but because their appearance augurs ill. Conversely, good alternatives are rejected because they predict an even brighter future.

When the stopping rule is more conventional, loosely coupled search is again desirable—Result (b). Because the superior will initially spurn inferior options, she does not want the second set of proposals closely related to the first set. And because she accepts good alternatives in the first round, the value of high correlation—fine early options precede fine later ones—is irrelevant (see sec. 11 of the Appendix).

The bureaucrat's tastes, however, do not always run to less correlated search even in the classical setting. If, for example, he is sufficiently impatient, he will prefer any first-round proposal to any

second-round one. Therefore he would want his superior to have an extremely low acceptance level. But more loosely coupled search can induce the superior to raise her standard, making the bureaucrat worse off.¹⁵

Because the above problems stem from conflicting time preferences, a sophisticated political superior would take these into account while designing incentives. One cure is simple. If her subordinate is more patient, she should scale down the budgetary payoff in the second period; if less patient, she should increase it. (Equivalently, she could increase the first-period payoff for a patient bureaucrat and decrease it for an impatient one.) Typically, one would expect a political executive to know that her subordinate is more patient; how much so would be uncertain. To simplify matters, we make the less plausible assumption that she knows δ_b precisely. Then finally we arrive at a scheme that induces unbiased search.

PROPERTY 2.7. *If the superior uses an implementation budget scheme and rescales the second-period budget by δ_s/δ_b , a budget-oriented bureaucrat will conduct unbiased search.*

Under this scheme, the bureaucrat's valuation of a second-period budget is proportional to δ_s/δ_b , multiplied by his own discount of δ_b , so it is proportional to the superior's discount of δ_s . Therefore, the bureaucrat's induced utility function is now qualitatively the same as the superior's in both periods, eliminating all the conflicts over search strategies described in Properties 2.2 and 2.4–2.6. We should remember that the superior has induced this without knowing anything about the technical details of the search process, what the returns to policy analysis and design are, and so forth. Thus, bureaucrats' greater technical expertise may be countered by incentive systems that tie the career officials' goals to the fulfillment of politicians' objectives.

But the conditions guaranteeing unbiased search are restrictive. Most importantly, we have assumed in this section that the bureau chief is interested only in appropriations; he cares not a jot about the type of program he implements.

Model 3: Budgets and Missions

The most important case empirically is one in which bureaucrats are interested in both budgets and missions. We now assume that utility = $k_1(\text{program}) + k_2(\text{budget})$, where k_1 and k_2 are positive constants symbolizing relative intensity for programs and budgets respectively. (The earlier models are special cases in which either k_1 or k_2 is zero.) The problem is otherwise the same.¹⁶

Can the superior control this more complex subordinate? She can since she knows his programmatic preferences.

PROPERTY 3.1. *The following implementation budget scheme ensures that the bureaucrat will carry out unbiased search: (a) if the superior accepts a proposal in the first period, she appropriates a budget that increases linearly in the value of the accepted alternative; if she chooses x , she gives the agency an additional fixed amount of c_1 , which gives the bureaucrat a utility of $k_1/k_2(u_y - u_x)$; (b) if the superior makes her decision in the second period, she again gives the agency a budget composed of a linear term plus a constant; the linear part is rescaled by δ_s/δ_b . If she chooses x , she adds a constant, c_2 , which equals $\delta_b c_1$. If she accepts z , she adds a constant of c_3 , which gives the bureaucrat a utility of $\delta_b k_1/k_2(u_y - u_z)$.*

These incentives neutralize the bureau chief's mission orientation. Because he is indifferent between, say, implementing y in the first period and implementing x plus

getting the extra funding of c_1 , only the variable part of the budget matters. And because that increases in the realized political benefits to the superior, the decision makers' interests are bound together.¹⁷

Thus, despite her ignorance of how alternatives are generated, the political superior can get exactly what she wants—if she knows exactly what the bureaucrat wants. Note that the incentive scheme is only moderately complicated; the hard part is obtaining accurate information about the bureaucrat's preferences. (The limitations of assuming perfect information are examined in the concluding section.)

However, although the political executive can induce unbiased search, because she does not know the search functions, it is unlikely that the above incentive scheme is *efficient*: other schemes might make both decision makers better off, once one factors budget costs into the superior's utility function. For example, suppose c_1 —the cost of inducing the bureaucrat to treat x and y evenhandedly—is large and its expected benefit—the improvement in the average value of $\max(X, Y)$ —is small, say ϵ . For simplicity, assume that without the incentive of c_1 , the bureaucrat will spend all his time on y and that his superior will choose y in the first period with certainty; with the compensation of c_1 he will devote all his attention to x , guaranteeing that it will be selected. In this circumstance, the incentive scheme of Property 3.1 is Pareto inefficient. The superior would be better off foregoing the ϵ improvement of unbiased search, saving the large sum of c_1 ; the subordinate is nearly indifferent as to having y approved versus the combination of having x approved and receiving the lump-sum appropriation of c_1 plus the tiny budget increase proportional to ϵ . Therefore if she gave him a small fraction of c_1 unconditionally, they would both be better off than under the incentive scheme

that induces unbiased search.

Of course, in our model, the superior does not know the requisite facts about search, and though the administrator does have this information and therefore could point out when the incentive scheme is inefficient, the former cannot trust the latter not to exploit his greater expertise. Thus inefficient arrangements can arise here from a combination of asymmetric information and the subordinate's inability to commit to the mutually beneficial course of action of revealing his information.

Let us assume, therefore, that the scheme of Property 3.1 is in place. We can now readdress the comparative-static issues of the first model. Consider personnel matters. Just as politicians vary in their commitment to policies versus their drive to win elections, so do career bureaucrats vary in their dedication to programs versus their appreciation of bigger budgets. This variation would matter to political executives if they were concerned about the cost of implementing programs.

PROPERTY 3.2. *Assume that for a fixed level of benefits, the superior preferred smaller to larger budgets. Then, given the above budget policy, (a) the superior always prefers a less biased bureaucrat to a more biased one; and (b) the superior always prefers a bureaucrat more oriented toward budgets than one less oriented.*

The cost of neutralizing a mission orientation is less, the smaller the difference between u_y and u_x ; hence, Result (a). Similarly, the more appropriations matter compared to programs (smaller ratios of k_1/k_2), the more cheaply neutrality is bought; hence Result (b).¹⁸

Thus, given this incentive structure, personnel policy has an important monotonicity property: the more the bureau chief approaches the pure, budget-oriented subordinate, the better off his

superior is. This may parallel the electoral control of politicians. It is sometimes argued that politicians oriented toward winning would tend to drive out rivals oriented toward policy, particularly in competitive districts (Mayhew 1974, 13-15; Wittman 1983, 148-50). Similarly, one might expect budget-oriented bureaucrats to outlast more programmatically driven colleagues because the selection environment favors the former.

Now consider shifts in the policy arena. Recall that in the pure-mission model, the superior's prospects could decline if the outside proposals improved, a perverse consequence of the agency's response to tougher competition. This cannot happen when the appropriate incentives are in place.

PROPERTY 3.3. *Assuming the described budget policy, the superior's expected utility always rises if the outside alternatives become stochastically better.*

Indeed, almost all the parametric changes that had indeterminate effects on the superior's welfare in the pure-mission model now have, given the budget policy of Property 3.1, determinate and intuitive effects.¹⁹ Pursuing its own interests, the mission-oriented agency's response to improved competition could wipe out the gains to the executive. However, once the agency's interests have been bound to those of the politician, it will not alter its search in a way that harms its superior. Accordingly, as a mental shortcut, one can simply consider how parametric changes affect the superior. Thus the right incentives make the bureaucracy irrelevant in an important sense: one need not study it if one is interested in how political leaders—their careers, electoral prospects, and so forth—are affected by exogenous changes in policy arenas.²⁰ Further, because political benefits accruing to political executives are usually related to benefits accruing to various constituencies, if one is interested in

macro issues—relations between voters, interest groups, and politicians and how these shift as policy ideas change—then, again, one can ignore the bureaucracy.²¹

Conclusions

These models have strikingly different implications concerning the political control of bureaucracy. In Model 1—the pure mission-oriented case—a superior using only the (credible) threat of rejecting a bureaucrat's proposals is sometimes unable to prevent her agenda from being completely rigged, and only under the most fortuitous circumstances will the subordinate carry out unbiased search. In Models 2 and 3, adding the appropriate budgetary incentive schemes induces the bureaucrat to plan just as a completely informed political executive would. Perhaps more importantly, when these incentives are in place, changes in the larger environment (such as the quality of options in the "policy primeval soup") affect the superior in the expected way, whereas in Model 1 such changes can, via the agency's mediating influence, have perverse effects. Thus Model 1 directs our attention to bureaucracy's role in policy formation; Model 3 implies we can ignore it.

Ignoring the inner workings of bureaucracies would economize on the information-processing resources of the discipline. Most descriptions of agencies' roles in policy generation and implementation are richly detailed. Theory is hard pressed to absorb such complexity; it is easier to assume that policy proposals need not be designed and that the beliefs and preferences of career bureaucrats can be set aside.

Of course, starting with simple theories is a sensible intellectual strategy. And Model 3 suggests that when the appropriate incentives are in place, a unitary-actor interpretation of the executive branch may be a reasonable approximation of

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"its" behavior. But a close examination of this principal-agent model suggests that caution is in order.

First, though the final reward structure ensures unbiased search, the bureaucrat's programmatic preferences still matter, for they affect the cost of the incentive scheme. Therefore, the classic issue of the compatibility of politicians' and bureaucrats' interests remains alive.

Second, the incentive scheme reflects a partial optimization. A complete optimization would require that the superior balance the benefits of improved search against the cost of inducing the improvement. Executives would find this more global calculation much more difficult, for it requires knowing or having beliefs about the probabilistic returns to search, and then trading off expected marginal gains of inducing bureaucrats to move toward unbiased search versus the marginal costs. Control strategies and outcomes would then be shaped by what politicians believed was feasible, by how they weighted the advice of specialists inside and outside government, and in general by the politics of belief formation. Once again, experts' policy preferences may matter.

Third, inducing unbiased search, even without maximizing net benefits, requires knowing the subordinate's goals exactly. This is implausible. Though agencies' mission orientations are part of Washington folklore, a political appointee will not know precisely how much a bureau chief prefers one program to another. Therefore an incentive scheme will never neutralize an agency's search exactly; there will be undercompensations and overcompensations.

To be sure, conclusions built on assumptions of perfect information may be robust, that is, introducing a "small" amount of uncertainty may lead only to "small" changes in results. For example, it has been shown recently that the control of politicians by voters in the classical

spatial model is robust in this sense (Calvert 1985b); perhaps this will also hold true for the control of bureaucrats by politicians. Most importantly, one would like to know about two kinds of robustness. First, do small misperceptions of the subordinate's goals produce small errors in the incentive scheme, in turn diminishing the superior's expected benefits by just a bit?²² Second, if the incentive scheme is "nearly" optimal, do the comparative-static results of Model 3—for instance that the superior prefers bureaucrats more oriented to budgets to those with more programmatic concerns (Property 3.2)—still hold up? Both questions are for future research to settle; clearly affirmative answers would give us more confidence in principal-agent models.

Fourth, the incentive scheme becomes more complex as the principal and agent differ on more dimensions. The scheme for the purely budget-oriented bureaucrat was simpler and required less information than the one designed for the administrator who cared about programs as well. In the real world, politicians and agency officials will differ by still more dimensions (Aberbach, Putnam, and Rockman 1981). Of course, politicians could deploy rough-and-ready reward schemes—indeed, we think these more common than refined ones. But as we saw with the first policy, the simple scheme of Property 2.1, rough-and-ready rewards are not likely to ensure the best results. In this domain it seems that simple control strategies are suboptimal, possibly significantly so.²³

The basic idea of principal-agent models is that superiors, anticipating that subordinates might exploit informational asymmetries for their own purposes, establish controls with this in mind. This is a valuable corrective to an overemphasis on bureaucratic influence. But we are struck by how demanding the prerequisites for full control are. Indeed, we suspect that the modern tools of principal-

agent analysis and search theory will demonstrate that rational superiors would not strive for complete control: even if feasible, it would be too expensive. Because of these limits, the older literature on bureaucratic influence still has much to teach us.

Appendix

This presentation of the model does not parallel the text's sequence. Note too that only mathematical properties not obviously established in the text are derived here.

1. The bureaucrat's fixed search budget (normalized to unity) is spent on generating proposals of two types, x and y : $e_x + e_y = 1$. The political benefits of these proposals to the superior are independent random variables X and Y , whose probability distributions are stochastically increasing in e_x and e_y , respectively. We assume that for equal effort levels e_x and e_y , X is stochastically larger than Y .

Whereas X and Y are available in both periods, the type- z proposal is generated by outside competition in the second period. Its political benefits are given by the random variable Z , which may or may not be independent of X and Y .

2. The superior can influence the bureaucrat's activities by providing him with a budget and manipulating this as an incentive scheme that may depend on which program (type x , y , or z) will be realized, the corresponding level of political benefits (X , Y , or Z), and the period (1 or 2) when the final choice is made by the superior. This budget function is known to the bureaucrat.

One would expect the budget function to be designed to counterbalance the bureaucrat's mission bias and to reflect the superior's utility function (see Properties 2.7 and 3.1). In that case, the bureaucrat would have an incentive to act more in accord with the superior's wishes.

Under a *winner-take-all* incentive policy, the bureau receives no budget if an outside proposal of type z is accepted, whereas under an *implementation budget* scheme, the bureau receives an appropriation no matter which proposal is accepted.

3. The bureaucrat is assumed to be an expected-utility maximizer. The only variables under his control are e_x and e_y , both nonnegative and summing to one. His utility function is of the form $u(\text{program}, \text{budget}, \text{period})$, where (1) program = x , y , or z (depending on which proposal is accepted by the superior), and

$$u(y, \cdot, \cdot) \geq u(x, \cdot, \cdot) \geq u(z, \cdot, \cdot)$$

i.e., the bureaucrat prefers y to x , and x to z , other things equal; (2) $u_2 > 0$, i.e., the bureaucrat prefers higher budgets, other things equal; and (3) period = 1 or 2 (depending on when the final choice is made by the superior), and $u(\cdot, \cdot, 1) > u(\cdot, \cdot, 2)$, i.e., the bureaucrat prefers period 1 to period 2, other things equal.

4. In addition to determining the budget scheme, it is the superior's prerogative to decide which program will be realized, and when. The superior is also an expected-utility maximizer, and her utility function is of the form $v(\text{benefits}, \text{period})$ where (1) $v_1 > 0$, i.e., the superior prefers higher levels of political benefits (X , Y , or Z), other things equal; and (2) period = 1 or 2, and $v(\cdot, 1) > v(\cdot, 2)$, i.e., the superior prefers period 1 to period 2, other things equal. For convenience we have assumed that the budget is not an argument of her utility function.

5. We impose the following important restrictions: (1) the superior rejects a first-period proposal only if she expects to improve on it by waiting; thus, she lacks the power to manipulate the bureaucrat by threatening decisions that would be to her own disadvantage to carry out; and (2) the bureaucrat in turn maximizes his

expected utility without trying to influence the superior's acceptance rule or incentive scheme by using his control over e_x and e_y as a weapon.

6. It is worth noting that the assumptions made so far do *not* guarantee that the superior's optimal strategy is "classical," i.e., to accept the better of X and Y in the first period if $\max(X, Y)$ is sufficiently large and otherwise postpone the decision to the second period and accept the best of X , Y , and Z . This *will* be the case, however, if we assume that (1) Z is independent of X and Y ; and (2) $v(\text{benefits}, 2) = \delta_s \cdot v(\text{benefits}, 1) \geq 0$ for some implicit discount rate $\delta_s \in [0, 1]$. Under those assumptions, the superior will accept the better realized value of X and Y , say m , in the first period if and only if

$$\begin{aligned} v(m, 1) &\geq E\{v[\max(m, Z), 2]\} \\ &= \delta_s E\{v[\max(m, Z), 1]\} \end{aligned}$$

which is equivalent to $m \geq a$ for some constant a , the *acceptance level*. If $m < a$, then it would be better for the superior to wait until the second period. Assumption (1) will be relaxed in section 11.

7. Let S_x denote the event that the type- x proposal is accepted in either the first or the second period, S_{x1} the event that the type- x proposal is accepted in the first period, and similarly for S_{x2} , S_y , S_{y1} , S_{y2} , and $S_z = S_{z2}$. Then

$$\begin{aligned} S_{x1} &= \{X \geq Y, X \geq a\} \\ S_{x2} &= \{X \geq Y, X < a, X \geq Z\} \\ S_x &= S_{x1} \cup S_{x2} \\ S_{y1} &= \{Y > X, Y \geq a\} \\ S_{y2} &= \{Y > X, Y < a, Y \geq Z\} \\ S_y &= S_{y1} \cup S_{y2} \\ S_z &= S_{z2} = \{\max(X, Y) < \min(a, Z)\} \end{aligned}$$

To analyze how the probabilities of these events depend on e_x and e_y , the following observation is helpful: because e_x cannot take on two different values at the same period, the assumption that $X(e_x = e_x^t)$ is

stochastically larger than $X(e_x = e_x')$ whenever $e_x^t > e_x'$ is equivalent to the seemingly stronger assumption that $X(e_x = e_x^t) \geq X(e_x = e_x')$, and similarly for Y with respect to e_y .

It follows that if $e_x' + e_y' = e_x^t + e_y^t = 1$, $e_x^t > e_x'$, and $e_y^t < e_y'$, then

$$\begin{aligned} S_x(e_x = e_x', e_y = e_y') &\subseteq S_x(e_x = e_x^t, e_y = e_y^t) \\ S_y(e_x = e_x', e_y = e_y') &\supseteq S_y(e_x = e_x^t, e_y = e_y^t) \\ S_{x1}(e_x = e_x', e_y = e_y') &\subseteq S_{x1}(e_x = e_x^t, e_y = e_y^t) \\ S_{y1}(e_x = e_x', e_y = e_y') &\supseteq S_{y1}(e_x = e_x^t, e_y = e_y^t) \end{aligned}$$

Therefore, if $e_x + e_y = 1$,

$$\frac{dP(S_x)}{de_y} \text{ and } \frac{dP(S_{x1})}{de_y}$$

are both negative, and since

$$\begin{aligned} \frac{dP(S_x)}{de_y} &= \frac{dP(S_{x1})}{de_y} + \frac{dP(S_{x2})}{de_y} \\ \text{so } \frac{dP(S_{x2})}{de_y} &< \frac{dP(S_{x1})}{de_y} \end{aligned}$$

Similarly,

$$\frac{dP(S_y)}{de_y} \text{ and } \frac{dP(S_{y1})}{de_y}$$

are both positive, and

$$\frac{dP(S_{y2})}{de_y} > \frac{dP(S_{y1})}{de_y}$$

8. If the budget function is such that $\max\{\text{budget} : \text{project} = z\} \leq \min\{\text{budget} : \text{project} = x \text{ or } y\}$, or any other condition guaranteeing that the bureaucrat always prefers his own proposals x and y to the outside competition of type z , and $\text{budget}(\text{period} = 1) \geq \text{budget}(\text{period} = 2)$, other things equal, or any other condition guaranteeing that the bureaucrat's time preference cannot be reversed by the budget, then (1) *a higher acceptance level makes the bureaucrat worse off*. This follows from the fact that if $a^t > a'$, then $S_{x1}(a = a^t) \subseteq S_{x1}(a = a')$, $S_x(a = a^t) \subseteq$

$S_x(a = a'), S_{y1}(a = a') \subseteq S_{y1}(a = a'), S_y(a = a') \subseteq S_y(a = a'),$ and $S_z(a = a') \supseteq S_z(a = a')$; and (2) a stochastically larger Z makes the bureaucrat worse off. This follows by a very similar argument. Note that if Z becomes stochastically larger, the acceptance level a will increase as well.

9. A completely mission-oriented bureaucrat's utility function does not depend on the budget. Therefore, the above conclusions 8.1 and 8.2 apply.

Under a winner-take-all incentive policy, budget = 0 whenever project = z , so the first condition of section 8 is automatically met. Therefore, if the other condition is also satisfied, those two conclusions apply to this case as well.

10. It is obvious that the superior always prefers a stochastically larger distribution of $\max(X, Y)$. If the bureaucrat is purely budget oriented (i.e., his utility does not depend on the program type), the budget is proportional to the political benefits, and his utility function is such that it implicitly defines an optimal acceptance level a^1 from his perspective (analogous to the superior's acceptance level a ; see section 6), then the bureaucrat also prefers a stochastically larger distribution of $\max(X, Y)$, provided that $a^1 \leq a$, i.e., he is less patient than the superior. To see this, note that in this situation both the budget and the period in which the final choice is made can only change to the bureaucrat's advantage. If, however, $a^1 > a$, it is conceivable (though not necessary) that the superior is more likely to accept proposals in the first period that do not meet the bureaucrat's own standard.

11. Given any two random variables U and V , a correlation-decreasing transformation (*cdt*) changes their joint distribution so that $p(U > s, V > t)$ is decreased for all s, t , without affecting the

marginal distributions of U and V (Epstein and Tanney 1980).

1. Relaxing the assumption that X and Y are independent (given e_x and e_y), we will analyze the effect of a *cdt*. Given e_x and e_y , a *cdt* on the joint distribution of X and Y leads to a stochastically larger distribution of $M = \max(X, Y)$, which is in the superior's interest. If the bureaucrat is purely budget oriented (i.e., his utility is independent of the program type), and less patient than the superior, this helps him as well. If the bureaucrat has some mission bias, the effect of such a *cdt* on his welfare is indeterminate, since the probability that Y exceeds X can either increase or decrease.

2. Relaxing the assumption that $M = \max(X, Y)$ and Z are independent, consider a *cdt* on their joint distribution. As illustrated in the text (Property 2.6.a), this is not always in the superior's interest. Moreover, even if the superior's optimal decision rule is classical (that is, M is accepted in the first period if and only if $M \geq a$), then a *cdt* on M and Z is not necessarily good for the superior under a complete-equilibrium analysis. To see this, note that since she cannot precommit to a suboptimal acceptance level (i.e., a must be a best response), she will in general change her cutoff if the joint distribution of M and Z changes. In turn the new a may cause the bureaucrat to alter his search so much to the superior's disadvantage that the net effect on her welfare is negative. However, under the partial-equilibrium analysis of Property 2.6.b, the superior prefers a *cdt* since it has the effect of stochastically increasing $\max(M, Z) = \max(X, Y, Z)$ on the set $\{M < a\}$.

12. Suppose the bureaucrat's utility function is of the simple form,

$$u(\text{program}, \text{budget}, 1) = \begin{cases} 1 + \text{budget} & \text{if program} = y \\ c + \text{budget} & \text{if program} = x \\ \text{budget} & \text{if program} = z \end{cases}$$

$$u(\text{program}, \text{budget}, 2) = \delta_b u(\text{program}, \text{budget}, 1)$$

where c and δ_b are both in the interval $[0, 1]$. Assume that the expected utility $E(u)$ is a concave function of e_y with an interior maximum satisfying $dE(u)/de_y = 0$ where $e_x + e_y \equiv 1$. By the results in section 7,

$$\frac{d^2 E(u)}{dc de_y} = \frac{dP(S_{x1})}{de_y} + \delta_b \frac{dP(S_{x2})}{de_y}$$

is negative. Therefore, as the relative utility of c of type x is increased, the bureaucrat's optimal e_y decreases, so e_x increases. This determinate result holds only for this special utility function. Even here, however, the remaining comparative statics on e_x and e_y are indeterminate. For example, analyzing the bureaucrat's response to a changed acceptance level a we have

$$\frac{d^2 Eu}{da de_y} = \left[\frac{d^2 P(S_{x1})}{da de_y} + \delta_b \frac{d^2 P(S_{x2})}{da de_y} \right] c + \frac{d^2 P(S_{y1})}{da de_y} + \delta_b \frac{d^2 P(S_{y2})}{da de_y}$$

since $u_y = 1$ and $u_z = 0$. Note that many of the components of the above equation cannot be signed. For example,

$$\begin{aligned} \frac{d^2 P(S_{x1})}{da de_y} &= \frac{d}{da} \left(\frac{dP(S_{x1})}{de_y} \right) \\ &= \frac{d}{da} \left[P(X > Y | X \geq a) \frac{dP(X \geq a)}{de_y} \right. \\ &\quad \left. + \frac{dP(X > Y | X \geq a)}{de_y} P(X \geq a) \right] \end{aligned}$$

$$\begin{aligned} &= P(X > Y | X \geq a) \frac{d^2 P(X \geq a)}{da de_y} \\ &\quad + \frac{dP(X > Y | X \geq a)}{da} \frac{dP(X \geq a)}{de_y} \\ &\quad + \frac{d^2 P(X > Y | X \geq a)}{da de_y} P(X \geq a) \\ &\quad + \frac{dP(X > Y | X \geq a)}{de_y} \frac{dP(X \geq a)}{da} \end{aligned}$$

Without further assumptions, the sign of this component cannot be determined, so neither can the overall expression for $d^2 Eu/da de_y$. Since changes in either Z or δ_s affect a , they are likewise indeterminate. Finally, grinding through similar comparative-static equations for δ_b and Y reveal that they cannot be signed either.

13. For any given acceptance rule, the superior prefers stochastically larger distributions of X , Y , and Z (obtained by adding nonnegative random variables Δ_x , Δ_y , and Δ_z , respectively). If the superior's utility function is linear or convex (risk-neutral or risk-seeking) in the benefits, she also prefers riskier distributions (obtained by adding random-noise terms ϵ_x , ϵ_y , and ϵ_z , respectively, satisfying $E(\epsilon_i | X, Y, Z) = 0$ for $i = x, y, z$) (Rothschild and Stiglitz 1971). However, paralleling the effect of reduced correlation in section 11.2 above, the effect of riskier Z on the superior's welfare is indeterminate once one allows a and e_y to vary.

Notes

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1. Consider, for example, Polsby's finding that "The civilian control of atomic energy . . . started with a scientific laboratory threatened with extinction. As the laboratory searched for ways to remain useful, it began also to seek to innovate and to invent new policies. It is not uncommon to hear of such a sequence of events" (1984, 164).

2. A recent CBO study (Congressional Budget Office 1986) on infrastructure management observed that "federal programs do not constantly encourage broad searches for ways to improve the productivity of infrastructure. Most . . . are managed not to support and promote broad policy goals but instead to provide capital for predetermined types of projects" (p. 15). For more on domestic policy formation see Salamon 1981, 190.

3. These political benefits need not correspond to those of a conventional cost-benefit analysis: the politician's goals need not be normatively appropriate. Indeed, many bureaucrats probably believe that without technical guidance, politicians would often make inappropriate decisions based on narrow electoral criteria. Such officials may think that by manipulating the menu of alternatives they are offering a form of guidance.

4. This condition implies that average political benefits also increase with search.

5. One may wish to consider the jurisdictional structure as an object of long-run strategic interest (Hammond 1986). It is fixed for the short run modeled here. In any case, recall that giving one agency a temporary monopoly is not necessarily suboptimal for the superior, given the opportunity costs of a simultaneous generation of alternatives. Moreover, we will see that the model yields simultaneous review of alternatives as a special case.

6. Rejecting poor proposals and accepting good ones, the "classical" stopping rule (Kohn and Shavell 1974), is optimal if Z is independent of X and Y and the future is discounted. We generally assume independence; correlated search is covered in Properties 2.5 and 2.6.

7. Of course, her decision depends on what the realized value of $\max(X, Y)$ is. It is the conditional decision rule that can be formulated without observing the bureau's proposals.

8. This preference ordering approximates the air force's attitudes toward intermediate range ballistic missiles (IRBMs) in the 1950s. Initially unenthusiastic toward missiles and strongly oriented toward bombers, the dominant coalition in the air force became much more interested in IRBMs when it became evident it could lose out to the army's Jupiter missile (Armstrong 1969, 55-56).

9. Suppose, for example, the superior was a political appointee, an assistant secretary who wanted to make her mark in a hurry. (For data on the length of tenure of political appointees in the Kennedy, Johnson, and Nixon administrations, see Heclo 1977, 105. For tenure data on senior career

bureaucrats, see McGregor 1974, 24).

10. Because the boss takes into account both benefits and costs, the two conditions are intertwined. For example, the more impatient she is, the better the outside competition can be and still be "sufficiently" poor so that the agenda is completely rigged.

11. Of course if the outside competition is sufficiently strong and the superior sufficiently patient, the bureaucrat's search will be irrelevant to the boss because she will never accept his proposals.

12. These results imply another sufficient condition for complete agenda manipulation: if u_x is "sufficiently" small and the chance of getting y approved is not too small, the agenda will be totally rigged.

13. Similarly, it is easy to show that changes in the quality of the agency's alternatives, in the bureaucrat's discount parameter, or in the superior's acceptance level all have indeterminate effects on search (Appendix, sec. 12).

14. The winner-take-all scheme would not necessarily be undesirable if the supply of outside alternatives depended on the prospect of receiving a budget for the program, as was partly the case for the army's interest in missiles (Kanter 1979, 112). In this model, however, Z is exogenously fixed.

15. Let $m_1 = 1$, $m_2 = 2$, $z_1 = 2$, and $z_2 = 4$; their unconditional probabilities equal $1/2$. The bureaucrat's discount is .2; the superior's, .4. If M and Z are perfectly positively correlated the superior will accept either m_1 or m_2 ; if they are independent she will reject m_1 . She prefers the latter situation; the bureaucrat, the former.

16. Though the purely budget-oriented administrator was a special case, almost all of Model 2's results carry over to the more realistic setting of an administrator interested in both missions and funding. (One must assume, of course, that the two types of agents face the same budgetary policy.) The only results that do not generalize are the parts of Properties 2.4 and 2.5 describing the responses of an impatient bureaucrat.

17. This scheme is not the only way to induce unbiased search. Assuming that the costs of implementing the program would still be covered, the superior could, in the first period, subtract a fixed amount of c_1 if she chose y . When combined with appropriate second-period adjustments, any weighted average of penalty for y of αc_1 and bonus for x of $(1 - \alpha)c_1$ would have the desired effect of neutralizing the bureaucrat's mission orientation. There are infinitely many such combinations. A choice between bonuses and penalties would probably matter to the superior. If she is operating under an overall budget constraint, a bonus for choosing x would require transferring funds from another bureau to this agency; a penalty for choosing y would reduce the program size of this agency. However, the comparative-static results of Properties 3.2 and 3.3 are unaffected by this choice between carrots

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and sticks.

18. In a model of advice giving, Calvert has discovered the intriguing result that superiors may prefer more biased subordinates (1985a). However, because he has modeled asymmetric information quite differently from the approach taken here, his finding and ours are not comparable.

19. The one exception is a change in the bureaucrat's discount factor: inspecting the second-period terms in the budget policy of Property 3.1 reveals that a change in δ_i can make the superior either better or worse off.

20. Personnel shifts are another matter: as we have seen, these affect a superior's welfare because it costs less to obtain unbiased search from less biased bureaucrats.

21. Note that most of the comparative statics on the bureau's allocation of search effort are still indeterminate: for instance, improved Z could make it focus more on x or y . Therefore, those interested in the details of policy formation might still find the agency's behavior interesting. In either case, however, the new search would be unbiased.

22. Two other assumptions of perfect information should also be subjected to sensitivity analysis: that the bureaucrat knows the superior's acceptance level, and that both know the distribution of second-round possibilities. It is worth noting that if the superior has an unbiased but noisy perception of Z , her acceptance level is higher than it would be if she knows Z (Kohn and Shavell 1974, 115). The reason parallels her preference for more objectively innovative search: because she can always fall back on the agency's better alternative, her increased perception of very bad possibilities is irrelevant, but her belief in very good possibilities raises her aspirations. (In this case, interestingly, the political superior need not have optimistic biases in order to have unrealistically high policy aspirations.) Yet, although pure uncertainty imparts a bias to behavior, as perceptions become less noisy the acceptance level converges to the full-information a^* ; thus here small amounts of uncertainty do indeed have only small effects on actions.

23. For an analysis of the control problems created by the multiplicity of political principals and the ensuing multiple-winning coalitions, see Hill 1985 and Hammond, Hill, and Miller 1986. For a more wide-ranging analysis of the problems politicians have in controlling bureaucratic agents, see Moe 1984.

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ASYMMETRIC INFORMATION AND THE COHERENCE OF LEGISLATION

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*L*egislators' beliefs, preferences, and intentions are communicated in committees and legislatures through debates, the proposal of bills and amendments, and the recording of votes. Because such information is typically distributed asymmetrically within any group of decision makers, legislators have incentives to reveal or conceal private information strategically and thus manipulate the collective decision-making process in their favor. In consequence, any committee decision may in the end reflect only the interests of a minority. We address a problem of sharing information through debate in an endogenous, agenda-setting, collective-choice process. The model is game theoretic and we find in the equilibrium to the game that at least some legislators have incentives to conceal private information. Consequently, the final committee decision can be "incoherent" by failing to reflect the preferences of all committee members fully. Additionally, we characterize the subset of legislators with any incentive to conceal data.

In sum, reasoned deliberation is important in decision making. Lawmaking consists of more than log rolling, compromises, or power plays. General debate enables members to gain a better understanding of complex issues, and it may influence the collective decisions of the House. The dilemma members often face . . . "is to know what is right, and to make the right decisions" based upon skimpy, incomplete, or unavailable information.

Oleszek, *Congressional Procedures
and the Policy Process*

The study of rhetoric is possibly the earliest form of political analysis. Within this tradition scholars have, for pedagogical purposes, categorized rhetorical devices and stances and judged the persuasiveness of appeals. From Aristotle onwards, a frequent proposition in the commentary on rhetoric is that rhetorical stances are and ought to be conditional on the particular audience and situation. Because there are, beforehand, infinitely many audiences and

situations, the mere compilation of appropriate stances on a case-by-case basis is not useful for generalization. To approach the proposition about audiences and situations systematically, it is necessary to explore more abstract models of rhetorical interaction, which, one hopes, will ultimately be testable. What follows is a step in this direction.

Legislation is rarely, if ever, an end in itself. Instead, lawmakers are concerned about the consequences of legislation. Typically, however, the connection between legislation and consequence is known only with uncertainty. And while it may be that legislators' goals and preferences as to outcomes are common knowledge, their individual information and beliefs about possible consequences of policies for these goals are at least partially, and often entirely, private. As a result, their induced preferences as to legislation are private and variable because they blend publicly known goals

with their private judgements. Thus, for example, when considering a minimum-wage bill, the individual legislators' ideals about income distribution are likely to be well known, but their information regarding technical data (e.g., the connection between minimum-wage levels and unemployment), their beliefs about its accuracy, and so on, are likely to be unknown.

Debate is one mechanism whereby such private information can be disseminated within a group of decision makers. With this mechanism each member faces a question of rhetorical strategy: To what degree should one reveal one's private information in debate so that the eventual decision of the group can be based on the information thus pooled? It is by no means clear *a priori* what the member will do.¹ On the one hand, each individual has an interest in sharing information in order to estimate better which policy is most in his or her particular interest. On the other hand, if information is both asymmetrically distributed and, when kept private, potentially advantageous in the decision-making process, there is an incentive to conceal data strategically. If the group is collegial in tone (as, for example, a cabinet) then one might expect sharing. In adversarial groups, however, with parties having widely divergent interests in policy, concealment seems more likely.

In any event, within the context of legislative decision making, understanding the circumstances in which individuals have incentives to conceal germane information is of considerable interest. For when such data are not shared, the legislative decision may turn out in the end to reflect only the interests of some minority.

In the next two sections we offer a simple model of committee decision making in which committee members can transmit private information, either directly, through debate, or indirectly, through offering motions. During an initial debate stage, individuals may reveal all, some, or

none of their relevant data. After debate, they simultaneously offer one policy proposal each. These proposals constitute the agenda. Given the agenda, a round-robin majority voting process determines the group choice. Finally, the model is set up to avoid voting cycles. Because we are concerned with the strategic management of information, we have deliberately eliminated other problems of strategy by standardizing and trivializing agenda formation and voting. A complete description of the process of decision making would, of course, require the analysis of strategic choice at every stage of the process, but we limit ourselves here to the initial task of studying the revelation of information.

In principle, individuals' information can vary across stages. Thus, if not all information is pooled at the debate stage, some legislators might, at the voting stage, wish to alter their proposals. But because our model is a single reading process, this option is not available. Evidently, if all information is fully shared in debate, no one can have reason to change his or her proposal and the agenda will be invariant over repeated readings. With a slight deviation from customary usage, we call such an agenda "coherent."² Our principle finding is that within this institutional setting, coherence in terms of information sharing is not guaranteed. Beyond that, we show that the committee divides into two disjoint and well-defined groups of legislators. All the legislators in one group, containing the median voter (measured with respect to preferences over the consequences of legislation), choose to reveal all their information. Each legislator in the remaining group has an incentive to conceal some or all of his or her private information. And if we pair any legislator other than the median from the first group with any legislator from the second group, they will be diametrically opposed with respect to the issue under consideration by the committee.

We describe the model and results informally, then discuss further implications and draw conclusions. We develop the model formally and offer proofs for the results in the extensive, entirely self-contained, Appendix.

Model and Results: Informal Development

The Model

The canonical problem the model addresses is easily stated. Consider a group of individuals (a committee) required to come to some collective decision, the consequences of which are known only with uncertainty. Suppose the individuals have diverse preferences over consequences and possess private information as to how different decisions will affect the outcome. Under what circumstances can we expect full revelation of private information, so that collective choice can reflect all the data relevant to the decision? The answer depends on, among other things, such factors as the nature of the decision and the degree of uncertainty, on the particular collective decision-making scheme used, on the distribution of preferences over consequences, and so on.

The structure here is straightforward. We consider a committee consisting of n individuals, where n is an odd number greater than or equal to three. Each member of the committee has quadratic preferences over a one-dimensional space of consequences. We suppose the preferences to differ only in ideal points so that, in particular, legislators' attitudes to risk are the same. Quadratic preferences are symmetric and single peaked and exhibit a high degree of risk aversion. Legislator i 's ideal point is labelled z_i . For convenience of argument, legislators are themselves named so that $z_1 < z_2 < \dots < z_n$; thus, the lower the index i , the further left is z_i

on the single dimension. This committee has to select a policy. Like consequences, policies are taken to be one-dimensional objects and are called bills. We impose the simplest relationship connecting bills to outcomes, or consequences: bills equal outcomes up to the realization of some random variable. Thus, letting b denote a particular bill, and β the random variable, the consequence of implementing policy b is itself a random variable given by $y = b + \beta$. So one can think of a bill producing outcomes probabilistically, an interpretation that reflects genuine disputes in the real world about the anticipated consequences of legislation.

An example of the sort of issue described here is the mandatory fluoridation of public water supplies. In this case, b denotes the level of fluoride that agencies are required to introduce into the water supplies—determined by legislation—while β describes the level introduced by nature. It is the total level of fluoride in water, the consequence y , that legislators care about; but they can only determine part of this beforehand and their expectations regarding the distribution of nature's contribution, β , will affect their preferences as to policies, b .

For convenience in abstract argument and model building, we assume some statistical regularities about β . Thus, the distribution of β is normal with an unknown mean, β^* , but with a known variance of one; the covariance between b and β is identically zero; all legislators share the same prior beliefs regarding β^* , and these are described by the standardized normal distribution. The assumption that β is distributed normally is largely a technical convenience. The assumptions about the extent of legislators' knowledge concerning this distribution, however, are substantive. That there is ignorance regarding the mean of the distribution is reasonable; that legislators know surely what the variance is, is not so reasonable. We justify the assumption by observing

that with only one source of uncertainty, we generate a model sufficiently rich to focus on the strategic issue of concern here. Furthermore, multiple sources of uncertainty can be expected to give added weight to legislators' incentives for concealing private information. Thus, any results obtained regarding the incentives to conceal are stronger with only a singleton source.

Before the committee meets, each member acquires some information about β^* in the form of an independent random sample from the distribution of β . This sampling is what produces members' private information. We describe legislator i 's private information as an expectation of the unknown mean, β^* , based on the sample i observes; denote this expectation here as $E_i[\beta^*]$. So, before observing his or her random sample, a legislator initially believes that nature, on average, adds no fluoride to water supplies; but, having observed the sample, the legislator comes to expect nature to contribute a level of $E_i[\beta^*]$ on average.³

Because legislator i 's initial belief regarding the average value of the random variable is zero, i 's induced ideal point over bills is given by z_i prior to observing the random sample. After observing the sample, however, i 's induced ideal point is revised consonant with i 's change in belief and is now given by $z_i - E_i[\beta^*]$. In terms of our running example, i 's most preferred level of legislated fluoridation is given by i 's most preferred level of fluoridation in the absence of natural intervention minus the expected level of nature's contribution. Once legislators have received their information, there is clearly no reason to presume that the distribution of induced ideal points over the policy dimension is the same as that over outcomes. Consequently we can have, say, $z_i - E_i[\beta^*]$ less than $z_j - E_j[\beta^*]$ even though z_j is less than z_i .

The common prior over β^* , the sample size, and the list of ideal points over out-

comes, $\{z_i\}$, are all common knowledge.⁴ Only individual-sample means, and thus expectations $\{E_i[\beta^*]\}$, are private data.

The institutional details of decision making have already been sketched. The process has three stages. During the first stage, legislators engage in debate. After debate, each committee member (simultaneously) makes a proposal, a bill b_i , generating an agenda $b = (b_1, \dots, b_n)$. Finally, majority voting over all pairs of proposals determines the committee decision. Given our earlier assumptions, there will always be a Condorcet winner at this stage if the set of proposals is not empty: that is, despite variations in the relative orderings of legislators along the policy dimension, single-peaked preferences over bills are preserved. And because there will always be some proposal offered, the committee's final decision is well defined. The agenda-setting stage and the voting stage are fairly standard; the debate stage, though, is novel.

"Debate" is modeled as a strategic signalling game (see, e.g., Banks and Sobel 1987; Crawford and Sobel 1982; Gal-Or 1985, 1986). The idea underlying the model is that speeches in committee reveal information as to where legislators stand on some issue and why they hold their positions. Such information is not going to persuade people to alter their evaluations of consequences; it can, however, affect their perceptions of what bill or policy will best promote particular outcomes. In the present context, committee members' beliefs regarding the mean of β determines their most preferred policy. Going into committee, these beliefs are based largely on private data. Sharing information in committee can lead members to revise their expectations and thus affect their induced preferences over legislation. Recognizing this makes information sharing a strategic consideration for individual legislators.

There are a variety of ways to model debate as a signalling game. Here, we

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adopt one of the simplest. Legislators simultaneously reveal unbiased estimates of their private data to the committee as a whole. The strategic character of information sharing is introduced by allowing the estimate to be noisy. If a legislator's message is noise free, that legislator fully reveals his or her private data. If the message is completely noisy, he or she reveals nothing. Intermediate levels of noise offer partial information. In this last case, because the selection of a noise level is a strategic decision, other participants may make inferences about the legislator's information in addition to that contained in the message per se, that is, saying nothing may sometimes reveal much.

Although it has precedent in other models of signaling (Gal-Or 1985, 1986), restricting strategies to unbiased estimates of individuals' true data is a strong assumption (Crawford and Sobel 1982). Emphasizing favorable information and playing down unfavorable information is commonplace in political argument, even if straightforward lying is not.⁵ Ultimately, this assumption needs to be relaxed. But we will use it here in order to concentrate on the simplest problem of rhetorical strategy, namely, the amount of information to impart, without complicating the analysis with considerations of the quality of information transmitted.

The remaining substantive restriction imposed is the strategic simultaneity of messages: this rules out the give-and-take of argument in the normal usage. In the present context, however, dialectical changes of position can have no role, so the assumption seems a legitimate working principle.

The institutional structure described induces a three-stage game with the legislators as players. For each legislator, a strategy consists of a signalling rule for the debate stage of the game, a proposal rule for the agenda-setting stage, and a voting rule for the final stage. Each of these rules specifies what the legislator

should do at the relevant stage, *given* his or her information at that stage and the strategies of others. For example, in the debate stage, a legislator's actual choice of noise depends on, among other things, his or her private information; while the legislator's actual choice of proposal, b_i , is conditional not only on this data but also on the signals received in debate from other legislators.

As noted, the agenda-setting stage results in an agenda, b , which is a list of proposals by members, $b = (b_1, \dots, b_n)$. In order to interpret the agenda b , consider the situation at the end of the third stage, when the agenda has been voted on. Each legislator, having observed others' proposals and votes, can then look back and formulate, afterwards, the proposal that he or she would now choose to make. Call this *ex post* proposal b_i^+ and let b^+ be the list of these preferred final proposals. Clearly, b^+ need not be the same as b because at the end one or more legislators might wish they had offered different proposals. Define an agenda to be *coherent*, however, if and only if $b^+ = b$. In other words, an agenda is coherent if and only if, once revealed to the committee and voted on, no legislator would wish to change his or her proposal from that offered (in ignorance of others' proposals) during stage two of the process. This definition of coherence means that all the information potentially available to the committee—as a whole and at the time of the legislative choice—has in fact been incorporated into bills and votes.⁶ The rationale for the definition is that when the committee decision reflects all information, the bill enacted is as consistent as possible with the values of the members. When not all the information is pooled, however, the enactment may be at odds with the values of some, even many, members of the group. But notice that coherence does not *require* all private information in fact to be pooled: only that the outcome from a coherent agenda be *as*

if such pooling had occurred.

Each legislator i chooses a strategy to maximize his or her expected pay-off from the eventual collective decision, that is, the bill passed at the voting stage of the game. Only pure strategies are considered and we look for perfect Bayesian equilibria (PBEs). This solution concept rules out cooperative behavior among legislators (our model is "adversarial" rather than "collegial") and ensures that strategies selected prior to any given stage of the game remain optimal once that stage is reached (Selten 1975). A formal description of the game and definition of the equilibrium is given in sections 1-8 of the Appendix.

Before going on to present the results, it is worth reemphasizing that the model is explicitly set up to isolate the strategic aspects of information transmission. For example, although agenda setting is endogenous, the way in which proposals are placed and the structure of the subsequent voting mechanism effectively give legislators' dominant proposal strategies. Were this not the case, any outcome of the decision-making process other than the pooled-information result (i.e., the median voters' ideal point relative to the pooled information) could not then be attributed surely to the strategic use of information. Moreover, with a single-dimensional issue space, simple-majority voting over all pairs of alternatives in the agenda, and (very) risk-averse preferences, the model is intrinsically biased toward legislators choosing to reveal all their private data in debate. Any results asserting that legislators may yet choose to conceal private information are thereby relatively strong.

Results

Consider the situation before any legislator has observed his or her private information but in which all legislators have the set of commonly known data, that is,

what the sample size will be, what everyone's priors over β are, and so forth. Given this situation, define $E[\mu]$ to be the expected location of the median proposal at the agenda-setting stage of the decision-making process. Let i^* be the legislator with the median ideal point in consequences, $i^* = (n + 1)/2$. Then the main results may be summarized as follows:

PROPOSITION. *There exists an essentially unique PBE such that, relative to their information at each stage,*⁷

1. *all legislators vote sincerely in the voting stage of the game*
2. *all legislators propose their expected induced ideal points in the agenda-setting stage of the game*
3. *legislator i^* always volunteers all his or her private information in debate*
4. *If $E[\mu] = z_{i^*}$, then all legislators volunteer all their private information in debate*
5. *If $E[\mu] < (>) z_{i^*}$, then any legislator $i > (<) i^*$ will volunteer all his or her private data in debate*
6. *If $E[\mu] < (>) z_{i^*}$, then any legislator $i < (>) i^*$ has an incentive to conceal part or all of his or her private data in debate. Partial revelation of data is not fully revealing, and what the legislators choose to do (reveal none, some, or all of their data) depends on the particular distribution of preferences and parameters of the situation*
7. *Suppose $E[\mu] < z_{i^*}$; then in the equilibrium, $i < j$ implies i will offer no more information in debate than j , all $i, j < i^*$, mutatis mutandis, the analogous result holds when $E[\mu] > z_{i^*}$*

It follows from Claims 5-7 of the proposition that coherent agendas cannot be guaranteed. Claims 5 and 6 also say that all of the legislators with an incentive to conceal their private information lie on

exactly *one* side of the median in outcome space, i^* . In other words, if any two legislators—say, j and k —fail to share all of their private data with the committee at the debate stage, then either (1) $z_j < z_{i^*}$ and $z_k < z_{i^*}$, or (2) $z_j > z_{i^*}$ and $z_k > z_{i^*}$ (which of these two possibilities obtain depends on, among other things, the distribution of ideal points $\{z_i\}$). And Claim 7 of the proposition asserts that more extreme legislators, in terms of their positions with respect to consequences, have a relatively greater incentive than others to conceal private information when anyone on their side of the issue has any such incentive.

Claim 1 of the result follows from the single-reading structure of decision making. Sophisticated voting is by definition an action taken at an earlier stage to influence an outcome at a later stage. Because the sequence of debate, agenda setting, and voting occurs only once, there is nothing to be gained by voting insincerely in the final stage. The intuition behind Claim 2 is much the same. Single-peakedness of individuals' preferences over bills is preserved in the model and so, given the equilibrium voting strategies specified in Claim 1, legislators can do no better in the agenda-setting stage than offer their expected induced ideal point for consideration (see section 9 of the Appendix).

Although, at the agenda-setting stage, legislators propose their expected induced ideal points, it may turn out afterwards that once the agenda is revealed, each individual will update his or her beliefs regarding an ideal point. This is because each member knows that in equilibrium, proposals reveal currently perceived ideal points. These are certainly invertible so that all private data is effectively public once the voting stage is reached. However, recognizing this fact does not alter the *ex ante* optimality of choosing proposals according both to one's private information and to the data actually

Figure 1. Ideal Points over Consequences, $y = b + b$

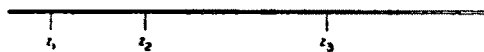
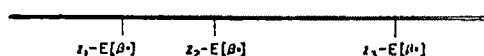


Figure 2. Induced Ideal Points over Bills, b , under Fully Pooled Information

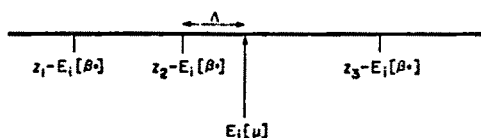


revealed in debate. Of course, if all information is revealed in debate, then no legislator would wish to alter a proposal afterwards.

Now notice that because individual proposal strategies are fully revealing, legislator $i^* = (n + 1)/2$ will surely be the median voter with respect to bills at the voting stage—with fully pooled data, legislators' *relative* positions over bills correspond exactly to those over outcomes (see Figures 1 and 2, in which legislators' common expectation regarding β under fully pooled information is denoted $E[\beta^*]$). Consequently, at the voting stage, the proposal most preferred by i^* will win. But it is *not* true that legislator i^* will necessarily make the winning proposal at the agenda-setting stage. It is precisely the possibility of being the proposer of the bill *de facto* most preferred by i^* that generates incentives to conceal information in debate.

Consider a single individual i at the start of the debate stage, prior to any opportunities for sharing information. For expositional purposes, suppose initially that there are three committee members. The legislator's induced ideal point at this stage is $z_i - E_i[\beta^*]$, as indicated earlier. His or her best estimate of the location of the other two legislators'

Figure 3. Legislator i 's Expectations at the Start of the Debate Stage



induced ideal points at the same stage is exactly as illustrated in Figure 1(b), but with the expectation $E[\beta^*]$ replaced by the legislator's own private—and almost surely distinct—expectation $E_i[\beta^*]$. However, even though all legislators are known to propose their induced ideal points at the agenda-setting stage, it is *not* the case that i 's expectation of the de facto median proposal, as evaluated at the start of the debate stage, is $z_2 - E_i[\beta^*]$, his or her current estimate of the location of legislator 2's induced ideal point. The reason is simple: because $E_i[\beta^*]$ is an estimate based on i 's private data and because the other legislators have formed similar estimates based on their own private data, there is a positive probability that legislators other than i are in fact located elsewhere. Hence, any of the three members of the committee has a positive probability of being the proposer of the de facto winning bill.

To estimate the location of the winning proposal, therefore, individual i must first calculate the probability of each of the six possible left-to-right orderings of the three committee members' induced ideal points and then, given each of these orderings, estimate the location of the relevant median's most preferred bill. Combining these data gives i 's expectation regarding the median proposal, $E_i[\mu]$. Evidently, this estimate, $E_i[\mu]$, must lie between i 's expectation of the extreme legislators' ideal points:

$$z_1 - E_i[\beta^*] < E_i[\mu] < z_n - E_i[\beta^*].$$

So, by virtue of the equilibrium second- and third-stage strategies discussed above, it follows that $E_i[\mu]$ must be i 's expectation of the de facto winning proposal. Figure 3 illustrates one possibility (see also Appendix, section 15, where an explicit example is worked through).

The choice of legislator i in the preceding discussion was arbitrary. But at the start of the debate stage, legislators are identical except for their ideal points in outcome space, $\{z_i\}$, and the specific estimates, $\{E_i[\beta^*]\}$, derived from their private data. Therefore, because any legislator i 's induced ideal point is given by $z_i - E_i[\beta^*]$, at the start of the debate stage all legislators will agree on their *relative* positions along the bill dimension. So if, in Figure 3, we first let $i = 1$, then let $i = 2$ and finally let $i = 3$, the picture we would draw in each case would differ *only* in the absolute location of the induced ideal points, and not in their relative locations. In particular, although individuals' predebate estimates of the median proposal, $\{E_i[\mu]\}$, need not coincide, they will all agree on the expected distance separating this estimate from the induced ideal point of the median legislator in consequences. For the example of three voters, this distance is denoted by Δ in Figure 3. In other words, the value of Δ is independent of anybody's private information, even though the individual estimates, $\{E_i[\mu]\}$, do depend on this information. An individual's estimate, $E_i[\mu]$, determines the location in the bill space for his or her calculation of Δ , though the calculation itself then depends only on matters of common knowledge. And this is true whatever the size of the committee.

Because Δ is independent of individuals' private data, we can set $E_i[\beta^*]$ equal to zero for any legislator i and calculate $E_i[\mu]$ on this basis—call it $E[\mu]$ —and obtain the identical value of Δ we would if we knew i 's actual expectation $E_i[\beta^*]$. Moreover, Δ is then given by the distance separating z_i^* and $E[\mu]$; it is this fact we exploit in stat-

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ing our proposition. If, in debate, all private data is fully revealed, then $E_i[\beta^*] = E[\beta^*]$ for all legislators i , and, therefore, Δ is surely equal to zero at the start of the agenda-setting stage. For our three-person example, this situation is depicted in Figure 2. More generally, the greater the amount of private data revealed in debate, the smaller (in absolute value) will be the size of Δ , and the less uncertainty there will be regarding the location of the median proposal (these facts are established in section 10 of the Appendix).

We are now in a position to elucidate Claims 3 through 6 of the proposition. Suppose, for the sake of this discussion, that there are three legislators and the situation is as described in Figure 3, so that everyone at the start of the debate stage estimates the median proposal to be to the right of legislator 2's induced ideal point. As we argued above, the more any private information becomes revealed to the committee as a whole, the closer the situation becomes to the case with fully pooled information (Figure 1b). That is, the value of Δ is closer to zero, and the uncertainty concerning the accuracy of any individual's estimate of the location of the median induced ideal point is smaller. Therefore, irrespective of what any other legislator chooses to do, both legislator 1 and legislator 2 can reduce Δ —so moving the expected winning proposal closer to their induced ideal points—and simultaneously lower the uncertainty regarding this outcome by each volunteering all of his or her private data in debate. By the assumption of risk aversion, both effects unambiguously improve these legislators' expected payoffs from the outcome of the decision-making procedure.

Because this result for legislators 1 and 2 depends only on the fact that Δ is positive and does not depend on what 3 chooses to do, it is easy to see that the argument extends to similarly placed legislators in larger committees. Hence,

Claims 3 through 5 follow (see section 11 of the Appendix).

Return now to the three-person example. Given Δ , legislators 1 and 2 have an unambiguously best strategy to pursue in debate. This is not true for legislator 3. On the one hand, revealing his or her private information in debate reduces the uncertainty surrounding the final outcome, which improves legislator 3's expected payoff. But on the other hand, revealing this data reduces the value of Δ , and so the location of the expected winning proposal moves further away from his or her most preferred alternative. Legislator 3 faces a trade-off and exactly what he or she chooses to do will depend on the details of the issue under consideration. In particular, exactly how much information 3 elects to impart in debate will depend on how much other legislators choose to share. In the three-person example we are discussing, 3 can deduce correctly that 1 and 2 will reveal all their private data. Therefore, 3 takes this into account and simply chooses that level of noise in his or her signal which maximizes the expected payoff from the decision-making process, namely, the level that balances 3's expected loss from reducing Δ with his or her expected gain from reducing the uncertainty surrounding the final outcome.

In committees larger than three, however, the strategic problem for legislators placed similarly to 3, relative to Δ , is more complex. The underlying trade-off is the same for each such voter and, given what everyone else does, the best strategy for any given legislator in this situation is determined by the maximizing problem described above. But there is a further difficulty because these legislators have distinct ideal points in outcome space. Because of this, for any arbitrary list of others' debate strategies, each individual may well have a different most preferred combination of Δ and the uncertainty surrounding the final outcome.

Consequently, we have to establish that there exists a set of signals at the debate stage—one signal for each of the legislators under consideration—that is mutually consistent in the sense that each legislator's signal maximizes his or her expected payoff, given the specified signals of everyone else. Technically, such a list is a Bayesian equilibrium to the debate stage of the decision-making game, and section 12 of the Appendix demonstrates that such an equilibrium exists. This establishes the first part of Claim 6.

The remaining part of Claim 6 says that giving some, but not complete, information is not fully revealing; that is, hearing a noisy message and knowing the level of noise constitute insufficient data for any legislator to infer exactly what the sender's private information must be. The reason for this relates to the fact, discussed above, that the distance Δ is independent of anyone's private information: therefore, since it is the value of Δ that matters for strategic purposes, any particular level of noise chosen in equilibrium by, say, legislator 3 is consistent with any specific observations of the random variable. Of course, the actual message received by the committee at large is informative. It is simply not fully revealing (unless there is no noise in the message at all).

The final part of the proposition is a straightforward consequence of legislators' ideal points being distributed along a single-dimensional outcome space. The further out an individual is located relative to the median, i^* , the greater the incentive to conceal information. Loosely speaking, this is because the expected utility loss associated with greater uncertainty as to the final outcome is relatively small. With a higher variance, there is a higher likelihood that the final outcome will be located closer to that individual's ideal point. But it is worth remarking that without the assumption of identical sample sizes, m , this last result does not neces-

sarily hold (although the other claims do).⁸

Which side of legislator i^* 's induced ideal point individuals at the start of the debate stage expect the de facto median proposal to lie on depends on the dispersion of the ideal points $\{z_i\}$ relative to the variances of the distribution of β and of the initial beliefs on β^* . In the Appendix (section 15), we illustrate the possibilities in the context of a three-voter example. Essentially, the example suggests that when committee members' ideal points over consequences are not "too" dispersed or asymmetrically distributed, it is the relative extremists who have an incentive to conceal private information. But if the distribution of these ideal points is sufficiently skewed about the median, it is the relative moderates who have such an incentive. A sufficient condition for all legislators to volunteer all of their private information in debate is (quantitative) symmetry in the distribution of ideal points, $\{z_i\}$; that is, for any z_i less than z_{i^*} , there is a distinct z_j greater than z_{i^*} such that $(z_{i^*} - z_i) = (z_j - z_{i^*})$. In this instance, the value of Δ is identically zero. Although slight deviations from symmetry leave Δ "close" to zero, we cannot say surely whether or not such deviations induce information sharing that is "close" to complete. Our conjecture is that it will be close. With small Δ , the expected change in the location of the winning proposal is small, but with, say, no information sharing on the part of those with any incentive to conceal, the variance associated with this location is high. Because legislators are risk averse, therefore, we expect relatively weak incentives to hide private information in this case.

Conclusion

The importance of imperfect and incomplete information for explaining much of what we observe in the political

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world has long been recognized (by, e.g., Mill 1910, Milton 1927; and, in a more abstract way, Downs 1957). However, it is only relatively recently that appropriate analytical techniques have been developed with which to address the issues. The relevant literature is correspondingly thin, although growing rapidly, and most of this work is concentrated in models of elections (see Calvert 1986 for a recent review). There are three principal ways in which information can be transmitted in the legislative process: through voting behavior, through making proposals or amendments, and through debate. We know of only one work explicitly concerned with any of these, namely Ordeshook and Palfrey 1985, which examines some examples of sophisticated voting in which legislators know only their own preferences surely. The authors do not address proposals and debate, the focus of attention here. In particular, we concentrate on the extent to which a legislative, decision-making process is coherent in the sense that it generates incentives for legislators to share any private information germane to the decision. For the procedure studied here, our results suggest that while agendas may indeed be set under effectively pooled information, coherence—in the sense of information sharing—is not generally guaranteed.

Given our contemporary pessimism about the rationality of social life, our main result on coherence may seem obvious. However, not everyone shares this pessimism and, frequently, what is presupposed in accepting the “obvious” is not always so transparent (witness, for instance, the history of the “law of demand” in economics). Furthermore, we did not anticipate our additional result as to *which* legislators, in committees of the type studied here, would have incentives to conceal private information. (It should be noted, however, that this characterization is essentially an artifact of the assumption of a one-dimensional issue.)

Admittedly, these results come from a sparse model. However, one way to study reality is to examine models where the assumptions are made for the sake not of verisimilitude but of a conservative test. Our model, as argued earlier, is biased toward full revelation. As such, the results have substantive implications both descriptively and normatively.

For the proposition on coherence, just as the possibility of strategic generation of cycles refutes the argument that Condorcet voting rules produce the socially preferred choice, so the possibility of strategic revelation of information refutes the argument that Condorcet voting rules produce the correct choice—Condorcet’s so-called jury theorem (see Black 1958, 163–65). More generally, our result calls into question some extreme claims of liberalism, for instance, that truth triumphs in the marketplace of ideas.

Given this descriptive and normative significance, it is important to ask, How general is our result? We deliberately made our model simple enough to analyze the bare bones of the strategic problem. While we did not find it necessary to limit the number of people (our assumption of an odd number of committee members is simply a convenience to identify the median uniquely), we did limit the number of dimensions, the legislative procedure (e.g., the number of readings), and the range of rhetorical strategy (by disallowing biased information). So it is reasonable to inquire whether our main result—that coherence cannot be guaranteed—carries over into more complicated models and even into the real world.

We conjecture that our result holds up. Suppose one increased the number of dimensions, thereby eliminating or obscuring the multidimensional median. Because the revelation that occurs in our model depends on knowledge of the median, it seems unlikely that the absence or invisibility of a median will motivate coherence.

Suppose one altered the legislative procedure by increasing the number of readings. Such a model, being more specific than ours, might institutionally encourage full revelation. That is, legislators might, after initial readings, formulate their ex post optima, b^+ , relative to these preliminary readings, to use as b for the final reading; thus perhaps actually achieving an agenda b^+ after the final reading such that $b^+ = b$. If so, they satisfy the definition of coherence. The expectation that coherence will be produced in this way depends, we conjecture, on whether the number of readings is finite or not. If there are an infinite number of readings, then with (in effect) an infinity of test votes, other participants may well be able to infer concealed information, thereby generating coherence. Suppose, however, that the number of readings is finite, perhaps limited to two or three as in normal legislative procedure. If there is strategic advantage to concealing information in debate, then this advantage surely persists through debate, proposals, and voting of all penultimate readings. In which case, legislators would be expected to reveal fully only at the proposal and vote of the final reading. Thus the model of multiple readings may well reduce to the case of a single reading.

Whether a model of committees and legislatures is appropriately conceived as having a finite or infinite number of readings depends on how specifically one wishes to model institutions. If institutions are subordinated to the whole political process (including elections, campaign polls, etc.) in a seamless and continuous world, then one ought to assume infinite readings, for politics goes on forever. If, however, the investigatory emphasis is on institutions as such, then the model of finite readings seems appropriate. In which case, the results of our model are likely to be robust.

Suppose, finally, that contrary to our assumptions, legislators reveal biased

information. This is surely a more realistic model than ours. It means that, besides giving simply noisy signals, rhetoricians might offer biased information. In our example of fluoridation, the variable would involve not only the amount of naturally occurring fluoride but also the quality of estimates made by persons variously concerned about caries, cancer, or communism. We conjecture that considerations of bias would do nothing to increase coherence (though they raise a host of important issues in their own right).

Consider two cases, one where any debater's bias is common knowledge and one where it is not. If bias is known, it can be appropriately discounted and is indeed irrelevant. If, on the other hand, voters believe that information is biased but do not surely know exactly how, this belief compounds uncertainty and complicates legislators' evaluations of the median and of the strategy of revelation. If voters are Bayesian in estimating bias, debaters are then likely to distort in strategic ways. If, however, voters are Laplacian in their treatment of bias (and thus inconsistent with the assumptions of this model), debaters are then encouraged to lie directly. So, if there is ever an advantage to conceal one's true position, the possibility of bias, it seems, will at least not increase, and will probably decrease, coherence.

Appendix

We begin by describing, in sections 1-7, the formal structure of the decision-making game being analyzed.

1. *Committee.* Let $N = \{1, 2, \dots, n\}$ be the set of committee members, n odd and $n \geq 3$. Each $i \in N$ is endowed with quadratic preferences over the set of consequences, called outcomes, R : $U_i(y) = -(z_i - y)^2$, $\forall y \in R$ where $z_i \in R$ is i 's given ideal point. Without loss of general-

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ity, index N so that $i < j$ if and only if $z_i < z_j$, for all $i, j \in N$.

2. *Policy Issue.* The committee has to select a policy, called a bill, $b \in R$. Policies equal outcomes up to a realization of a one-dimensional random variable β . An outcome is then a mapping, $y: R^2 \rightarrow R$ such that $y(b | \beta) = b + \beta$. Assume $\beta \sim N(\beta^*, 1)$ and $\text{cov}(\beta, b) = 0$.

3. *Information.* The mean β^* is assumed unknown. All legislators share common priors over this mean, described by the standardized normal distribution, Φ . Prior to going into committee, each $i \in N$ observes an independent random sample of m draws from $N(\beta^*, 1)$: let i 's sample mean be β_i . The structure of preferences, the distribution of ideal points over outcomes $\{z_i\}$, the sample size m , and the shared prior distribution of β are all common knowledge. Each individual i 's sample mean β_i is private information, known only to i .

4. *Induced Preferences.* From 1, 2, and 3, on going into committee, each $i \in N$ has induced preferences over bills given by

$$\begin{aligned} u_i(b) &= E[-(z_i - b - \beta)^2 | \beta_i] \\ &= -(z_i - b)^2 + E[\beta | \beta_i] \cdot 2(b - z_i) \\ &\quad - E[\beta | \beta_i] - \text{var } \beta \end{aligned}$$

This indirect utility function is symmetric-single-peaked in b , with induced ideal point given by $\zeta_i = z_i - E[\beta | \beta_i]$. Applying Bayes Rule to individuals' priors over β gives $E[\beta | \beta_i] = m\beta_i / (m + 1)$, $i \in N$. Clearly there is no reason for the distribution of induced ideal points $\{\zeta_i\}$ to be the same as that of ideal points over consequences.

5. *Decision-Making Procedure.* Committee decision making has three stages: debate, agenda setting, and voting. In debate, legislators simultaneously reveal noisy messages:

$$\begin{aligned} \hat{\beta}_i &= \beta_i + \eta_i; \eta_i \sim N(0, s_i), s_i \in [0, \infty], \\ &\forall i \in N. \end{aligned}$$

Here, the level of noise s_i is a control variable of legislator i . We suppose that on hearing any legislator's message, the level of noise selected by the legislator becomes common knowledge. This is evidently an extreme case. It is, however, extreme at the conservative end of the spectrum, particularly since we have ruled out biased signals. Once messages are revealed, each legislator i (simultaneously, from a strategic perspective) makes a proposal, a value $b_i \in R$. The list of proposals $\{b_i\}$ constitutes the agenda. Finally, the committee decision is determined by simple majority voting over all pairs of alternatives in the agenda. By construction, a determinate decision will always emerge (cf. sec. 4). Without loss of generality, assume the sequence of votes is given by $\Sigma = ((b_i \text{ vs. } b_{i+j})_{j=1, \dots, n-1})_{i=1, \dots, n-1}$. In words, Σ is the sequence-of-sequences given by placing b_1 first against b_2 , then against b_3 , and so on until finally placed against b_n . Then b_2 is placed against b_3 , then against b_4 , and so on until finally placed against b_n . Then b_3 is placed against b_4 , and so on. Finally, the sequence-of-sequences ends with b_{n-1} being placed against b_n .

6. *Strategies.* A strategy for legislator i is a triple consisting of a signalling rule, a proposal rule, and a voting rule. Mixed strategies are not permitted. Then, i 's signalling rule is a function, $s_i: z_i \times R \rightarrow [0, \infty]$; i 's proposal rule is a function, $b_i: z_i \times R \rightarrow R$; i 's voting rule is a function, $v_i: z_i \times R \times R^n \times \Sigma \rightarrow \{0, 1\}^{n(n-1)/2}$, where in the vote between b_i and b_{i+j} , a value of 0 denotes a vote in favor of b_{i+j} and a value of 1 denotes a vote in favor of b_i . R^n here is the set of possible agendas.

We write $t_i = (s_i(z_i, \beta_i), b_i(z_i, \beta_i), v_i(z_i, \beta_i, \{b_j\}, \Sigma)) \in T$, $\forall i \in N$, where T is the set of all possible strategies.

7. *Objectives.* Let $\Omega_i^o = \{\beta_i, \{z_i\}, m\} \in \Omega^o$ describe i 's information at the start of the debate stage, where Ω^o is the family of all possible such informations; and let $\Omega_i = \Omega_i^o \cup \{(\beta_j, s_j) \mid j \in N\} \in \Omega$ describe i 's information at the start of the agenda setting stage, where Ω is the family of all possible such informations. Each legislator i chooses a strategy t_i to maximize i 's expected pay-off from the committee's final decision. Let $E[b^* \mid \Omega_i^o, \{t_j\}]$ be i 's expectation of the winning proposal in the voting stage, conditional on i 's beliefs at the start of the debate stage and all legislators' strategies. Let $\{t_{-i}\} = \{t_j \mid j \neq i\}$. Substituting b^* for b in i 's indirect utility schedule specified in 4, i 's maximand is given by

$$\begin{aligned} & E[u_i(b^*) \mid \Omega_i^o, t_i, \{t_{-i}\}] \\ &= E[-(z_i - b^*)^2 + E[\beta \mid \Omega_i^o, \{t_{-i}\}] \\ &\quad \cdot (2(z_i - b^*) - E[\beta \mid \Omega_i^o, \{t_{-i}\}]) \\ &\quad \mid \Omega_i^o, \{t_j\}] - \text{var}(\beta \mid \Omega_i^o, \{t_{-i}\}) \\ &= -z_i^2 + E[\beta \mid \Omega_i^o, \{t_{-i}\}] \\ &\quad \cdot (2z_i - E[\beta \mid \Omega_i^o, \{t_{-i}\}]) \\ &\quad + E[b^* \mid \Omega_i^o, \{t_j\}] \\ &\quad \cdot (2(z_i - E[\beta \mid \Omega_i^o, \{t_{-i}\}]) \\ &\quad - E[b^* \mid \Omega_i^o, \{t_j\}]) \\ &\quad - \text{var}(\beta \mid \Omega_i^o, \{t_{-i}\}) \\ &\quad - \text{var}(b^* \mid \Omega_i^o, \{t_j\}) \end{aligned}$$

To economize on notation, let $w_i(t_i, t_{-i} \mid \Omega_i^o) \equiv E[u_i(b^*) \mid \Omega_i^o, t_i, \{t_{-i}\}]$. We can now write the committee decision-making game in normal form as, $\Gamma = (T^n, \{w_i \mid i \in N\})$.

8. *Equilibrium and Proposition.* Let $\tau = 1, 2, 3$, index each of the three stages of the committee game—respectively, debate, agenda setting, and voting. Let $\Omega_{i(\tau)}$ be i 's information at the start of stage τ : thus, $\Omega_{i(1)} = \Omega_i^o$, $\Omega_{i(2)} = \Omega_i$, and $\Omega_{i(3)} = \Omega_i \cup \{b_j\}$. Let $w_{i(\tau)}(\cdot, \cdot \mid \Omega_{i(\tau)})$ be i 's expected payoff at the start of stage τ , conditional on i 's information at that stage: by defini-

tion, $w_{i(1)}(\cdot, \cdot \mid \Omega_{i(1)}) \equiv w_i(\cdot, \cdot \mid \Omega_i^o)$. A perfect Bayesian equilibrium (PBE) to the committee game Γ is an n -tuple of strategies $\{t_i^*\}$ such that for each $i \in N$, for all $\beta_i \in R$, and for each $\tau = 1, 2, 3$,

$$\begin{aligned} & w_{i(\tau)}(t_i^*, t_{-i}^* \mid \Omega_{i(\tau)}) \\ & \geq w_{i(\tau)}(t_i, t_{-i}^* \mid \Omega_{i(\tau)}) \quad \forall t_i \in T \end{aligned}$$

Hence, a PBE is a set of strategies, $\{t_i^*\}$, that constitutes a Bayesian equilibrium at each stage of the game.

Let $E[\mu]$ be the common expectation of the median proposal in the agenda-setting stage of the game, conditional on the common-knowledge data but prior to any legislator observing his or her private information: this will be defined rigorously below (sec. 10).

PROPOSITION: *There exists an essentially unique PBE to Γ , and it is such that relative to their information at each stage,*

1. all legislators vote sincerely in the voting stage
2. all legislators propose their expected induced ideal points in the agenda-setting stage
3. legislator $(n + 1)/2 \equiv i^*$ always volunteers all his or her private information in debate ($s_i = 0$)
4. if $E[\mu] = z_i^*$, then all legislators volunteer all their private data in debate
5. if $E[\mu] < (>) z_i^*$, then any legislator $i > (<) i^*$ will volunteer all his or her private data in debate
6. if $E[\mu] < (>) z_i^*$, then any legislator $i > (<) i^*$ has an incentive to conceal part or all of his or her private data in debate ($s_i \in [0, \infty)$). Partial revelation of data is not fully revealing; and what the legislators choose to do depends on the particular distribution of preferences and parameters of Γ
7. suppose $E[\mu] < z_i^*$. Then in the

equilibrium, $s_i \geq s_{i+1}$ for $i = 1, \dots, i^* - 1$. *Mutatis mutandis*, the analogous result holds when $E[\mu] > z_i^*$

9. *Claims 1 and 2 of the Proposition.* Because we are looking for a PBE, we prove the proposition by solving the final stage of the game first, then working backwards to the debate stage. So consider the voting stage and fix the earlier proposal and debate strategies arbitrarily.

For the voting process described in section 5, it follows from results of McKelvey and Niemi 1978 (for the complete information case) and Ordeshook and Palfrey 1985 (for the incomplete information case) that all individuals have a weakly dominant voting strategy, namely, for any agenda, to vote sincerely with respect to their induced preferences and beliefs (section 4). This is Claim 1 of the proposition. Now consider the proposal stage of the game.

Let $\{b_i\}$ be any agenda and let $\{(\xi_i | \Omega_i, \{b_i\})\}$ be the list of induced ideal points at the start of the voting stage of the game: from section 4, $(\xi_i | \Omega_i, \{b_i\}) = z_i - E[\beta | \Omega_i, \{b_i\}]$, all i . Then by section 4 and the previous result on voting strategies, the winning proposal from $\{b_i\}$ will be that most preferred by the individual with the median induced ideal point in $\{(\xi_i | \cdot)\}$. In particular, the winning proposal is defined by $b^* \in \{b_i\}$ such that

$$|(\xi_\mu | \cdot) - b_i| \geq |(\xi_\mu | \cdot) - b^*|, \forall b_i \in \{b_i\}$$

where ξ_μ denotes the relevant-median induced ideal point. Now consider any individual i at the start of the agenda-setting stage. Let $(\xi_i | \Omega_i)$ be i 's induced ideal point and let $E[\xi_\mu | \Omega_i]$ be i 's expectation of the median induced ideal point at this stage of the game. Then by the characterization of the de facto winning proposal, we have $E[b^* | \Omega_i, \{t_j\}] = E[\xi_\mu | \Omega_i]$ and $\text{var}(b^* | \Omega_i, \{t_j\}) = \text{var}(\xi_\mu | \Omega_i)$. Fix an arbitrary list of pro-

posals of legislators other than i , $\{b_{-i}\}$. Then by the single-peakedness of induced preferences over bills, it follows that legislator i has a dominant proposal strategy, namely, set $b_i = (\xi_i | \Omega_i)$. This proves Claim 2 of the proposition.

10. *Some Preliminary Results.* In this section, we establish some properties of legislators' expected payoffs, evaluated at the start of the debate stage of the game. In particular, we show that any legislator's expected payoff is independent of his or her private information.

Given equilibrium second- and third-stage strategies, let $\{s_j\}$ be an arbitrary list of debate strategies, and consider individual i 's maximand at the start of the debate stage of the game, $w_i(t_i, t_{-i} | \Omega_i^o)$. Let $E[\xi_\mu | \Omega_i^o, \{s_j\}]$ be i 's expectation of the median induced ideal point on going into committee, with associated variance $\text{var}(\xi_\mu | \Omega_i^o, \{s_j\})$. Note that $E[\xi_\mu | \Omega_i^o, \{s_j\}] \neq E[\xi_\mu | \Omega_i]$ is both possible and likely. By convention, we have $z_i < z_j$ if and only if $i < j$ (sec. 1). Let Π be the set of permutations of N and, $\forall \pi \in \Pi$, let $\mu(\pi)$ be the median individual under the bijection π (so for the identity bijection, π_o , $\mu(\pi_o) = (n + 1)/2$). Hence we can write,

$$E[\xi_\mu | \Omega_i^o, \{s_j\}] = \sum_{\pi \in \Pi} p(\pi | \Omega_i^o, \{s_j\}) \cdot E[\xi_{\mu(\pi)} | \Omega_i^o, \{s_j\}]$$

where $p(\pi | \Omega_i^o, \{s_j\})$ is the probability that the ordering of the set of induced ideal points $\{\xi_j\}$ going into committee is given by π , given i 's information at this stage. Across individuals, the family of information sets $\{\Omega_i^o\}$ differs only in the realizations of individual sample means, $\{\beta_i\}$. Because all individuals' draws are i.i.d. from the same normal distribution and the common prior on β^* is also normal, we have, therefore,

$$p(\pi | \Omega_i^o, \{s_j\}) = p(\pi | \{s_j\}) \quad \forall i \in N, \\ \forall \pi \in \Pi$$

From section 4,

$$\xi_{\mu(\pi)} = z_{\mu(\pi)} - E[\beta \mid b_{\mu(\pi)}, \{s_j\}].$$

Hence, $\forall \pi$:

$$\begin{aligned} E[\xi_{\mu(\pi)} \mid \Omega_i^o, \{s_j\}] &= z_{\mu(\pi)} \\ &- E[\beta \mid \Omega_i^o, \pi, \{s_j\}] \\ &= z_{\mu(\pi)} - E[\beta \mid \beta_i] + f(\pi \mid \Omega_i^o, \{s_j\}) \end{aligned}$$

where $f(\cdot) \in \mathbb{R}$ accounts for the difference between i 's observation, β_i , and that of individual $\mu(\pi)$, $\beta_{\mu(\pi)}$, necessary to yield individual $j = \mu(\pi)$ median in induced ideal points going into committee. (By definition, $f(\pi \mid \Omega_i^o, \cdot) = 0 \forall \pi \in \Pi$ such that $\mu(\pi) = (n+1)/2$.) Specifically, the term $\{E[\beta \mid \beta_i] + f(\cdot)\}$ is the mean of the appropriately truncated normal distribution, that is, $N(E[\beta \mid \beta_i], (m+1+\sigma)^{-1})$, where $\sigma = \sum_{N/\mu(\pi)} m \cdot (m \cdot s_j + 1)^{-1}$, with the truncation depending on $\{\{z_i\}, \pi\}$. Again, by virtue of all legislators' draws being i.i.d. from the same normal distribution, and the common prior on β^* being normal, it follows from the definition of $\xi_{\mu(\pi)}$ that

$$\begin{aligned} f(\pi \mid \Omega_i^o, \{s_j\}) &= f(\pi \mid \{s_j\}) \quad \forall i \in N, \\ &\quad \forall \pi \in \Pi \end{aligned}$$

Consequently, since both $p(\cdot)$ and $f(\cdot)$ are independent of private information,

$$\begin{aligned} \text{var}(\xi_{\mu} \mid \Omega_i^o, \{s_j\}) &= \text{var}(\xi_{\mu} \mid \{s_j\}) \\ &\quad \forall i \in N \end{aligned}$$

Now i 's maximand $w_i(\cdot \mid \Omega_i^o)$ depends on i 's private information β_i if and only if it depends on $E[\beta \mid \beta_i]$. By section 9, i 's expectation of the winning proposal at the start of this debate stage is given by i 's expectation of the location of the median proposal, $E[\xi_{\mu} \mid \Omega_i^o, \cdot]$. So substituting for $E[b^* \mid \cdot]$ and $\text{var}(b^* \mid \cdot)$ in $w_i(\cdot \mid \Omega_i^o)$ and differentiating with respect to $E[\beta \mid \beta_i]$ gives (where we write $E_i \equiv E[\beta \mid \beta_i]$)

$$\begin{aligned} \partial w_i(\cdot \mid \Omega_i^o) / \partial E_i &= 2\{z_i - E_i + (z_i - E_i \\ &- E[\xi_{\mu} \mid \cdot]) \cdot \partial E[\xi_{\mu} \mid \cdot] / \partial E_i \\ &- E[\xi_{\mu} \mid \cdot]\} \end{aligned}$$

But from the earlier reasoning,

$$\begin{aligned} \partial E[\xi_{\mu} \mid \cdot] / \partial E_i &= \sum_{\Pi} p(\pi \mid \cdot) \\ &\cdot \partial E[\xi_{\mu(\pi)} \mid \Omega_i^o, \cdot] / \partial E_i = -1 \end{aligned}$$

Hence, $\partial w_i(\cdot \mid \Omega_i^o) / \partial E_i = 0 \forall i \in N$. In other words, the value of any individual's maximand for the committee game Γ is independent of that individual's particular observation, β_i . So in choosing strategies for the game, all that matters are the relative distances separating individuals' induced ideal points over bills.

From section 9, individual i 's best proposal strategy in the agenda-setting stage is to propose his or her induced ideal point, $(\xi_i \mid \Omega_i)$, at that stage. Since the agenda generated in this way constitutes a unique subgame equilibrium, the agenda fully reveals all the private data once it is announced at the start of the voting stage. Legislator $i^* = (n+1)/2$ will therefore surely be the median voter at the voting stage; and this is known to all individuals on going into committee for the debate stage of the game. Still assuming that equilibrium second- and third-stage strategies are used by all individuals, define the gap

$$\begin{aligned} \Delta(s_i \mid \Omega_i^o, \{s_{-i}\}) &= E[\xi_{i^*} \mid \Omega_i^o, \{s_j\}] \\ &- E[\xi_{\mu} \mid \Omega_i^o, \{s_j\}] \\ &= z_{i^*} - E[\beta \mid \Omega_i^o, \pi_o, \{s_{-i}\}] \\ &- E[\xi_{\mu} \mid \Omega_i^o, \{s_j\}] \end{aligned}$$

Define $E[\mu] \equiv E[\xi_{\mu} \mid (\Omega_i^o : \beta_i = 0)]$. Then, by the facts established above and the structure of admissible debate strategies (sec. 5), we have $\forall i \in N, \forall \Omega_i^o \in \Omega^o$:

$$\begin{aligned} [s_i = \infty \quad \forall i \in N] &\rightarrow [\Delta(s_i \mid \cdot, \{s_{-i}\}) \\ &= z_{i^*} - E[\mu] \text{ and } \text{var}(\xi_{\mu} \mid \cdot, \{s_j\}) > 0] \\ [s_i = 0 \quad \forall i \in N] &\rightarrow [\Delta(s_i \mid \cdot, \{s_{-i}\}) \\ &= 0 \text{ and } \text{var}(\xi_{\mu} \mid \cdot, \{s_j\}) = 0] \end{aligned}$$

and

$$[s_i \leq s_i'] \rightarrow [|\Delta(s_i \mid \cdot, \{s_{-i}\})|$$

$$\leq |\Delta(s_i' | \cdot, \{s_{-i}\})| \text{ and} \\ \text{var}(\xi_\mu | \cdot, s_i, \{s_{-i}\}) \leq \\ \text{var}(\xi_\mu | \cdot, s_i', \{s_{-i}\})$$

11. *Claims 3, 4 and 5 of the Proposition.* Given the results established in section 10, we can now prove claims 3, 4, and 5 of the Proposition. We do this directly, by showing that the expected payoff to any of the specified legislators at the start of the debate stage is strictly decreasing in the amount of noise added to the debate message.

Suppose $E[\mu] \leq z_i^*$ and consider $i \geq i^*$. Fix $\{s_{-i}\}$ and consider i 's maximand, $w_i(t_i, t_{-i} | \Omega_i^0)$. Assuming equilibrium second- and third-stage strategies, the argument of section 9 allows us to substitute for $E[b^* | \Omega_i^0, \{t_j\}]$ and obtain

$$w_i(t_i, t_{-i} | \Omega_i^0) = w_i(\Omega_i^0, \{t_{-i}\}) \\ + E[\xi_\mu | \Omega_i^0, s_i, \{s_{-i}\}] \cdot (2(\xi_i | \Omega_i^0) \\ - E[\xi_\mu | \Omega_i^0, s_i, \{s_{-i}\}]) \\ - \text{var}(\xi_\mu | \Omega_i^0, s_i, \{s_{-i}\})$$

where $\omega_i(\cdot)$ is the sum of terms in $w_i(\cdot)$ that are independent of t_i . Given $\{t_{-i}\}$ and given i 's best second- and third-stage strategies, legislator i 's optimal debate strategy is given by $s_i \in [0, \infty]$ which maximizes $W_i(\cdot) = [w_i(\cdot) - \omega_i(\cdot)]$. Substituting for $E[\xi_\mu | \cdot, \{s_{-i}\}]$ in $W_i(\cdot)$ and rearranging gives

$$W_i(\cdot) = \{E[\xi_{i^*} | \cdot] - \Delta(s_i | \cdot, \{s_{-i}\})\} \\ \cdot \{2(\xi_i | \cdot) - E[\xi_{i^*} | \cdot]\} \\ + \Delta(s_i | \cdot, \{s_{-i}\}) \\ - \text{var}(\xi_\mu | \cdot, s_i, \{s_{-i}\})$$

Differentiating $W_i(\cdot)$ with respect to s_i gives

$$\partial W_i / \partial s_i = -2\partial |\Delta(s_i | \cdot)| / \partial s_i \cdot \{(\xi_i | \cdot) \\ - E[\xi_{i^*} | \cdot] + \Delta(s_i | \cdot)\} \\ - \partial \text{var}(\xi_\mu | \cdot, s_i, \cdot) / \partial s_i \\ = -2\partial |\Delta(s_i | \cdot)| / \partial s_i \cdot \alpha_i(s_i, \{s_{-i}\})$$

$$- \partial \text{var}(\xi_\mu | \cdot, s_i, \cdot) / \partial s_i$$

where $\alpha_i(s_i, \{s_{-i}\}) = \{(\xi_i | \cdot) - E[\xi_\mu | \cdot, s_i, \cdot]\}$, the gap between i 's induced ideal point and i 's expectation of the location of the median proposal. From section 10, both partial derivatives are positive. By supposition, $E[\mu] \leq z_i^*$; so, again from section 10, $E[\xi_\mu | \cdot, s_i, \cdot] \leq E[\xi_{i^*} | \cdot] \forall \{s_j\}$. Because $i \geq i^*$, therefore, $\alpha_i(\cdot) \geq 0$. Hence, $\partial W_i(\cdot) / \partial s_i < 0 \forall s_i > 0$. This establishes the first part of Claim 5 in the proposition.

When $E[\mu] \geq z_i^*$, an entirely symmetric argument establishes that all $i \leq i^*$ will choose to volunteer all their private data (i.e., select $s_i = 0$). This proves the rest of Claim 5. Combining the two cases establishes Claims 3 and 4.

12. *Claim 6 of the Proposition.* To prove this claim, we first show that given the debate strategies of others, any of the specified legislators may have the ability and incentive to conceal some or all of his or her private information in debate. Then we exploit a straightforward fixed-point argument to demonstrate that an equilibrium set of debate strategies exists.

So suppose $E[\mu] < z_i^*$. For all $\{s_j\}$ and for at least one individual $i < i^*$, the difference $\alpha_i(\{s_j\})$ in $\partial W_i(\cdot) / \partial s_i$ must be negative; and for s_j sufficiently small for all j , $\alpha_i(\cdot)$ must be negative for all $i < i^*$ (see sec. 10). From Claim 5, all legislators $j \geq i^*$ will volunteer all their private data, that is, $s_j = 0 \forall j \geq i^*$. Leaving this implicit, let $\{s_{-i}\}$ be an otherwise arbitrary list of debate strategies of all legislators other than i , where $i < i^*$. Suppose $\alpha_i(\infty, \{s_{-i}\}) < 0$. Then for individual i , decreasing the amount of noise in his or her debate signal raises the expected payoff through reducing $\text{var}(\xi_\mu | \cdot)$ but lowers the expected payoff through reducing $|\Delta(s_i | \cdot)|$. Thus, i faces a trade-off. Suppose there exists an $s_i' \in (0, \infty)$ such that $\partial W_i(\cdot) / \partial s_i = 0$ at s_i' (see sec. 11, where this derivative is calculated). In this

case, i has an incentive to reveal some, but not all, of his or her private information. From section 10, the derivative of variance in $\partial W_i(\cdot)/\partial s_i$ (which is clearly monotone in s_i) and $\alpha_i(\cdot)$ are independent of β_i and thus common knowledge. Moreover, substituting from section 10, we have

$$\begin{aligned} \partial \Delta(s_i | \cdot) / \partial s_i &= -\partial E[\xi_\mu | \Omega_i^\rho, s_i, \{s_{-i}\}] / \partial s_i \\ &= -[\Sigma_\Pi \partial p(\pi | \cdot) / \partial s_i \cdot \{z_{\mu(\pi)} \\ &\quad - E[\beta | b_i] + f(\pi | \cdot)\} \\ &\quad + \Sigma_\Pi p(\pi | \cdot) \cdot \partial f(\pi | \cdot) / \partial s_i] \\ &= -[\Sigma_\Pi \partial p(\pi | \cdot) / \partial s_i \cdot \{z_{\mu(\pi)} + f(\pi | \cdot)\} \\ &\quad + \Sigma_\Pi p(\pi | \cdot) \cdot \partial f(\pi | \cdot) / \partial s_i] \end{aligned}$$

And this is independent of i 's private information, β_i . Thus, choosing an interior value of noise, $0 < s_i < \infty$, is not invertible by other committee members, and, therefore, given $\{s_{-i}\}$, i 's best choice of s_i can be determined by simply examining the derivative of W_i with respect to s_i . In other words, i has the ability to offer some but not all of his or her private information if he or she so wishes. However, $\partial W_i(\cdot)/\partial s_i$ is clearly dependent on $\{s_{-i}\}$, so we need to demonstrate the existence of an equilibrium set of debate strategies.

Let $G_i(\{s_{-i}\})$ be i 's best reply correspondence (in debate strategies) to others' choices, and let $g_i(\{s_{-i}\}) = \inf\{s_i \in G_i(\cdot)\}$. Now $\partial W_i(\cdot)/\partial s_i$ is surely differentiable in $\{s_{-i}\}$. Therefore, by the Implicit Function Theorem, $g_i(\cdot)$ is a continuous function of $\{s_{-i}\} \forall \{s_{-i}\}$ such that $g_i(\cdot) \in (0, \infty)$. Suppose there is a sequence $(\{s_{-i}\}^\nu) \rightarrow \{s_{-i}\}^0$ such that $(g_i^\nu) \rightarrow g_i^+ > g_i(\{s_{-i}\}^0) = 0$. Then $\partial W_i(\cdot)/\partial s_i = 0$ at $g_i^\nu \forall \nu < \infty$ and $\partial W_i(\cdot)/\partial s_i < 0$ at g_i^+ . But this implies that $W_i(\cdot)$ is not smooth in $\{s_j\}$ for sufficiently large ν , a contradiction. Hence, $g_i(\cdot)$ is a continuous function. Assume all legislators' debate strategies are bound above by some arbitrary finite

number, say, $K < \infty$ (note that this bound is irrelevant for $j \geq i^*$, by Claim 5). Then for any i , $g_i(\{s_{-i}\}) \in [0, K]$. Let $\gamma(\{s_j\}) = (\{g_i(\{s_{-i}\})\}_{i \in N})$. Then $\gamma(\cdot)$ is a continuous mapping from $[0, K]^n$ into $[0, K]^n$. By virtue of the n -dimensional version of Brouwer's Fixed Point Theorem, the mapping $\gamma(\cdot)$ has a fixed point. Given the restriction on individuals' strategy spaces, this point clearly constitutes an equilibrium for the debate stage of the game: call this set of strategies a K -equilibrium and write $s_i(K)$ to denote i 's K -equilibrium strategy. Note that $s_i(K)$ is continuous in K . If for some suitably large (and finite) K , $s_i(K) < K \forall i \in N$, then we are done. Suppose that for all finite K and all $i < i^*$, $s_i(K) = K$. Then as $K \rightarrow \infty$, $\lim s_i(K) = \infty \forall i < i^*$. Hence, $s_i = \infty \forall i < i^*$ and $s_i = 0 \forall i \geq i^*$ is surely an equilibrium for the debate stage. So suppose there is some (not necessarily fixed) individual j for whom $s_j(K) = K$, all finite K and that there is some distinct $i < i^*$ for whom $s_i(K) < K$ in some K -equilibrium, say, $K = K^*$. Then as $K \rightarrow \infty$, $\lim s_j(K) = \infty$. From definition of $W_i(\cdot)$, it follows that for arbitrary $i, j < i^*$, if $s_j > s_j'$, then, holding $(\{s_{-i}\}/s_j)$ fixed,

$$\begin{aligned} [g_i(s_j', \cdot) < \infty] &\rightarrow [g_i(s_j, \cdot) \\ &\leq g_i(s_j', \cdot)] \end{aligned}$$

Therefore, $s_i(K)$ continuous in K and $s_i(K^*) < K^*$ imply $s_i(K) < K, \forall K > K^*$. Hence, $s_i(K) \in [0, K^*) \forall K > K^*$, and, therefore, the sequence $(s_i(K))_{K \rightarrow \infty}$ contains a convergent subsequence with limit in $[0, K^*)$. Thus, an infinite equilibrium exists. This completes the proof of Claim 6, since the case of $E[\mu] > z_i^*$ follows in a symmetric manner.

13. "Essential uniqueness" of equilibria to Γ . Before going on to establish Claim 7, it is convenient here to show that the equilibrium to the game is unique. This amounts to showing that if there are dis-

tinct equilibrium sets of debate strategies, each such set gives rise to the same expected payoff to all legislators.

Sections 9–12 establish the existence of a perfect equilibrium $\{t_i^*\}$ to the committee game Γ , which satisfies Claims 1–6. Because the second- and third-stage strategies are dominant strategies, the equilibrium is essentially unique if, whenever $\{s_i'\}$, $\{s_i''\}$ are any pair of equilibrium sets of debate strategies, then $|\Delta(\{s_i'\})| = |\Delta(\{s_i''\})|$.

To check this equality, suppose the claim to be false. Let $E[\mu] < z_{i^*}$ and assume $|\Delta(\{s_i'\})| > |\Delta(\{s_i''\})|$. Then, $\text{var}(\cdot | \{s_i'\}) > \text{var}(\cdot | \{s_i''\})$ by section 10. Hence, there exist legislators $j < i^*$ such that $s_j' > s_j'' \geq 0$. Suppose for such a legislator that $w_j(\{t_i'\}) > w_j(\{t_i''\})$. From section 10, $|\Delta(s_j^o | \{s_{-j}''\})| > |\Delta(s_{-j}'' | \{s_{-j}''\})|$ (and similarly for the variances) for some $s_j^o > s_j''$. So by continuity of $w_j(\cdot | \cdot)$, there is $s_j^o > s_j''$ for which $w_j(t_j^o, \{t_{-j}''\}) > w_j(\{t_i''\})$. This contradicts the assumption that $\{s_i''\}$ is an equilibrium for the debate stage. Similarly, $w_j(\{t_i'\}) < w_j(\{t_i''\})$ is impossible. Hence, $w_j(\{t_i'\}) = w_j(\{t_i''\})$ is necessary for all $j < i^*$ having $s_j' > 0$. Suppose there are at least two such individuals, $j, k < i^*$. Without loss of generality, let $j < k$. But then $|\alpha_j(\{s_i\})| > |\alpha_k(\{s_i\})| \forall \{s_i\}$. Hence, $w_k(\{t_i'\}) = w_k(\{t_i''\})$ implies $w_j(\{t_i'\}) > w_j(\{t_i''\})$, a contradiction. Therefore, there can be only one individual $j < i^*$ with $s_j' > 0$. Symmetric reasoning likewise yields that there can be at most one individual $k < i^*$ with $s_k'' > 0$. If $j \neq k$, then $|\alpha_j(\cdot)| \neq |\alpha_k(\cdot)|$, in which case it is impossible for j and k to be indifferent between the two equilibria, as required: for, say, j to be indifferent would require $s_j' = s_k''$, but this would mean k could not be indifferent. Hence, either $j = k$ or there exists no $k < i^*$ such that $s_k'' > 0$. In the first case, if $j = k$, then $s_i' = s_i'' = 0$ for all $i \neq j$. In which case $\text{argmax } w_j(\cdot)$ is unique; but then

$|\Delta(\{s_i'\})| = |\Delta(\{s_i''\})|$. In the remaining case, we have $s_i'' = 0 \forall i \in N$. But since $s_i' = 0 \forall i \in N \setminus \{j\}$, uniqueness of $\text{argmax } w_j(\cdot)$ then implies either that $\{s_i''\}$ is not an equilibrium or that $\{s_i'\}$ is not an equilibrium. This contradiction proves the assertion.

14. *Claim 7 of the Proposition.* Suppose $E[\mu] < z_{i^*}$ and let $s_i \in (0, \infty)$ for some $1 < i < i^*$ at equilibrium. Fix the equilibrium strategies $\{s_{-i}\}$ and suppose they are such that $s_{i-1} = s_i$. Then, $\partial W_i(\cdot) / \partial s_i = \partial W_{i-1}(\cdot) / \partial s_{i-1} = 0$. From section 10,

$$\partial |\Delta(s_i | \cdot)| / \partial s_i = \partial |\Delta(s_{i-1} | \cdot)| / \partial s_{i-1}$$

and

$$\partial \text{var}(\cdot | s_i) / \partial s_i = \partial \text{var}(\cdot | s_{i-1}) / \partial s_{i-1}$$

But $z_{i-1} < z_i$ implies $|\alpha_i(\{s_j\})| < |\alpha_{i-1}(\{s_j\})|$. Since $E[\mu] < z_{i^*}$, then, we must have $\partial w_{i-1}(\cdot) / \partial s_{i-1} > 0$, a contradiction. The claim now follows.

15. *The Relative Location of $E[\mu]$ and z_{i^*} .* The sign of $(z_{i^*} - E[\mu])$ depends on the dispersion of ideal points $\{z_i\}$ relative to the variance of the random variable. Here, by a suitable choice of units, we have normalized this variance to one. Although we have been unable to obtain a closed-form solution connecting the dispersion of ideal points and the variance of β to $\text{sgn}(z_{i^*} - E[\cdot])$, the general principles can be illustrated with an example.

Let $n = 3$ and suppose $z_2 = 0$. Also for this example, suppose that the variance of the common prior on β^* is not, as assumed earlier, equal to one but is such that the variance of the posterior used to calculate $E[\mu] = E[\xi_\mu | \beta_i = 0, \{s_j = \infty, \forall j\}]$, given m draws, is 1. Thus, we can work entirely with the standardized normal distribution, Φ ; and need consider only permutations $\pi \in \Pi$ such that individuals 1 and 3 are median in induced ideal points on going into committee. Let $\pi = \{\pi(1) = i, \pi(2) = j, \pi(3) = k\}$. Let

$p(ijk) = \Pr\{\xi_i < \xi_j < \xi_k \mid s_j = \infty, \forall j\}$ and let $f(ijk) = f(\pi \mid \cdot, s_j = \infty, \forall j)$, and so on. Then,

$$\begin{aligned} \Delta^\infty &\equiv z_2 - E[\mu] = -\{p(312) \\ &\cdot [z_1 + f(312)] + p(213) \\ &\cdot [z_1 + f(213)] + p(132) \\ &\cdot [z_3 + f(132)] + p(231) \\ &\cdot [z_3 + f(231)]\}. \end{aligned}$$

Since $z_1 < 0 < z_3$, and Φ is the standardized normal, we have

$$\begin{aligned} f(312) &= \phi(z_1)/(1 - \Phi(z_1)); \\ f(132) &= \phi(z_3)/(1 - \Phi(z_3)); \\ f(213) &= -\phi(z_1)/\Phi(z_1); \\ f(231) &= -\phi(z_3)/\Phi(z_3) \end{aligned}$$

Here, ϕ is the density function associated with the cumulant, Φ ; and, for instance, $f(312)$ is obtained by truncating Φ to the left at z_1 and calculating the mean of this truncated normal. Let $\beta(jk) \equiv (E[\beta \mid \beta_j] - E[\beta \mid \beta_k]), j, k \in N$. Then,

$$\begin{aligned} p(ijk) &= \Pr\{\xi_i < \xi_j \mid \xi_j < \xi_k\} \\ &= \Pr\{\xi_i < \xi_k \mid \xi_i < \xi_j\} \cdot \Pr\{\xi_i < \xi_j\} \\ &= \Pr\{\beta(kj) < (z_k - z_j) \mid \beta(ji) \\ &< (z_j - z_i)\} \cdot \Pr\{\beta(ji) < (z_j - z_i)\} \end{aligned}$$

Write this last term as $p(jk \mid ij) \cdot p(ij)$. Let $\phi_j \equiv \phi(z_j)$, etc. Then on substituting and collecting terms, we obtain

$$\begin{aligned} \Delta^\infty &= p(13) \cdot \{p(32 \mid 13) \\ &\cdot [(\phi_3/(1 - \Phi_3)) - z_3] \\ &- p(21 \mid 13) \cdot [\phi_1/\Phi_1] - |z_1|\} \\ &+ [1 - p(13)] \cdot \{p(12 \mid 31) \\ &\cdot [\phi_1/(1 - \Phi_1)] + |z_1| - p(23 \mid 31) \\ &\cdot [(\phi_3/\Phi_3) + z_3]\} \end{aligned}$$

Suppose $|z_1| = z_3$. Then both the terms in curly brackets, and therefore Δ^∞ , equal

zero. If $|z_1| \neq z_3$, this conclusion is not generally true. So, although there are cases in which $\Delta^\infty = 0$ when $|z_1| \neq z_3$, this equality typically does not obtain. Differentiating Δ^∞ with respect to z_3 (without loss of generality) and evaluating this derivative at any point at which $|z_1| = z_3$ gives

$$\begin{aligned} \partial \Delta^\infty / \partial z_3 &= p(13) \cdot p(32 \mid 13) \\ &\cdot \{((1 - \Phi_3)\phi_3' + \phi_3^2)/(1 - \Phi_3)^2 - 1\} \\ &- [1 - p(13)] \cdot p(23 \mid 31) \\ &\cdot \{(\Phi_3\phi_3' - \phi_3^2)/\Phi_3^2 + 1\} \end{aligned}$$

A sufficient condition for this derivative to be strictly negative is for the first term in curly brackets to be negative and the second to be positive (at least one of these being strict). Combining these two conditions and collecting terms gives (where the subscript 3 is dropped)

$$\begin{aligned} (*) \quad [\phi^2 - \Phi^2]/\Phi &\leq \phi' \\ &\leq [(1 - \Phi)^2]/(1 - \Phi) \end{aligned}$$

Since ϕ is the pdf for the standardized normal, $\phi'(x) = -x \cdot \phi(x)$. Substituting and rearranging in (*) gives

$$\begin{aligned} (**) \quad [\phi/(1 - \Phi)] - [(1 - \Phi)/\phi] \\ \leq z_3 \leq [\Phi/\phi] - [\phi/\Phi] \end{aligned}$$

It is straightforward to check that the LHS of the first—and the RHS of the second—inequality goes to plus-infinity as z_3 goes to infinity. Numerical calculations show, however, that if z_3 (and $|z_1|$) are within (approx.) 3 standard deviations of $z_2 = 0$, then both inequalities hold and we therefore have $\partial \Delta^\infty / \partial z_3 < 0$ —in other words, $E[\mu]$ moves positively with z_3 .

Finally, suppose $|z_1|$ is "small" relative to z_3 , assumed "large," so that $p(32 \mid 13) \approx p(31) \approx 0$. In this case, $\Delta^\infty \approx -p(21 \mid 13) \cdot [(\phi_1/\Phi_1) - |z_1|]$. And this is strictly positive if and only if $[\cdot] < 0$. This condition holds for all $|z_1| > x^*$, where x^* is the unique positive solution to

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$\phi(x) = \Phi(x) \cdot x$. The solution to this equation is $x^* \approx .5$.

Putting the above observations together, we conclude (1) that if the distribution of ideal points $\{z_i\}$ is symmetric about the median, then $E[\mu] = z_{i^*}$; and (2) that there exist nonsymmetric distributions for which (a) $E[\mu] = z_{i^*}$, (b) the relative extremists in outcome space have incentives to conceal information, and (c) the relative moderates in outcome space have incentives to conceal information.

Notes

This essay has benefited greatly from conversations with Patricia C. O'Brien. Austen-Smith's research was supported by the National Science Foundation, SES-8600965.

1. Nor, for that matter, is it clear what he or she ought to do. If the long-run advantage of the individual sometimes depends on revealing information and sometimes on concealing it, then simple moral precepts, like "always reveal," are certainly unjust to those whose advantage is to conceal.

2. We use the word *coherent* to describe this particular equilibrium outcome because the invariance assures us that statutory prescriptions are as closely aligned with legislators' goals as is technically feasible (given the information available to the committee members as a group). In the absence of full revelation, legislators may prescribe actions inappropriate to their goals (e.g., prescribing a minimum wage too low to bring about any redistribution, though the prescription implies an intent to redistribute) or that are inconsistent with their goals (e.g., prescribing a minimum wage so high that it induces high levels of unemployment among unskilled workers, whom minimum wages are presumably intended to help). Full revelation, on the other hand, precludes such inappropriate or inconsistent policies—at least, on informational grounds per se. In that sense, therefore, full revelation and the consequent invariance over repeated readings assures a practical kind of coherence, while incomplete revelation potentially admits inappropriate and inconsistent outcomes. (Strictly, as will be made clear in the model section, our formal definition of *coherence* does not demand full revelation per se—only that the outcome be *as if* there were full revelation.)

3. In this example nature is allowed to add negative amounts of fluoride. However, this is simply an expository convenience and has no bearing on subsequent analysis.

4. Assuming that all legislators' sample sizes, m ,

are the same means that there are no "experts." So long as individual sample sizes are common knowledge, this assumption can be relaxed and all of our qualitative results (with one exception, which we identify in the text later) go through. (The details of the analytics, however, are considerably messier.) On the other hand, the implications of allowing individual sample sizes to vary and to constitute private information are not clear.

5. See Jacobs 1985 for recurrent examples of such phenomena in the debate over public policy towards the elderly.

6. See n. 2.

7. The qualification *essentially* refers to the fact that while there may exist several equilibrium sets of debate strategies, the equilibrium expected payoff to any legislator is the same in each case. See Appendix, sec. 13.

8. See n. 4.

9. There is also "behind-closed-doors" negotiation; but this can be subsumed within the debate category by permitting precommitment and cooperation among legislators.

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Forthcoming in December

The following articles, controversies, and research notes have been tentatively scheduled for publication in the December 1987 issue:

David Austen-Smith. Sophisticated Sincerity: Voting over Endogenous Agendas. A Research Note.

Donna Bahry and Brian D. Silver. Intimidation and the Symbolic Uses of Terror in the USSR.

Gregory A. Caldeira. Public Opinion and the U.S. Supreme Court: FDR's Court-Packing Plan.

Thomas H. Hammond and Gary J. Miller. The Core of the Constitution.

Robert Huckfeldt and John Sprague. Networks in Context: The Social Flow of Political Information.

Jon Hurwitz and Mark Peffley. How

Are Foreign Policy Attitudes Structured?

Ronald Inglehart and Scott C. Flanagan. Value Change in Industrial Societies. A Controversy.

Sadafumi Kawato. Nationalization and Partisan Realignment in Congressional Elections.

Gary King and Robert X. Browning. Democratic Representation and Partisan Bias in Congressional Elections.

Robert P. Kraynak. Tocqueville's Constitutionalism.

John Langton and Mary Dietz. Machiavelli's Paradox: Trapping or Teaching the Prince. A Controversy.

Ronald Rogowski. Political Cleavages and Changing Exposure to Trade.

Robert H. Salisbury, John P. Heinz, Edward O. Laumann, and Robert L. Nelson. Who Works with Whom? Interest Group Alliances and Opposition.

CONTROVERSIES

LIBERALISM, HUMAN RIGHTS, AND HUMAN DIGNITY

Do international standards regarding human rights require the existence of a liberal regime? This was the thrust of Rhoda Howard and Jack Donnelly's essay in the September 1986 issue of this Review. Neil Mitchell takes vigorous issue with this contention, arguing first and foremost that Howard and Donnelly have not defined liberalism satisfactorily. Howard and Donnelly present a spirited rejoinder.

In "Human Dignity, Human Rights, and Political Regimes" (this Review, September 1986), Rhoda Howard and Jack Donnelly argue that human rights require liberalism: "We contend that internationally recognized human rights require a liberal regime" (p. 802). This contention is supported by a very abstract definition of liberalism, by inappropriate comparisons, by neglecting other ideological traditions that can also claim to support human rights and the integrity of individuals, and by ignoring consideration of other conditions that may be necessary for human rights.

Howard and Donnelly's definition of liberalism is tied to no particular theorist, century, or country, making it easier to produce a set of values with an article-by-article correspondence to the Universal Declaration of Human Rights. The definition is variously defended as an "ideal type," as "a plausible standard reading of the liberal tradition," and finally with the revealing comment, "Our subject in this article is human rights, not liberalism" (p. 802). Their subject needs be both if they are to make their argument effectively. Simply stipulating a set of values and calling them liberalism and then showing that these values are compatible with a conception of human rights is not a compelling method of argument. One could as easily stipulate a floating pastiche of

"socialist" values, affiliated neither with Marx nor with Tawney, grounded on no particular piece of history or society, but which also fits human rights. Without a more careful explication of liberalism, the connection Howard and Donnelly assert raises all sorts of objections. What we end up with is a philosophical hybrid that no one will claim but that is apparently created by crossing John Locke with Edward Kennedy, perhaps in the cosy confines of a British welfare state that works, presumably after having first detached Northern Ireland.

Thus, we have emphasis on the pre-political rights of individuals that one finds in the *Two Treatises*, without the more awkward bits like the limiting of government to the protection of property. Instead we are told that liberalism means that government is required "to cancel unjustifiable market inequalities" and ensure a decent standard of life for all—so bringing it into line with Articles 22 and 25 of the universal declaration. The familiar liberal theme, going back to Locke's peculiar "title" to charity, and often espoused by Thatcher or Reagan, that these needs can be met privately is not considered by our authors. They even extend their argument to include a right to work (Article 23)—not simply the more recognizable liberal position of a chance to work—which they derive from the

liberal principle of "equal concern and respect." One wonders why they stop here—why not also derive equality of condition from this principle? They do say that "a certain amount of economic liberty is also required" to "reflect free decisions based on personal values that arise from autonomously chosen conceptions of the good life" (p. 803), so rather than derive absolute equality, perhaps we should stop at the well-known liberal slogan "to each according to his need"!

This argument that liberalism is not only fully consonant with civil and political rights but also with economic and social rights crystallizes in the contrast made between liberalism and the minimal state. The minimal state which elsewhere one sometimes hears described as "classical liberalism," is for Howard and Donnelly not liberalism because "in its very essence [it] is a violator of human rights" (p. 807). But their argument does not rest on circularity alone; the authors add to this their uncertainty as to whether minimalism is logically consistent: "Beyond minimalism's obvious incompatibility with international human rights standards . . . its deep commitment to protecting private property while denying all other economic and social rights borders on logical contradiction" (p. 807). While hedging with the word "borders," Howard and Donnelly say that unlimited accumulation (which one finds in the scholarly interpretation of the *Two Treatises* incidentally) cannot be justified in terms of "enjoyment of personal autonomy" (p. 807). For "at a certain point, additional economic resources contribute nothing at all to personal autonomy" (p. 807). Who is to determine this point without infringing on the other liberal principle that they assert of "one citizen's conception of the good life being no nobler or superior to another's" (p. 803) is not made clear, and the other grounds for unlimited accumulation—found in Locke, for example—are not considered. Further, it is

argued that the minimal state is "self-destructing if it recognizes equal, universal civil and political rights" (p. 807). People will use their political rights to destroy minimalism and institute welfare states. But, of course, there is no general requirement that I am aware of that value systems, traditions, or regimes be internally consistent; they may even contain within them the "seeds of their own destruction." Howard and Donnelly hold the conviction that Sir Isaiah Berlin warns against in "Two Concepts of Liberty," namely, "that all the positive values in which men have believed must, in the end, be compatible, and perhaps even entail one another" (1970, 167). In any case the ideal of a minimal state is electorally popular at the moment, as the elections and reelections of Thatcher and Reagan illustrate. The point is that the minimal state is not compatible with some articles of the universal declaration, as Howard and Donnelly recognize, but their claim, which is presumably essential to their thesis, that it is somehow a less "pure" form of liberalism is not convincing, given the arguments presented. Without clearly specifying the origins of liberalism, then, one is left with the suspicion that the method of constructing the "plausible standard reading of the liberal tradition" is to first read the universal declaration and then pick, choose, derive, and discard as necessary.

The authors contrast liberalism with what they call *communitarian* regimes "that give ideological and practical priority to the community . . . over the individual" (p. 808). One type of communitarian regime is communism. Their abstract, or "pure," definition of liberalism is inappropriately compared to a concept of communism based on "the structure and official ideology of contemporary communist societies" (p. 809). Not surprisingly, communism is found to fall short of human-rights standards—as would liberalism if it was constituted in a

similar way by, say, the McCarran-Walter Act, the Diplock courts of Northern Ireland, and so on.

In communist societies, we are told, "one is equal not by birth or by nature, but only to the extent that one is essentially indistinguishable from one's fellow communist citizens" (p. 810). Communism, then for Howard and Donnelly, is Orwell's satire or "the dark night of totalitarianism in which all cats are grey." They go on to claim that communism is even incompatible with economic and social rights because these rights are "contingent on the discharge of social duties" (p. 810). This argument seems to forget that civil and political rights, even in liberal *theory* (let alone practice), are contingent. With John Stuart Mill, freedom of opinion is contingent on not harming others or not instigating "some mischievous act"—corn dealers starve the poor. With John Locke, toleration is contingent on loyalty (not to be extended to Catholics, for example, in his day). And in practice, of course, foreign "communists" are denied entry to the United States, and domestic communists are off and on denied free speech for national-security reasons. In short, the good of the community in liberalism also takes precedence over individual rights. What makes Howard and Donnelly's argument here extraordinary is that the penultimate article of the Universal Declaration of Human Rights, which they say they "accept without argument," explicitly recognizes the importance of "discharging social duties" and the contingency of individual rights on these duties, despite their claim to the contrary (p. 806).

Now to move from what is included to what is omitted; Howard and Donnelly, in asserting that human rights require a particular type of liberal regime, ignore other traditions and value systems that condemn torture and political repression and provide for economic and social security. Most obvious, of course, is

Marxism, not as caricatured in the Soviet constitution or Soviet practice but that form of Marxism that provides the analysis of the Paris Commune or looks forward to "an association, in which the free development of each is the condition for the free development of all." We could also look at the socialism of R. H. Tawney, George Orwell, Bernard Crick, or the Swedish Social Democrats for that matter. Liberalism, then, even when it is defined as it is in this article, does not have exclusive claim to values that support human rights. There are alternative traditions and regimes, and thus liberalism cannot be said to be a "requirement" for human rights.

Finally, one is left puzzled by the wider purpose or use of the argument of Howard and Donnelly. It is remarkably barren in terms of producing empirical expectations about the variation in human-rights violations cross-nationally, except for the unexciting proposition that liberal societies will do better than other societies of the world, which, right-wing or left-wing, communist or corporatist, presumably do more or less equally badly. At least with Jeane Kirkpatrick, whose work develops some similar themes, we are provided with distinctions in terms of human rights performance in "non-democratic" countries that are empirically testable. And while the comparatively good performance—though only in terms of civil and political rights—of liberal regimes is plausibly attributed to values, no consideration is given to alternative explanations or other conditions, like the presence of a market economy or the generally superior material well-being that one also finds in these societies. Nor is any explanation provided for why human-rights violations still occur within these societies.¹ Is it the result of the inadequate diffusion of liberal values? But we are never even told who should hold these values: the people, policymakers, or both. Nor are we told why these societies,

in their foreign policies, can consistently support hideous client regimes if there is such a "deep and essential theoretical connection" between human rights and liberalism. Whatever the article's purpose, the reason for this comment is the worry that in such an important and politically sensitive area this article, perhaps inadvertently, contributes to what our profession should be dedicated to fight—ethnocentrism.

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Neil Mitchell's major concern appears to be our definition of liberalism, particularly that a liberal regime in our conception looks like a European welfare state rather than the world of Locke, or the robber barons, or the United States in 1987. This results, he claims, from the fact that it is "tied to no particular theorist, century, or country." Even more seriously, he charges that it "is a philosophical hybrid that no one will claim" and that his "suspicion" is that our procedure was "to first read the universal declaration and then pick, choose, derive, and discard as necessary."

In "Human Dignity, Human Rights, and Political Regimes," we directly and explicitly (pp. 802-3) tie our conception of liberalism to the work of Ronald Dworkin, which stands firmly within a well-recognized theoretical tradition running back to Locke, through Paine. The implicit empirical referent for our ideal type is the modern liberal-democratic welfare state, particularly in its European social-democratic form. We connect the socialism of Tawney, Orwell, Crick, and the Swedish Social Democrats with liberal social democracy. In other words, we explicitly argue for both the theoretical cogency and historical pedigree of our conception of liberalism.

Of course, liberalism thus ends up look-

ing more like a world of "to each according to his need" than Engels's account of early industrial Manchester. This has been precisely the direction of development of the liberal tradition, both in theory and in practice, over the last century. Our ideal type was intended to fit this real and concretely embodied tradition, rather than to maintain an anachronistic fidelity to Locke, as Mitchell would apparently have us do. We see no reason why liberals must accept every opinion of Locke—for example, non-toleration of Catholics—as dogma. Locke was certainly an important source of the liberal tradition, but he did not engrave the complete tenets of liberalism in stone. We can see no reason why liberals should be required to wear the cement boots of a three-hundred-year-old book of 243 numbered paragraphs. Mitchell's reference to Mill, moreover, seems a non sequitur; we can recall no defense of natural or human rights in any of Mill's major political or moral works.

It is true that we provided a country referent for communist states, namely Soviet-bloc countries, but not for liberal. We agree that we ought to have provided such a referent, as Mitchell appears, especially in his statement that "these [liberal] societies in their foreign policies can consistently support hideous client regimes," to think that our implicit referent is the United States. It is not. We are perfectly prepared to entertain the hypothesis that the United States more closely approximates a minimal than a liberal state (and not merely because minimalism is currently "electorally popular" under Reagan). A society that has the material capacity to provide universal health care or universal maternal benefits but nevertheless chooses not to does not strike us as one based on equal concern and respect for all its citizens (see Goldstein, N.d.). The U.S. government's lack of respect for its citizens can perhaps explain why it supports hideous client

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regimes such as Chile and Guatemala, whereas Norway, for example, extends its foreign aid to such countries as Mozambique and Nicaragua (Skalnes and Egeland 1986).

Thus, we proposed an ideal type of rights-protecting regime, which resembles European social democracy and which we called *liberalism*. But suppose we are wrong and this regime should not be called *liberal*. Let's call it *x* instead. Our argument remains the same: international human-rights standards require an *x* regime.

Mitchell professes to be "puzzled by the wider purpose or use of this article." This is perhaps because he entirely misses its major, and quite explicit, analytical purpose, namely, to distinguish between societies based on human rights and societies with alternative—non-rights-based—conceptions of human dignity. We do not argue that the only valuable way to preserve human dignity is through human rights; we merely argue that it is one way, and a particularly useful one in a world of state societies (p. 803). Mitchell could have challenged our definition of human rights as "*entitlements* that ground particularly powerful *claims* against the state, that each person has *simply as a human being*" (p. 802). Had he rejected our definition of rights and proposed another, he might have been able to show that *x* is not the only rights-protecting regime and substantiated his claim that there are "alternative traditions and regimes" that support human rights. But as he does not make any such argument, we can find no reason to alter our views.

Mitchell also claims that we are guilty of "inappropriate comparisons," particularly in our account of communism, which we based on contemporary theoretical works from, as well as observed practice in, Soviet-bloc countries. For neither liberalism nor communism do we rely on the original classics, neither Locke nor Marx, as Mitchell appears to wish us

to. An ideal type of communist regime derived from Marx, rather than from Lenin or via Soviet practice, either is likely to look very much like our social democratic liberalism, or will have few or no empirical referents. We should also note that while Mitchell may find it unsurprising that communism à la the Soviet Union falls short of international human-rights standards, such a view is very controversial, as several works cited in our article indicate. Furthermore, we want to stress that Soviet human-rights shortcomings do not arise merely because that country has deviated from the "true" path of socialism. Theoretical concepts such as the dictatorship of the proletariat demand denial of human rights.

Mitchell finds particularly "curious" our argument that economic benefits in communist regimes are not enjoyed as human rights. He appears to believe that having the substance of a right, for example, having access to health care, is synonymous with having a claim against the state to provide it. We agree that having the substance is better than nothing at all, but it is a privilege (subject to arbitrary state removal without any kind of redress, even in principle or law) rather than a right. In communist regimes, all rights are contingent grants of the state, rather than entitlements one has simply as a human being. This difference, enshrined in practice and even in the Soviet constitution, is of monumental theoretical and empirical importance. Mitchell claims however that Article 29 of the universal declaration supports the Soviet constitution by stating that the enjoyment of human rights is contingent on the discharge of social duties. It does not; rather it holds that "everyone has duties to the community in which alone the free and full development of his personality is possible," and that the *only* legitimate reason to limit an individual's rights and freedoms is to secure "due recognition and respect for the rights and freedoms of

others." While in the Soviet constitution rights are contingent on the fulfillment of duties, no such contingency is provided in the universal declaration.

There are also three minor points to which we would like to respond. First, Mitchell takes issue with our description of minimalism as internally inconsistent. We readily agree that a political regime can quite comfortably adhere to an inconsistent set of political principles. But we do believe that a fundamental internal inconsistency (and one that reflects a clear class bias) is enough to remove minimalism from consideration as a plausible regime for those concerned with human rights.

Secondly, he claims that we do not clearly specify the origin of liberalism. But we did state as briefly as we could the generally accepted proposition (with which we expect he would agree) that liberalism arose with the creation of modern capitalist society, especially with the rise of the bourgeois class (p. 804). We agree that we did not explain, in this particular short article, why liberal regimes still violate so many rights.

Thirdly, Mitchell claims that our article is "remarkably barren in terms of producing empirical expectations about the variations of human-rights violations cross-nationally," and he states that we "presumably" believe that all nonliberal regimes perform "more or less equally badly." This is simply wrong. In the last half of our article, we explicitly differentiate among various nonliberal regimes. We do, in fact, provide some empirical expectations about human-rights performances. We think that traditional, communist, and perhaps developmental, regimes are more likely to provide for the substance of social and economic rights than minimalist or corporatist ones; and we also think that communist and developmentalist regimes are more likely to value equality than minimalist, traditional, or corporatist societies. From a moral point of view, we think equality is

better than hierarchy, and the substance of social and economic rights is better than nothing at all, even if the regime holding these ideals is not rights based. Thus were we, for example, to be advising a U.S. president on foreign policy in Central America, we would advise supporting Nicaragua over Guatemala or El Salvador, in contrast, for example, to Jeane Kirkpatrick's (1979) minimalist preferences.

Finally, there is the charge of ethnocentrism, which Mitchell puts in a rather condescending fashion. As it comes out of the blue without any explanation, we can only ask, What is ethnocentric about taking a standard that is reflected legally in a universal document to which practically all states now pay at least lip service and that applies to everybody in an entirely universalistic fashion? Certainly, Mitchell does not appear to want to argue that any of the regimes we criticize respects human rights. Nor do we imagine that he wants to argue that simply because someone lives in a Third World Nation his or her government should be held to lower standards.

To insist on the universal relevance of international human-rights standards is not ethnocentrism. Rather, it is to insist on the liberating power, in a world of state societies, of the idea of human rights. And it is to refuse to agree that anything goes, that whatever a dictator or ruling class (of whatever ideological stripe) calls respect for human rights is such respect. Like so many others, Mitchell, out of a misplaced fear of ethnocentrism, would leave us unable to charge tyrants, despots, and oppressive ruling classes with violating human rights. A central objective of our article was to help lay to rest this all-too-familiar erosion of the idea of human rights.

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Notes

Neil Mitchell thanks James McCormick of Iowa State University and Karen Remmer of the University of New Mexico for their comments.

1. For Justice Thurgood Marshall, liberal documents and regimes seem to provide scant protection from repression. He said in 1978, "During most of the past two hundred years, the Constitution as interpreted by this court did not prohibit the most ingenious and pervasive forms of discrimination against the Negro" (McCluskey, 1986).

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WHY ARE CONGRESSIONAL COMMITTEES POWERFUL?

In "The Institutional Foundations of Committee Power" (this Review, March 1987) Kenneth Shepsle and Barry Weingast made the case that congressional committees are powerful not so much because of members' deference to them as because of the committees' ex post veto, a potential negative committees might deliver, say, at the conference committee stage of lawmaking. But Keith Krehbiel argues that congressional committees have, in fact, never possessed an uncircumventable ex post veto and are very much constrained by their parent chambers. In response, Shepsle and Weingast defend their model of the foundations of committee power.

In their essay on sequential decision making, Shepsle and Weingast (1987) propose that standing committees are disproportionately influential on legislative outcomes because they possess an "ex post veto" in the penultimate, conference stage of the process. I do not dispute the general observations that sequence plays a subtle but central role in legislatures and that committees have some disproportionate power attributable to "institutional foundations" (i.e., formal rules and precedents). Rather, I focus on the specific institutional constraints assumed in the ex post-veto model, question whether they are binding on members of the U.S. House of Representatives, and argue that several alternative institutions in Congress erode committee power.

Institutional Foundations and Institutional Alternatives

The ex post-veto argument is based on a four-stage, spatial model. First, a standing committee reports a bill. Second, the parent chamber considers the bill under

an open rule. Third, the standing committee, as one of two actors in a conference committee, either negotiates "ex post adjustments" in the bill or unilaterally exercises an "ex post veto" that maintains the status quo. Finally, the parent chamber considers the conference report with no opportunity to amend it or vote separately on its parts.

Shepsle and Weingast's principal substantive claim is that committee power is predominantly attributable to "institutional foundations" rather than to "deference." Members on the floor only appear to defer to the committee, but their decision not to "roll" the committee is actually determined by their precise expectations about conference procedures. The corresponding formal argument thus rests crucially on assumptions about institutional constraints in the last two stages of decision making. The key assumption about the final stage is that the parent body takes a single "up-or-down" vote on the conference report. The key assumptions about the penultimate stage are that a conference is appointed, that it is "dominated" by standing-committee members, and, most importantly, that its ex post-veto and ex post-adjustment

powers cannot be circumvented or overridden.

The claim that these are the "institutional foundations of committee power" requires qualification in light of several related House rules and precedents. For example, the parent chamber may choose to employ any of the following procedures to circumvent or override unwanted exercises of committee power:

1. In the absence of the majority vote or unanimous consent required to send a bill to conference, the House uses a process of "amendments between the houses" (Bach 1984).¹ The House and Senate may directly report amended legislation to the other chamber, thereby avoiding conference entirely. Or they may use this procedure before and/or after conference. For example, major postconference differences were resolved via amendments between the houses on the fiscal-1986 omnibus reconciliation bill (Bach 1986a) and on the \$576-billion fiscal 1987 omnibus appropriations bill (*Congressional Quarterly Weekly Report*, October 18, 1986, 2584-87).

2. A majority can circumvent an anticipated ex post veto by accepting the other chamber's version of a bill. Then conference is not possible.

3. Similarly, an anticipated ex post veto triggered by a single provision of a bill may be circumvented by accepting, unamended, the other chamber's version of that provision, thus rendering that portion of the bill "unconferencable." A recent case in point is the Senate's action on a military-construction appropriation bill in which controversial aid to contras was included. In spite of significant opposition to the contra provision, "supporters fought back all attempts to amend the measure. They want[ed] the Senate to approve the same bill that passed the House, in order to minimize the possibility of

last-minute delays that could stop the aid package from becoming law this year" (*New York Times*, August 14, 1986, 14).

4. The prospects for an ex post veto may also be constrained by Rule X, clause 6(f), which states, "In appointing members to conference committees the Speaker shall appoint no less than a majority of members who generally supported the House position as determined by the Speaker. The Speaker shall name members who are primarily responsible for the legislation and shall, to the fullest extent feasible, include the principal proponents of the major provisions of the bill as it passed the House."²

5. A majority can override a suspected veto by invoking Rule XXVIII, clause 1(b) to discharge the conference committee. This rule states, "After House conferees on any bill or resolution in conference between the House and Senate shall have been appointed for twenty calendar days and shall have failed to make a report, it is hereby declared to be a motion of the highest privilege to move to discharge said House conferees and to appoint new conferees, or to instruct said House conferees. . . ." The rationale for the rule is clearly stated by the former House parliamentarian: "The purpose of Rule XXVIII, clause 1(b) is to permit the House to retain some control over the deliberations of its agents in conference where they have failed to reach an agreement within the designated period" (Deschler 1982, 33, Sec. 11.2).

6. A majority can also override unwanted "ex post adjustments" by passing a motion to recommit a conference report, with or without instructions. Two qualifications are that conferees do not have to comply with the instructions and that the motion must precede Senate action on the report; otherwise the con-

ference is officially dissolved (Deschler 1982, 33, Sec. 26.1). Recommittal is not necessarily ineffective, however. When the 67th Congress considered the Fordney-McCumber Tariff Bill, for example, majorities in both houses rolled their money committees by striking a provision for a dye embargo. Conferees from Finance and Ways and Means then flouted their parent bodies by restoring the provision. But when the conference report came to the floor of the House, a motion to recommit the report with instructions to strike the provision was passed, 177 to 130 (Chiu 1928, 96). The bill ultimately passed, too—without the dye embargo provision (*Congressional Record* Sept. 15, 1922, 12709-18).

7. A final and admittedly unusual example of the general absence of a certain end to the bicameral game is that a two-thirds majority can suspend the rules to negate a conference report—even one that had passed both houses. This was the effect of House Concurrent Resolution 527 in the 94th Congress (Deschler 1982, 33, Sec. 26.11).

In striking contrast to the ex post-veto model in which the parent chamber is *constrained* by the stylized conference procedure, these rules and precedents indicate that the parent chamber *constrains* postfloor decision making. Combined with the parent chamber's right to choose procedures for resolving differences, the institutional alternatives raise questions about whether the postulated constraints in the ex post-veto model are binding in the House.

Use of Institutional Alternatives

Perhaps high costs of employing alternative procedures render the constraints in the model binding de facto if not de jure. If so, then the ex post veto may be a source of committee power even in the

presence of institutional alternatives. Although these costs are not directly observable, use of the procedures is. Table 1 illustrates the possible inferences. In the top row, the alternatives are used often. Regardless of whether costs are low or high, they are not prohibitively high. Thus, the constraints in the ex post-veto model cannot be binding. In the bottom row the alternatives are rarely used, but two different inferences are possible. The rare-use-low-cost combination would not support the ex post-veto model. If existent at all, committee power would have to be attributed to *procedural deference* by floor majorities that choose restrictive conference procedures when, by definition, they could easily be avoided. Finally, only the rare-use-high-cost combination would support the conjecture that costliness of use of institutional alternatives regularly constrains the parent chamber.

Unobservability of costs precludes distinguishing between columns, but data on use can distinguish between rows. Table 2 presents evidence from the 98th Congress. For over 90% of public laws, one or more of the alternative institutions was used. Over five hundred bills avoided conference entirely via amendments between the houses or outright acceptance of the other chamber's bill. Furthermore, of the 85 bills for which conference was requested, 40% did not culminate in a final "up-or-down" vote on the whole conference report. Rather, for most of the bills using a combination of procedures, the conference reported "amendments in disagreement" (see Bach 1984) that were considered apart from the conference report and resolved via amendments between the houses. Thus, "ex post adjustments" were often made by parent bodies rather than by "committee-dominated" conferences.³

The unambiguous inference from these data is that for most bills, institutional alternatives to Shepsle and Weingast's

Table 1. Are the Ex Post Veto Constraints Binding?
(Inferences from the Use of Institutional Alternatives)

Actual Use (Observable)	Cost of Use (Unobservable)	
	Low	High
Common	No	No
Rare	No: procedural deference	Yes: de facto, ex post veto

stylized conference procedure were not prohibitively costly to employ. Unfortunately, direct inferences regarding committee power are not possible even for those relatively few cases in which the stylized conference procedure was used. Was committee power exercised contrary to majority preferences in any or most of these cases? If so, did institutional foundations prohibit the parent chamber from rolling the committee, or was the parent chamber simply exercising a form of procedural deference? If the former, why in some cases but not in others?

**Institutional Erosion
of Committee Power**

The necessarily limited inference from the data in Table 2 accentuates the need for bill-specific evidence about preferences of the committee and the parent chamber and for examining the specific predictions of the ex post-veto model. For situations in which preferences

of the committee and parent chamber diverge, the model predicts that

- 1. committees are not discharged or forced to report bills under duress
- 2. committees are not rolled on the floor
- 3. conference committees are appointed and are dominated by preference outliers from the committee⁴
- 4. bills disproportionately reflect preferences of the committee

Evidence consistent with these predictions would not unequivocally support the ex post-veto model, however. It would likewise be consistent with the fundamentally different view that committees are powerful *in spite of institutional alternatives*. This interpretation would seem more plausible than one based on institutional constraints that seem not to bind. Yet notice its similarity to the "cooperative system of reciprocated deference" that Shepsle and Weingast

Table 2. Use of Institutional Alternatives During the 98th Congress

Procedures	Number of Cases ^a	Percentage
No conference	540	86.4
Combined procedures	34	5.4
Conference only	51	8.2

Source: *Calendar of the U.S. House of Representatives*, final edition, 98th Congress.
^aNumber of cases = 625 (623 public laws, 2 concurrent budget resolutions).

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regard as more of a description than an explanation.

Although empirical evidence cannot straightforwardly distinguish between institutional foundations and procedural deference, an alternative multistage model can be examined that yields predictions different from those of the ex post-veto model and the deference description. Consistent with the complete set of House rules and precedents, such a model permits members to choose not only policies but also procedures. Ultimately, it suggests that committees are *less* powerful than is commonly presumed precisely *because of institutional alternatives* to Shepsle and Weingast's conference procedure.

Suppose a majority on the floor wishes to roll the standing committee. Rather than passing and perfunctorily sending a bill to a "committee-dominated" conference that may exercise an ex post veto or ex post adjustments, the majority does any of the following:

1. It may roll the committee via outright acceptance of the Senate's bill.

2. It can either designate a member to object to the unanimous-consent request to go to conference or vote no on the motion to go to conference, thereby sending the bill directly to the Senate.

3. It can consent to conference but appeal to clause 6(f) of Rule 10 to encourage the Speaker to "include the principal proponents of the major provisions of the bill as it passed the House."

4. Finally, if a conference is appointed but does not comply with majority wishes, the majority can (in various combinations) discharge conferees, recommit the conference report, appoint a new conference, or revert to amendments between the houses.

The general point should be evident. Each strategy circumvents the ex post veto and undesired ex post adjustments

and yields an outcome that erodes committee power. More precisely, this model of institutional choice, in competition with the ex post-veto model, predicts that

1. committees may be discharged (or compelled to report bills against their wishes)

2. when committees are discharged (or report bills out of duress), they will get rolled if a floor majority opposes the bill as discharged or reported

3. when committees are rolled, the parent chamber will either circumvent conference or carefully monitor conference behavior

4. if conferees nevertheless attempt an ex post veto or undesired ex post adjustments, alternative institutions will be used to override the veto or adjustments.

An extensive empirical test of the predictions is beyond the scope of this comment, but examples supportive of rational choice of institutions are readily available. Gun control legislation in the 99th Congress indicates that the institutional alternatives are used to circumvent the ex post veto and to roll standing committees.

On July 9, 1985, the Senate passed S. 49, a bill to weaken the 1968 Gun Control Act. The vote was 79 to 15. The bill was sent to the House, where it was referred to the Judiciary Committee. To the chagrin, but not the surprise, of the National Rifle Association (NRA), the Committee's chairman, Peter Rodino (D-NJ), pronounced the bill "dead on arrival" (*Congressional Quarterly Weekly Report*, March 15, 1986, 599). Many members in the parent chamber demanded relaxation of the gun controls, however. Harold Volkmer (D-MO) circulated a discharge petition to bring gun-control legislation to the floor without committee action, and it soon became apparent that the necessary 218 signatures would be obtained. Judiciary's Subcommittee on Crime thus per-

formed an "extremely fast" markup of H.R. 4305, which became H.R. 4332 and was passed in the full committee on March 11. Although Volkmer's petition secured its 218th signature two days later, the relevant committees "beat the clock" (*Congressional Quarterly Weekly Report*, March 8, 1986, 548).

On March 19, the Rules Committee met to reconcile Volkmer's anticipated discharge with the Judiciary Committee's attempt to preempt it. The product of its hearings and deliberations was H.Res. 403, a rule that allowed Volkmer to substitute the text of his bill, a close approximation to S. 49, for the Committee's bill, H.R. 4332. After brief floor consideration of H.R. 4332 on March 20, the House resumed debate on gun-control legislation on April 9 and 10. Because the signatures had convinced members that Volkmer could pass his substitute, the vehicle for amendments was the substitute rather than the committee's bill. Subcommittee Chairman William Hughes (D-NJ) circulated a "Dear Colleague" letter on March 21 urging members to support a set of "law-enforcement" amendments to the Volkmer substitute, including provisions to bar the interstate sale and transportation of handguns, to expand the category of people who must get licenses to buy and sell guns, and to allow federal agents to make unannounced inspections of gun dealers' premises. Each of these would dilute Volkmer's proposed reductions in gun control. Relative to the 1968 law, however, the Hughes amendments would still diminish gun control.

Hughes managed the bill on the floor, and his amendments consumed almost all of the allotted five hours. He was uniformly unsuccessful in his major attempts to dilute Volkmer's substitute. His first and most significant failure was the loss of his package of law enforcement amendments by 72 votes. Second, his proposed ban only on interstate transportation of handguns failed, 177 to 242. Although his

amendment to retain only the existing ban on interstate sale on handguns was adopted, it was regarded as a minor victory. For example, NRA legislative director Wayne LaPierre said the NRA "could live with the handgun ban because it simply maintained the status quo" (*Congressional Quarterly Weekly Report*, April 12, 1986, 784).

In the closing moments of debate, Hughes persisted in his efforts to dilute the bill. His several attempts to gain additional time via unanimous consent received objections by Volkmer proponents, who were content with the minor changes in the substitute (*Congressional Record*, May 6, 1986, H1750-52). Thus, the Committee of the Whole proceeded to pass the Volkmer substitute, 286 to 136. After the committee rose, the bill as amended passed 292 to 130. Finally, in accordance with the "hook-up" provision in the rule, a motion was made to strike the text of S. 49 and substitute the amended contents of H.R. 4332. The motion was agreed to. Then, with no discussion of a possible conference, the bill was returned to the Senate. In the ensuing off-the-floor, cross-chamber negotiations, neither of the key House actors—Volkmer and John Dingell (D-MI)—were Judiciary Committee members (*Congressional Quarterly Weekly Report*, May 10, 1986, 1034-35; *Congressional Record*, May 6, 1986, S5358-68). Finally, on May 6, the "Senate concurred in the amendments of the House to S. 49" (*Congressional Record*, May 6, 1986, D529).

Given the revealed preferences of House members on the gun-control issue, the ex post-veto model predicts different behavior at every stage than that which occurred. At stage one, Volkmer and his colleagues would not have attempted the discharge had they regarded the ex post veto as a binding constraint. Furthermore, a committee armed with a credible ex post veto would have "opened its gates" without reluctance, whereas in the

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ex post-veto model the committee is guaranteed an outcome at least as good as the status quo. At stage two (given that the gates were opened), the ex post-veto model predicts that in anticipation of a veto, Volkmer proponents would sophisticatedly *oppose*, or not offer, the Volkmer substitute, in order to give the committee a bill it *would* support in conference. Or, for the same reason, proponents should have supported all of Hughes's weakening amendments. At stage three (given that the roll occurred), a conference committee "dominated" by preference outliers would have been perfunctorily appointed and, of course, would have exercised the ex post veto.

Instead, the subcommittee was rolled at each stage: first via discharge, second via amendments on the floor,⁵ and finally via amendments between the houses. The likely reason for the third and most relevant roll is that gun-control opponents anticipated the possibility of an ex post veto but worked around it due to a readily available institutional alternative. This interpretation is supported by the following observation: "Privately, NRA officials and some of their House supporters say they are hoping the House will pass a bill so close to S. 49 that a conference with the Senate will not be necessary. They are concerned, despite Rodino's pledge to go to conference, that the two chambers could get stalled in disagreement and the gun legislation would die" (*Congressional Quarterly Weekly Report*, March 15, 1986, 598). In sum, the parent chamber's choice of institutional alternatives can erode committee power.

Conclusion

The three specific aims of this comment were to highlight institutional alternatives to Shepsle and Weingast's conference procedure, to present evidence that their use in the U.S. House is frequent and may

render the key constraints of the ex post-veto model nonbinding, and to illustrate that their use can erode committee power. The analysis supports the conclusion that the ex post veto is not a significant institutional foundation of congressional committee power.⁶

The restricted applicability of the ex post-veto model and its corresponding underestimate of *parent chamber* power raises challenging questions regarding the fundamental tension between standing committees and floor majorities in Congress. How do standing rules and precedents moderate this tension? Why were they adopted? How did they evolve? Existing models do not accommodate these questions satisfactorily because of their disproportionate focus on the institutional weapons in the committee's arsenal (e.g., obstruction, restrictive rules, and the ex post veto) at the expense of parallel mechanisms at the disposal of floor majorities (e.g., the discharge petition, open rules, and amendments between the houses). Shepsle and Weingast offer a promising set of tools and insights for further development of models that incorporate a more balanced set of institutional alternatives and are potentially amenable to empirical tests. Regardless of the implications of future studies for the ex post-veto argument in particular, the general effect of such studies will be to clarify the foundations of committee *and* *parent chamber* power and, in turn, to facilitate an understanding of the more subtle committee-floor tension that pervades congressional politics. Thus, while Shepsle and Weingast's "puzzle of committee power" persists, several of its key pieces have been identified.

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Krehbiel has provided a thoughtful,

provocative comment on our recent paper on committee power. In putting forth an alternative point of view, providing some data, and reporting on a particular case in considerable detail, Krehbiel claims that his analysis "supports the conclusion that the *ex post veto* is not a significant institutional foundation of congressional committee power." Although we believe there are several important facets of his argument that need to be incorporated into future work on this subject, the main thrust of his argument and the conclusion he derives from it are, in our opinion, misplaced.

Since Krehbiel's critique goes to the heart of our original essay, we first briefly summarize our argument. Second, we try to place our work in theoretical context and suggest how second-generation modeling might proceed. Third, we take a more detailed look at the frequency distribution that constitutes the empirical foundation for Krehbiel's critique. Finally, we suggest that the real value of our approach lies in the comparative-statics predictions it permits.

The Role of Sequence

We began our original essay with the observation that many students of congressional committees "explain" committee power by naming some of its agenda-setting concomitants (e.g., gatekeeping, proposal power). We questioned whether "naming" and "explaining" were the same thing and, more importantly, whether these named features of committee power really did confer power on committees. A theory linking named properties with influence over final outcomes was required.

In the context of a garden-variety spatial model of legislative deliberations, we demonstrated, in fact, that neither gatekeeping nor proposal power by committees, individually or jointly, could be linked to positive influence over final out-

comes. Chamber majorities had sufficient tools to open any gate or alter any proposal. Once a committee made its move, the final outcome was in the hands of floor majorities. We concluded that if gatekeeping and proposal power fully characterized committee power, then committees would not be terribly powerful and, indeed, that it would be highly unlikely to find members investing their time and energy in committee careers. In short, the effective division-of-labor system that has characterized the U.S. House committee system, from the time of the overthrow of Cannonism in 1910 until at least the 1970s, would not, in our view, have been sustained. But it was sustained for more than half a century and, although significant reforms in the mid-1970s have transformed it, the system of committees and jurisdictions remains a vital feature of legislative life in today's Congress. Something in the "explanation" was missing.

Some scholars appreciated the insufficiency of gatekeeping and proposal power for committee influence, though they did not cast their doubts in the form of an explicitly theoretical argument. Their attention focused on a normative framework of reciprocal deference. In the language of the more recent theoretical formulation of Axelrod (1981, 1984), deference to committee preferences—whether in allowing the gates to be kept closed or in exercising restraint in amending committee proposals—constituted a form of cooperation within a division-of-labor institution.

We agreed that something in the old explanation was missing, but we argued that there were reasons to be skeptical of cooperative arrangements and deference-like arguments. The unenforceability of agreements and the ever changing legislative and electoral context combined to render such arrangements vulnerable to reneging and opportunistic behavior (see Weingast and Marshall 1987 for a fuller elaboration of this theme). We focused

instead, on institutional features of the legislative process, on the sequence in which legislative business was conducted. Using simple game-theoretic reasoning, we demonstrated that a committee role *after* its chamber had "worked its will" (ex post powers) had the nonobvious effect of making earlier committee moves (ex ante powers) effective. In short, the sequential process provides the glue sustaining a system of reciprocal deference.

Ex Post Power in the U.S. Congress

In our paper we developed the ex post-power theme both in the abstract (in effect, as an extensive-form game with committees possessing moves both early and late in the play) and with the U.S. Congress in mind. We argued that the conference procedure by which differences in legislation are resolved between the chambers of a bicameral legislature constituted an institutional manifestation of ex post power and, consequently, was the "missing link" in the story of committee power. Krehbiel spends the bulk of his remarks on this latter argument, and we turn to them shortly. But it must be emphasized that first and foremost, ours is a stylized argument, in which we sought to assess claims about ex ante power, to show them wanting, and then to demonstrate the remarkable consequences of a deeper sequence in which ex post committee moves played a role. In this sense, ours is an exercise in theory, in which we identified flaws in previous arguments and elaborated an improved theory that removed those flaws.

As modelers, how might we address the issue of less-than-complete implementation of ex post powers? This is not precisely the question raised by Krehbiel, but perhaps it should have been. In our stylized legislative world, a committee is *assured* the penultimate move (say, in a

conference proceeding) and the legislative chamber is *restricted* at the ultimate move (a majority may only vote the conference report up or down). However, as Krehbiel correctly notes, chamber majorities may frustrate efforts to go to conference (by accepting the other chamber's version of a bill, by amending the other chamber's version and "messaging" it back, etc.). As well, chamber majorities are not restricted, at the final move, to an up-down vote on a conference report. They may, for example, accept a "partial" report and resolve remaining difference via some extraconference procedure. We report two responses here to the "imperfect-implementation" problem, leaving a third response (comparative statics) for special consideration.

Costs and Biases

Even if it were conceded that chamber majorities are able to attenuate ex post committee powers, it is necessary to determine under what circumstances they would actually do so. A messaging-between-the-chambers strategy without the cooperation of the originating committee, as in Krehbiel's example of the 1986 gun-control legislation, is bound to be a legislatively intricate undertaking. Undoubtedly, there are times when such costs are insufficient to deter a challenge to the committee. In even rarer circumstances, a zealous proponent of a bill mistreated by the committee can rally large numbers of legislators around the cause. However, the costliness of the undertaking is not trivial and in many situations is a deterrent. Committees will try to anticipate, and possibly accommodate, these prospects; they may also try to raise the costs of opposition. For example, we suspect that the heavy workload on the floor at the end of a Congress is not only an indication of the time it takes to produce a bill but also a way in

which committees raise the costs to others of nullifying their ex post powers. The spate of conferences that greets the closing days of a Congress is a reflection of the fact that, short of accepting the other chamber's version, the only practical way to resolve differences is via the conference procedure. In the rush to adjourn, committees (serving as conferees) are in the proverbial catbird seat. Opponents are severely disadvantaged because the opportunity cost of opposing a committee at this late stage is high.

Krehbiel believes that imperfect implementation nullifies ex post committee powers. We respond that the existence of such opportunities is insufficient to sustain his claim, that exercising such opportunities comes at a cost, and that ex post powers bias legislative results in the direction of committee preferences.

Although we do not believe the extreme conclusion Krehbiel draws is warranted, his general point (as we interpret it) is sound. The ability of chamber majorities to short-circuit ex post as well as ex ante committee powers undoubtedly has a sobering effect on committees. One effect (alluded to above) involves an adaptive response by committees in which they time matters for the end of a Congress. Another is partly rhetorical, partly reputational: committee rhetoric emphasizes its representational role in conference. Conferees portray themselves as defenders of the *chamber's* version of a bill, not the *committee's* version. And in many respects their behavior is not entirely inconsistent with their rhetorical claims, for they wish to acquire reputations that would further discourage efforts to short-circuit their ex post powers. This suggests some second-generation future directions for modeling imperfect implementation. Thus, Krehbiel is correct to call attention to these possibilities; insecure ex post powers will dampen committee incentives to extract all the advantage from a situation. But committees must still be reck-

oned with, and the ex post veto provides the institutional underpinnings of this power.

Imperfect and Incomplete Information

Our model is one of perfect and complete information. At each stage in the process, each actor knows exactly what has transpired up to that point (legislative history) and what consequences will flow from any decision made—hence *perfect* information. Moreover, at each stage each player knows the set of feasible paths the future might take, how they depend on the choices at later stages, and what the preferences of all players are—hence *complete* information. Consequently, the legislators in this model world are capable of perfect retrospection and perfect foresight. It is a world in which rational players play error free. These characteristics, though obviously lacking verisimilitude with the real world of imperfect and incomplete information, are nevertheless standard for first-generation models. Only recently have modelers in both economics and political science begun to advance beyond these stylized features. To point out, as Krehbiel does, that mistakes occur—chairmen fail to appreciate the threat of a discharge, committee bills get amended beyond recognition on the floor, ex post powers are short-circuited—does not necessarily mean that the process has been misspecified. In our opinion the mistakes to which Krehbiel alludes are indicative of information asymmetries and imperfections. To conclude from the occurrence of mistakes that ex post committee powers are insignificant, as Krehbiel does, is much like concluding that the presidential power to veto is insignificant the first time a successful veto override is observed. Properly interpreted, however, Krehbiel's point is well taken. Models of incomplete-imperfect information are natural future steps to take. We should warn the reader,

however, that models of asymmetric or imperfect information existing in the literature are extremely delicate, fragile, and specialized. Their conclusions are also subtle and nonobvious, so that while one's natural inclination might be to believe with Krehbiel that uncertainties mitigate committee powers, it would not surprise us to discover that exactly the opposite is true.

We have tried in this section to put Krehbiel's criticisms in context. Our model of ex post committee powers was a theoretical effort to salvage the view of committees as powerful agenda agents in division-of-labor legislatures. Even allowing for (costly) opportunities to short-circuit these ex post powers, imperfect information, and incomplete information, we believe our conclusion—that ex post powers bias final outcomes toward the preferences of committees—will stand.

Alternative Procedures for Resolving Bicameral Differences

At the heart of Krehbiel's critique of our argument is the existence of alternatives to the conference procedure. If, as Krehbiel suggests, there are such alternatives and if, moreover, they are used with considerable frequency, then the import of ex post committee powers is illusory and the "puzzle of committee power" remains. Krehbiel proceeds to present data drawn from the 98th Congress on the frequency with which various procedures are employed to resolve chamber differences. His data reveal that only in 13.6% of the 625 public laws and budget resolutions passed in 1983-1984 were conference procedures employed in some form. From this Krehbiel concludes that "for most bills, institutional alternatives to Shepsle and Weingast's stylized conference procedure were not prohibitively costly to employ," thus strongly

implying that floor majorities, *not committees*, were firmly in control.

We believe Krehbiel's data are misleading and his interpretations simplistic. Frequency distributions are not always what they seem and need to be interpreted in the context of a theory. Consider the parallel between ex post committee powers and the presidential veto power. A naive collector of frequency distributions might arrive at the (erroneous) conclusion that the presidential veto is unimportant. As Rohde and Simon (1985, 398) put it, "If we consider vetoed bills as a proportion of all bills passed by Congress, this characterization is supported, for only a small proportion of public bills is vetoed in any given year. Nonetheless, there are several reasons for probing beyond the conventional wisdom." The most obvious reason is that it makes no reference to the *content* of the bills that pass (and avoid the veto). Three important qualifiers emerge from just such a consideration: on some proportion of bills, Congress and president are not in disagreement; on a considerable proportion of the bills, the content is trivial, uncontroversial, or insignificant to the interest of the president; finally, the ultimate content of many bills takes into account in advance the preferences of the president.

Precisely the same reasoning may be employed to question Krehbiel's frequency distribution. Of the 86.4% of all public laws passed by the 98th Congress that avoided a conference

1. how many reflected consensus between committee and floor majorities?
2. how many were trivial, uncontroversial, or insignificant to floor majorities?
3. how many passed in a final form that reflected the wishes of committees?

This short list suggests the obvious: not all bills are the same. Comparing all the

**Table 3. Disposition by the House of Public Laws
and Resolutions of the 98th Congress
(in percentages)**

Disposition	Krehbiel	Nickels	Congressional Quarterly Key Votes ^a
No conference	86.4	88.0	13.0
No messaging		62.8	
Messaging		25.2	
Conference	13.6	12.0	87.0
No messaging	8.2	8.0	
Messaging	5.4	4.0	
Total	100.0	100.0	100.0
Number of cases	625	623	23

Sources: Krehbiel 1987; Nickels 1986; *Congressional Quarterly Almanac*, vols. 39-40, Appendix C.

^aThere were 16 key votes in each session of the 98th Congress. In both sessions two of the votes dealt with the same bill, so there were 15 distinct bills in each session. Of the 30, 7 were either defeated in the House, not taken up in the Senate, or defeated in the Senate, leaving an N of 23.

bills in one session and then noting frequencies implicitly assumes that all bills are effectively similar. Those bills over which there is little controversy—either because there is widespread agreement or because almost no one but a handful of congressmen care—may well end up in the messaging category because this is the most expedient method of reconciliation between the chambers. For major controversies over tax legislation, economic or social regulation, or important redistributive issues, however, it may not be possible to settle disputes in this manner, so the committees opt for a conference. This suggests the hypothesis that the choice of reconciliation procedures is largely (though not exclusively) a *committee* matter. This hypothesis leads to a much different interpretation of Krehbiel's data, namely that the frequencies reflect committee choices favoring simple, expedient procedures when the bill in question is not controversial and differences between the chambers negligible. The plausibility of this hypothesis means that we cannot infer from the data, as Krehbiel tries to, that end-running the

committee is both easy and frequent.

In support of our interpretation, we have gathered some data on a subset of bills from the 98th Congress, namely those that exhibit substantial controversy within each chamber. The more controversial an issue, the more likely the committee-opposition within the chamber is to attempt to short-circuit the committee's veto power, that is, to avoid a conference. If the frequency of conferences were low for these bills, then Krehbiel's contention that the *ex post* veto is easily evaded would indeed cast doubt on the ability of a committee to exercise an *ex post* veto.

Our approach is simple but direct, leaving a more systematic investigation for future work. Specifically, for the same 98th Congress from which Krehbiel's data were drawn, we examined the disposition of bills designated by the *Congressional Quarterly Almanac* as involving "key votes." We took this designation as a crude indicator of the importance of a bill to committees, floor majorities, the president, and so on.

The results are reported in Table 3. In

addition to Krehbiel's tally, we have included an independent tally for the 98th Congress by Nickels (1986) of the Congressional Research Service. Although the two tabulations employ slightly different counting conventions, they are in substantial agreement. However, Nickels breaks the numbers down a bit further. Nearly 63% of all bills not only avoid conference, they avoid *any* difference resolution whatsoever. These bills pass both houses in precisely the same form. Included in this category is an occasional "important" measure—for example, H.J. Res. 364, permitting the presence of marines in Lebanon. We suspect, however, that the overwhelming preponderance of measures in this category is of the trivial, uncontroversial, or insignificant sort. Thus, we believe Krehbiel puts far too much weight on the fact that so many bills avoid conference.

This is made especially forcefully by the *Congressional Quarterly* key-votes data. On "important" bills, the proportion going to conference is almost the inverse of the numbers reported by Krehbiel and Nickels for the population of all bills. While we have not untangled those that were resolved purely by conference, as opposed to those that entailed some messaging between the chambers, the proportions are quite impressive: nearly seven of every eight "important" bills require a resolution of differences *by conference*.⁷ Were Krehbiel's bold contentions true, we would expect to see, in this category of controversial measures, a number of cases in which a committee brings its bill to the floor and some floor majority successfully replaces the committee bill with that from the other chamber; or cases in which committees seeking to go to conference on a measure are end-run by noncommittee members who successfully initiate messaging. While there are undoubtedly instances of these, we suspect that an extensive study of controversial measures would confirm

what our examination of *Congressional Quarterly* key votes revealed, namely that these instances are rare.

Making empirical sense of the various ways the two chambers coordinate on a particular piece of legislation requires considerably more investigation than we can provide at this time. But it should be evident that the naive population frequencies reported by Krehbiel give a distorted view of the importance of the conference institution. Krehbiel's frequencies are consistent with the hypothesis that floor majorities may short-circuit a committee's ex post powers. But they are also consistent with our hypothesis that they reflect the discretionary choices made by committees about the best method of reconciling legislative differences (if there are any). Our own examination of *Congressional Quarterly* key votes gives some additional weight to this latter hypothesis.

Comparative Statics

One of the virtues of an equilibrium theory is the comparative-statics analysis it permits. This affords an opportunity to explore how changes in institutions affect behavior and outcomes. Our stylization of the committee process ignored key features of the congressional environment, and Krehbiel has raised some important instances. We believe that the way to think about these omissions is to attempt to model them as comparative-statics predictions, that is, to determine how they affect the results of our model.

The committee system of our model has, as its empirical referent, the House committee system of the 1950s and 1960s. By the mid-1970s, this system had experienced some substantial institutional changes as reform fever swept the House—recorded-teller votes, electronic voting, Subcommittee Bill of Rights, and growth in staff, among others. These reforms, in turn, affected the norms of ap-

prenticeship and specialization that were so characteristic of the earlier system. As these norms atrophied, member activities were channeled and constrained less by their committee assignments than had been the case in an earlier era. To oversimplify, the effect was a general decline in the division and specialization of labor. The theoretical issue implicitly raised by Krehbiel requires that we model these changing features to determine whether a new equilibrium has resulted, one entailing an "institutional erosion of committee power."

Recently, some fine empirical work (Bach 1986b; Smith 1986) has provided us with the first comprehensive snapshots of what these changes have wrought. As the subtitle of Smith's paper, "Why Don't We Do It on the Floor?," conveys, there has been tremendous growth in the amount of legislative activity that occurs on the floor. Hints of this began to appear even earlier in the more anecdotal literature. Bibby (1983), for example, reports of the seven freshmen from the class of 1978 who participated in an American Enterprise Institute roundtable: "There was a feeling among the group of freshmen that committees were having an increasingly difficult time defending their bills on the floor of the House. That is, the committees were being 'rolled' on the floor, and deference to committee decisions was being eroded. The freshmen felt little obligation to support committee decisions. . . . There was also a feeling of general skepticism about the work of the committees and their tendency to be special pleaders for the causes that fall under their jurisdiction" (p. 23).

From these empirical regularities, we need to determine what has happened to the equilibrium of the earlier system (in which committees were hegemonies), as that hegemony has (apparently) begun to erode. Krehbiel's examples, as well as Bach's and Smith's empirical work, tell a part of that story, namely, they show the

extent to which committee bills of the postreform era are transformed substantially by chamber majorities on the floor of the House. But there is an important part of the story they do not tell, a corollary to the law of anticipated reactions.

This "other part" of the story is best understood by considering what behavior one might expect of a committee in two different circumstances. In the first, committees expect their bills to win, perhaps because no (or only a few) amendments are allowed; in the second, committees expect a considerable number of amendments to be offered. We claim—and this is the subject of our current research—that committee proposals will differ in these two circumstances in systematic ways. With a modest ability to forecast future action on the floor, committees will anticipate and react to the differing amendment prospects accordingly. Thus, when committees expect their bills to pass without amendment, they will bring forth their most preferred alternative from among those that command the support of a majority. In this environment, if the committee loses on the floor, then there is real meaning to the notion that the committee was "rolled." However, when committees no longer expect to win (i.e., when they expect major amendments), they do not bring forth their most preferred alternative. Rather, they bring forth a *different* proposal, an instrument or vehicle they use to manage the amendment process. They care about the final (possibly amended) outcome and may or may not have any stake in the original bill they reported. We further predict that committees will participate heavily in the amendment process, in part to fend off less-desirable amendments by others (the data from Bach 1986b and Hall and Evans 1985 show that this is indeed the case). Thus, in the second environment, it is no longer clear whether we can view floor amending activity as constituting the committee being "rolled," since the com-

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mittee did not expect (or desire) its original bill to win. While committees are undoubtedly worse off under the more open procedures—"after hegemony," if you will (with apologies to Robert Keohane)—it is premature to interpret this increase in floor activity as evidence that committees have lost control of the process or that it is easy to neutralize their power.

Conclusion

We have gone on at some length expanding our view of committee power, suggesting how it might be extended to incorporate elements of the postreform Congress. In this manner, we have responded to Krehbiel's comments by suggesting that they require the appropriate comparative-statics analysis of our model. Key here are the effects of rules that foster considerably more floor activity by noncommittee members and of rules that allow noncommittee members to end-run the ex post powers of committees. In the latter case, we have argued that these procedures are costly to use, especially for bills coming up late in the session. Therefore we do not believe that these procedures effectively negate ex post committee powers, though they may qualify them in important ways.

In those circumstances where such attempts are likely to be tried, committees may well anticipate their use so that the legislation they bring forth reflects this threat, typically by slanting the measure toward the interests of those threatening to end-run. We suspect that this is the most likely effect of these procedures and that actual end runs are relatively rare. This reveals that the real empirical issues

as to the effects of these procedures may be extremely difficult to assess since it is their potential use that affects committees, not necessarily their actual use.

This type of comparative static is readily studied within our framework. The above discussion suggests that the existence of procedures that allow floor members to end-run committees under certain circumstances have predictable effects. It also suggests, however, that because they are costly, they are not always used, and hence their existence does not *negate* ex post committee powers. We reiterate our earlier claim, namely that these ex post powers form a necessary underpinning of the congressional-committee system and that without them, committees would not play the important role in the congressional process described by congressional scholars over the last thirty years.

If there is a broader message in our remarks, it is a plea for a keener appreciation of equilibrium analysis. A mature institution is, in our view, an organization in equilibrium. Changes in its environment ripple through the institution. One must appreciate the totality of these "ripples," not just the first one. In the post-reform Congress, committee products are more vulnerable on the floor. But that is not the end of the story. Committees select the vehicle to take to the floor. As well, committees (and their chairs) play an active role in arranging for bills to be transmitted to the other chamber; in seeking conferences, opting for the "messaging" approach, and making other tactical decisions in the negotiations between the chambers; in debating and moving approval of conference reports; and so on. In short, committee personnel play an active legislative-management role before, during, and after floor consideration, a role assumed by others only in unusual circumstances and at very high cost. Documenting the rest of this story is an

objective that Krehbiel's constructive comments have encouraged us to pursue.

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Notes

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1. Shepsle and Weingast summarize this procedure on pp. 19-20 and n. 11 but regard it as unimportant for major legislation. Thus, it plays no role in their model.

2. Precedent indicates that the rule may afford the Speaker considerable latitude. For example, his judgment cannot be challenged on a point of order (Bach 1984; Nagler 1986). Thus, the effect of the rule depends upon whether the Speaker is more responsive to standing committees or to majorities.

3. If an amendment in disagreement is defeated, the whole report is defeated. Shepsle and Weingast's closed-rule characterization does not follow, however, because refusal to pass an amendment in disagreement neither ends the game nor exhausts the parent chamber's strategic opportunities (see Bach 1984, 59-74 for a long list of possibilities). A common occurrence in the 98th Congress, for example, was for the House to accept such an amendment *with further amendments* and then to revert to amendments between the houses.

4. Bill-specific data on preferences of committee and noncommittee members are essential for measuring "committee dominance." In its absence, Shepsle and Weingast's main empirical finding of "dominance" (as measured by *representation* rather than *preferences*) is consistent with an alternative hypothesis that bills go to conference only when committee and floor preferences are similar.

5. On both of the failing Hughes amendments, the parent chamber opposed a 6 to 3 subcommittee majority. On the vote of the Volkmer substitute, as amended, the House opposed a 5 to 4 subcommittee majority. Only on the Hughes amendment that passed did the House and subcommittee majorities correspond.

6. Although the focus has been on the House, generalization to the Senate is justified by strong similarities between House and Senate procedures for resolving bicameral differences. Support is found in the Senate's procedural guide for conferences,

Cleaves' Manual, which consists primarily of precedents compiled for the House by its (former) parliamentarian, Asher C. Hinds. For example, all of the following are possible in the Senate: amendments between the houses before or after conference, discharge of an inactive conference, and appointment of a new conference. Sophisticated choice of institutions is thus possible in the Senate, too.

7. In regard to mixed procedures, i.e., those using conference and messaging, we believe that here, too, Krehbiel has overinterpreted the data. While we have not investigated this, many bills in this category are those to which the Senate has added a nongermane amendment. Here, the conferees may deal completely with the germane issues, leaving the nongermane ones to some form of messaging. The important point is that there should be no presumption that bills in this category represent cases in which floor majorities are getting around intransigent committees.

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RESEARCH NOTES

GUNS AND BUTTER AND GOVERNMENT POPULARITY IN BRITAIN

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Britain under the government of Prime Minister Thatcher provides a unique opportunity to probe the effects of both war and macroeconomic performance on government popularity. Monthly ratings for Thatcher and Conservative-party support (June 1979 to July 1985) are examined by way of Box-Tiao intervention models. The model that best captures the impact of the Falklands War of 1982 is of the gradual-temporary variety: popularity gains accrued through the three-month war shrink in a geometric fashion. Nevertheless, they prove to be worth over five percentage points for the Conservative party a year later, in the 1983 election. Macroeconomic performance, meanwhile, is found to have an asymmetric effect on government popularity, with unemployment strongly significant but inflation not significant at all. Apparently, the British public is punishing failure (on employment) while letting success (on inflation) go unrewarded. Qualifications of the positive effect of war and the negative effect of macroeconomic performance are suggested.

War and economics have few rivals when it comes to making or breaking governments. Many observers take for granted that prosperity spells electoral success for those in power, and bad economic times, political doom. Likewise, military victory has long been thought to augur well for the popularity of political leaders, whereas defeat may cost them more than their popular support.

On the other hand, the question of victory or defeat may be moot. There are signs that popular support for a war being conducted wanes as the costs of war mount. U.S. public opinion on the wars in Korea and Vietnam drives home the lesson that war, if anything, is more likely to erode government support in the mass public than to boost it (Kernell 1978; Mueller 1973; NorpOTH 1984). From World War I up to the Vietnam War, the

presidential party typically suffered severe losses at the polls after the United States went to war, win or lose. And in Britain the Conservative party lost the general election of 1945 despite the triumph over Germany only a few months earlier.

Like war, the economy, too, may be confronting political leaders with a losing, not a winning, proposition. While bad economic times appear to erode popular support for political leaders, good times may be doing little to boost it. Such an asymmetric effect of economic performance emerges in some studies of congressional elections (Bloom and Price 1975; Kernell 1977) and presidential popularity (Lanoue 1986; Mueller 1973). Prosperity, according to this school of thought, has come to be expected by the public. A government presiding over it is doing its job and will thus be judged by other,

noneconomic criteria. An economic slump, on the other hand, makes the economy an "issue," with blame falling on the government in office. Voters are seen as primarily concerned with the negative side of economic performance. They punish failure but do not reward success.

Great Britain under the government of Margaret Thatcher provides a special opportunity to test those hypotheses about war and economic performance. In April 1982, Argentina and Britain went to war over the Falkland Islands (Malvinas), a war that ended with Argentina's surrender in mid-June (for accounts, see Hastings and Jenkins 1983). To be sure, in the short run this war does not seem to have diminished public support for the Thatcher government (Clarke, Stewart, and Zuk 1986; Dunleavy and Husbands 1985; Mishler, Hoskin, and Fitzgerald 1986; Worcester and Jenkins 1982). But that by itself does not prove that the war made a lasting impression on the public and paid electoral dividends for the governing party.

The Falklands War occurred amidst a wrenching economic upheaval in Britain brought on by the Thatcher government (King 1986; Newhouse 1986; Riddell 1985). While inflation was slowing, unemployment rose to Depression-like levels. This set of economic outcomes presented the public with a dilemma: reward the government for the relative success on inflation, or punish it for its failure on unemployment? The public's dilemma, on the other hand, gives us an opportunity to probe for the "asymmetry" of economic effects. (For studies on economic voting in Britain, see Alt 1979; Butler and Stokes 1969; Goodhart and Bansali 1970; Hibbs, Rivers, and Vasilatos 1982; Miller and Mackie 1973; Whiteley 1986; Yantek 1985).

This paper examines the effects of the split macroeconomic performance and the Falklands War on the popularity of Prime Minister Thatcher and her party by way

of an "intervention analysis" with monthly data from June 1979 to July 1985 (Box and Tiao 1975; Hibbs 1977; Lewis-Beck 1986; McCleary and Hay 1980).

The Falklands and Thatcher's Popularity

An event like the Falklands War may leave a mark on government popularity in two ways. First, it may either lead to a *permanent* shift in the level of support or just make a *temporary* impression. Certain international events have been noted to generate "rally-around-the-flag" reactions (Brody 1986; Kernell 1978; Mueller 1973; Norpoth 1984). A boost in popularity is followed by erosion to the preintervention level. Second, the onset of the intervention impact may be *abrupt* or *gradual*. It may take some time for the intervention to register with the public. Thatcher's popularity leading up to, and following, the Falklands crisis is depicted in Figure 1.

The four intervention models have been estimated with an error (noise) specification that assumes a first-order autoregressive (AR) process. This AR(1) specification emerges from an analysis of Thatcher's popularity in the time period prior to the Falklands War (Box and Jenkins 1976; McCleary and Hay 1980). For now, we assume that the prime minister's popularity at any given time is a function of a long-term component approximately captured by the overall mean, plus short-term oscillations due to the impact of the Falklands War, autoregressive noise, and "white noise."

The results of the estimation of the various models are presented in Table 1. The abrupt-permanent model receives some support, as indicated by the significant estimate for *April*. Thatcher's popularity, according to this model, rose in April of 1982 to an equilibrium six points higher than before. The findings of the

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Figure 1. Satisfaction with Thatcher's Job as Prime Minister

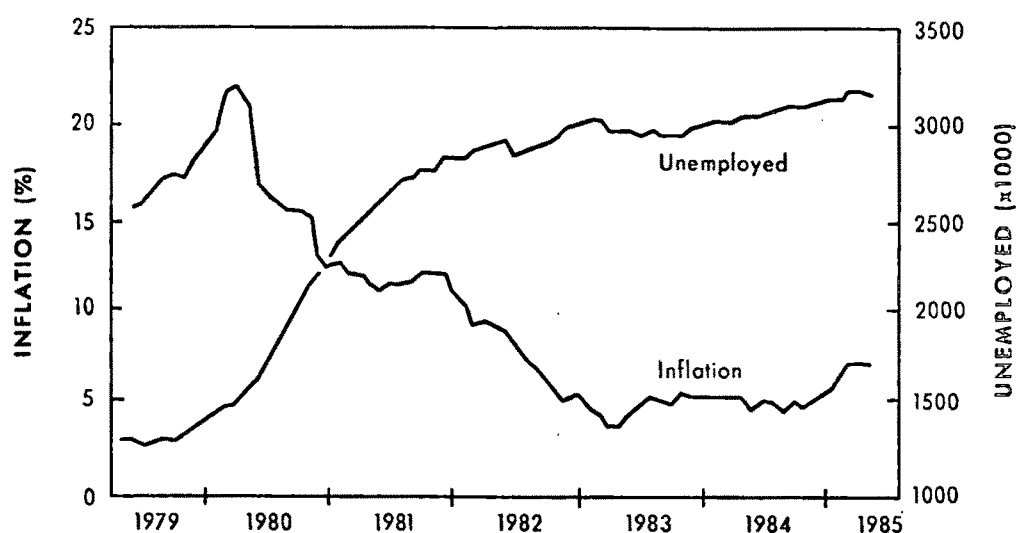


Table 1. The Falklands and Thatcher's Popularity

Variable	Models of Intervention							
	Permanent		Temporary					
			Abrupt			Gradual		
	Abrupt	Gradual	April	May	June	April-June	April-May	May-June
Constant	-3.6 (2.6)	-6.3 (3.2)	-3.2 (2.6)	-4.3 (2.2)	-.36 (2.9)	-4.8 (2.4)	-4.6 (2.3)	2.0 (2.8)
April 1982	6.3 (2.8)	6.4 (3.2)	7.9 (3.2)	—	—	4.5 (2.8)	4.8 (2.8)	—
May 1982	—	—	—	15.9 (2.8)	—	14.7 (2.8)	15.0 (2.8)	14.2 (2.7)
June 1982	—	—	—	—	5.5 (2.8)	3.5 (2.9)	—	9.6 (3.0)
Rate	—	.44 (.30)	.98 (.03)	.96 (.02)	-.05 (.50)	.95 (.02)	.95 (.02)	.48 (.18)
AR(1) ^a	.83 (.07)	.85 (.07)	.81 (.07)	.80 (.07)	.89 (.05)	.82 (.06)	.82 (.07)	.93 (.05)
SER ^b	3.3	3.2	3.3	2.9	3.4	2.8	2.8	2.8
Q ^c	19	20	19	25	18	22	26	20

Note: These results were obtained with BMDP2T. Standard errors for parameter estimates are in parentheses. Estimates more than twice as large as their standard errors are significant at the .05 level.

^aFirst-order autoregression specification for noise.

^bStandard error of residuals.

^cThe Ljung-Box test of model adequacy; with 19 degrees of freedom, none of the Q estimates is statistically significant at the .05 level.

Source: Market and Opinion Research International, June 1979-July 1985 (74 monthly observations; for three missing values, Gallup data used).

gradual-permanent model are less compelling, by contrast. The rate-of-change parameter turns out to be insignificant.

In the temporary domain, each of the two basic scenarios, abrupt and gradual, has been examined in several versions. For the abrupt-temporary type of intervention impact we have separately estimated effects for April, May, and June of 1982. The most convincing evidence is found for the May version, which suggests a boost of 16 points in May of 1982, to be reduced subsequently at the pace of 4% per month (100%-96%). By May 1983, a 10-point gain remains, according to the model estimates. Still, how does the fit of this temporary-impact model compare with that of the *gradual-temporary-impact* models?

Here, too, we have examined three versions. The April-May-June version assumes that the buildup of the Falklands effect was distributed over those three months while the alternatives either exclude the final month or the opening month. The May-June version assumes that the Falklands impact registered among voters only after a delay, whereas the April-May version assumes that no further gain in public support registered in June, the month of victory.

The May-June version can be quickly dismissed in light of the results presented in Table 1. The AR(1) parameter is practically 1.0, which dooms the whole model. The April-May-June version is somewhat credible but fails to yield an impressive estimate for June. The inclusion of a June effect does not yield any benefit above and beyond what the April-May model, which excludes it, provides. We conclude that the Falklands War boosted Thatcher's popularity in April and May of 1982, with the lion's share coming in May, but that these gains proved temporary. No substantial rise occurred in June, when Argentina capitulated.

Does this mean that victory did not

matter to the British public? that we would have found the same results in case of defeat? Of course not. The point is that victory was anticipated in Britain. The public had no doubts of victory, as shown by a Market and Opinion Research International survey in April 1982 ("How" 1982). Victory came as expected and the electorate apparently behaved in the manner of "rational expectations." No additional reward accrued the government in public support over what had been given in April and May in the expectation of victory. Had the Thatcher government disappointed its public by either losing the war or being unable to bring it to a swift conclusion, public reaction might have been quite different. Most likely, the April-May boost would have evaporated at a much faster rate.

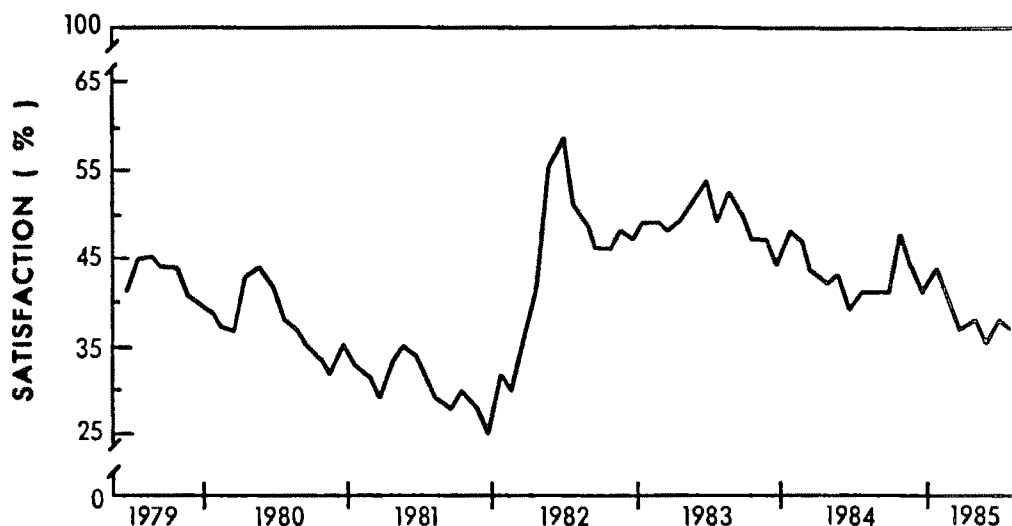
The Economy and Thatcher's Popularity

Rare are the opportunities for government leaders to capitalize on foreign-policy events to the degree apparently done by Margaret Thatcher in 1982. For the most part, it is domestic performance that matters for government popularity. Hardly any aspect of domestic performance has been more thoroughly scrutinized in this context than the state of the economy. Thatcher has closely tied her political fortunes to the economy by her attempts to wean the economy from much of its state tutelage (King 1986; Riddell 1985).

While many of her policies have affected some part of the British public, what is most visible to the general public is the change in overall unemployment and inflation, the two key concerns cited in public-opinion polls in response to the question about "problems facing the nation." As can be gleaned from Figure 2, under Thatcher these two macroeconomic indicators have moved in a classic

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Figure 2. Two Sides of British Economic Performance under Thatcher



Phillips-curve fashion. While unemployment has surged to over three million the inflation rate has declined to roughly 5%.

How did those developments affect Thatcher's approval ratings? There probably was a time when it would have been unthinkable for a prime minister to remain in office with three million unemployed. How much popularity has it cost her? Has the decline of the inflation rate offset some or all of those losses? In modeling the way the economy influences the political judgments of the public, we assume a retrospective, incumbent-oriented calculus where the public relies on the most visible indicators of economic performance (Kramer 1971).

Specifically, we examine the effect that a change in unemployment and prices over a year may have had on public approval of government. The inflation rate is normally calculated this way, as a percentage change of the price index over a 12-month period. For unemployment, we proceed analogously, by taking the change in the number of unemployed over a year. These specifications are supported

by an examination of cross-correlations, which shows popularity ratings to be most strongly correlated with 12-month changes of unemployment and prices.

So defined, macroeconomic performance joins the Falklands intervention as modeled by the best fitting specification (the gradual-temporary, April-May version) and the autoregressive component as predictors of government popularity. The overall mean of the popularity series as adjusted by the constant again captures the long-term component of government popularity.

The results presented in Table 2, column 1, make a strong case for the effect of unemployment on Thatcher's approval ratings. Based on the estimate for unemployment given in Table 2, we would estimate that the rise in the number of the unemployed from 1.5 million in the middle of 1980 to over 2.5 million one year later cost her roughly 10 points. By the same token, the drop in inflation from 20% to 5% has utterly failed to compensate for that loss. The parameter estimate for inflation in column 1 is minimal and

nowhere near statistical significance.

As the asymmetry hypothesis predicted, the relative success of Thatcher's economic policy (on inflation) went unrewarded whereas the failure (on unemployment) was punished. Confronted with two bits of key economic news, the public

took note of the negative bit and ignored the positive one in judging government performance. Perhaps it did so in the assumption that the decline of inflation simply restored a situation considered reasonably normal. By contrast, the rise of unemployment pushed this topic into

**Table 2. The Falklands (Gradual-Temporary Impact), the Economy,
the 1983 Election, and Thatcher's Popularity:
Estimates for Five Models**

Variables	Models				
	1	2	3	4	5
Constant	-3.7 (1.9)	-5.3 (2.7)	-3.5 (1.1)	-3.5 (1.2)	-3.5 (1.1)
Falklands					
April 1982	4.3 (2.6)	5.0 (2.8)	4.2 (2.6)	4.2 (2.5)	4.2 (2.5)
May 1982	14.5 (2.7)	15.2 (2.9)	14.5 (2.6)	14.5 (2.6)	14.5 (2.6)
Rate	.93 (.03)	.96 (.02)	.93 (.02)	.93 (.02)	.93 (.02)
Inflation ^a	.03 (.29)	.16 (.37)	—	—	—
Unemployment ^b	-.0112 (.003)	—	-.0112 (.0026)	-.0112 (.0026)	-.0112 (.0025)
Election 1983					
June 1983	—	—	—	3.8 (2.1)	4.2 (2.2)
Rate	—	—	—	-.41 (.39)	—
AR(1) ^c	.60 (.10)	.80 (.07)	.60 (.10)	.61 (.10)	.60 (.10)
SER ^c	2.7	2.8	2.7	2.6	2.6
Q ^e	21	26	21	17	19

Note: These results were obtained with BMDP2T. Standard errors of parameter estimates are in parentheses.

^aMeasures the percentage change of the retail-price index since a year before; entered into the analysis with a lag of one month.

^bMeasures the change in the number of the unemployed (in 1000) since the previous year; entered into the analysis with a lag of one month.

^cFirst-order autoregressive specification for noise

^dStandard error of residuals.

^eThe Ljung-Box test of model adequacy; with 19 degrees of freedom, none of the Q estimates in the table above is statistically significant at the .05 level.

Sources: Market and Opinion Research International, June 1979–July 1985 (74 monthly observations); for three missing values, Gallup data used; *Economic Trends*; *Monthly Digest of Statistics*.

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the public spotlight, where it endures as the number-one problem for government to solve. Another possibility is that the public did not regard the drop of inflation as a success since prices still kept going up at something like 3%-5%.

It might also be argued that the missing effect of inflation is an artifact. As Figure 2 pointedly illustrates, unemployment and inflation run in opposite directions. The two are strongly, if negatively, correlated with one another. So to the extent that one of the two accounts for variation in government popularity, it may deprive the other one of its rightful share. Is unemployment crowding out inflation in their competition to explain popularity? To check this possibility, we excluded unemployment from the analysis. However, as shown in Table 2, column 2, inflation gets absolutely no help from this protective treatment. Its estimate gets neither larger (in the right direction) nor more significant.

As for the Falklands effect, the inclusion of unemployment in the analysis does it little harm. As before, the gradual-temporary specification yields a cumulative popularity gain in April-May of approximately 20 points, eroding at a rate of roughly 7% a month. This estimate changes little when what turns out to be a small and inconsequential effect of the 1983 election on Thatcher popularity is also taken into account (Table 2, cols. 4 and 5).

The Falklands War and the General Election of 1983

While the election of 1983 may have added little to Thatcher's own popularity, it gave her party a landslide majority in the House of Commons. How much did the electoral victory owe to the Falklands victory? This question cannot be simply answered by citing Thatcher's popularity gains and what remained of them a year

later. A leader's gains and losses do not automatically translate into gains and losses for the party. On election day, as Churchill sadly experienced it in 1945, "the British electorate tends to vote according to what a party represents rather than who represents the party" (Crewe 1981, 275). We have to ask, therefore, whether the Conservative party gained in public support as the result of the Falklands War. And if so, what was left of that gain one year later, at election time?

As Table 3 shows, the Falklands War left a strong mark on the Conservative share (based on answers to the question, "How would you vote if there were a general election tomorrow?" and if no answer to this, "Which party are you most inclined to support?"). The model that best fitted Thatcher's popularity, however, does not suit her party equally well, as one can gather from the results in column 1. The April-May gradual-temporary specification of the Falklands intervention fails to confirm an effect for April and only yields one for May half as large. On the other hand, as columns 2 and 3 make clear, the Conservative party registered a substantial gain in June 1982.

At the same time, unemployment hits the party almost as strongly as its leader, while inflation again fails to matter. With unemployment included in the model, the gradual-temporary specification of the Falklands intervention (May-June version) yields a gain for the Conservative party of 11 points, as can be calculated from the results in column 3. This gain shrinks at a rate of 7% each month. By May 1983, when the House of Commons was dissolved, a five-point gain, according to our estimate, remained.

In order to predict, as it were, the Conservative vote share in the 1983 election, we can, of course, avail ourselves only of information available in early June 1983. Reestimates of the model of Conservative support with just that information are

presented in column 4. These estimates lead us to a forecast of 45.5% for the Conservative party in the June 1983 election. That slightly overpredicts the actual party share of 43.5% but stays close to the boundary of one standard error.

Our forecast, based on the effect of

unemployment and the Falklands bonus plus Conservative base support, does no worse than most last-week polls, which overestimated the Conservative share by 2.5 points, on the average (Dunleavy and Husbands 1985, 81). The Falklands, according to our estimate, were worth be-

Table 3. The Falklands (Gradual-Temporary Impact), the Economy, the 1983 Election, and Conservative Party Support: Estimates for Four Models

Variables	Models			
	1	2	3	4
Constant	-2.3 (1.0)	-2.8 (1.5)	-2.3 (1.0)	-2.1 (.9)
Falklands				
April 1982	.8 (1.9)	—	—	—
May 1982	9.2 (2.0)	8.6 (2.0)	8.4 (1.9)	7.9 (1.9)
June 1982	—	3.7 (2.0)	3.8 (2.0)	3.1 (2.0)
Rate	.94 (.03)	.94 (.04)	.93 (.03)	.945 (.038)
Inflation ^a	—	.11 (.23)	—	—
Unemployment ^b	-.0093 (.0023)	-.0090 (.0024)	-.0093 (.0023)	-.0100 (.0021)
Election 1983	-1.9 (1.7)	—	—	—
AR(1) ^c	.67 (.10)	.67 (.10)	.68 (.10)	.66 (.12)
SER ^d	2.0	2.0	2.0	1.9
Q ^e	18	12	14	12
Number of cases	74	74	74	48

Note: These results were obtained with BMDP2T. Standard errors of parameter estimates are in parentheses.

^aMeasures the percentage change of the retail-price index since a year before; entered into the analysis with a lag of one month.

^bMeasures the change in the number of the unemployed (in 1000) since the previous year; entered into the analysis with a lag of one month.

^cFirst-order autoregressive specification for noise.

^dStandard error of residuals.

^eThe Ljung-Box test of model adequacy; with 19 degrees of freedom, none of the Q estimates in the table above is statistically significant at the .05 level.

Sources: Market and Opinion Research International, June 1979–July 1985 (74 monthly observations); for three missing values, Gallup data used; *Economic Trends*; *Monthly Digest of Statistics*.

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tween five and six percentage points to the Conservative party in June 1983. Without them, the party might have gained no more than 38% on election day. In the three-cornered race of 1983, given the desolate state of the Labour party and its unpopular leader, such a vote share might still have secured a majority in the House of Commons. But historically speaking, no party has won a majority with 38 points or less in Britain since 1945.

Conclusions

The Falklands War may have had little strategic and international significance—the Argentinian writer Borges dismissed it as “two bald men fighting over a comb” (cited in Theroux 1984, 47)—but it stirred the British public as few events before. Of the four intervention models considered, the one that best captures the war’s impact on government popularity is of the gradual-temporary variety. A surge of popularity is followed by a decline. It must be stressed, however, that what is gained in essentially two months is not gone in another two months. Though “temporary” in a technical sense, the impact erodes so slowly that it pays a handsome electoral dividend for the governing party a year later. Even two years later traces of a Falklands effect can still be detected.

Unlike other wars, the Falklands War thus did not undermine the popularity of the incumbent government. One reason, to be sure, was that Britain won the war; another, that it was provoked to take military action by what most Britons considered a blatant act of aggression. Furthermore, it allowed Thatcher to demonstrate her blend of decisiveness and perseverance (Hastings and Jenkins 1983, 355-56; Newhouse 1986, 75). As for the press, the chauvinistic tabloids as well as the sophisticated *Times* by and large supported Britain’s role in the war. In order-

ing the military recapture of the Falklands, Thatcher could even count on broad support from the opposition benches.

Still more important perhaps, the war was brief and cost relatively little by comparison to other wars. It was over before its costs were felt by the public. The boost in Thatcher’s popularity actually crested before the conclusion of the war. A protracted war with mounting casualties, we suspect, would have chipped away that boost quickly enough to leave her with little in the short run.

The successful handling of the Falklands War relieved the heavy pressure on government popularity exerted by macroeconomic conditions. Confronted under the Thatcher government with a rising tide of unemployment and an ebbing away of inflation, the British public was found to key on the negative side of economic performance. Public support for Thatcher and her party proved sensitive to unemployment but not to inflation. In this instance, then, it seems that the public punished its government for failure but refused to reward it for success.

While this finding lends support to the asymmetry hypothesis of economic effects, certain qualifications are in order. First, the inflation “success” is limited, falling short of eradicating rising prices. Perhaps zero inflation would have registered significantly in government support. Second, bringing down inflation may not yield any palpable gains for most people, although rising inflation is felt as a loss. It may be different for employment, where the gains and losses are more immediately felt by individuals. Furthermore, the rise in unemployment under Thatcher reached unprecedented postwar heights and went far beyond what might seem appropriate as a cost for curbing inflation.

It must be noted with a touch of irony that the Thatcher government set as its top economic priority the fight against in-

flation while eschewing responsibility for unemployment, leaving that for the private sector to handle. Viewed from that perspective, the public's reaction is not only ungrateful but also unfair. The public is punishing the government for failing to deliver what the government did not promise to deliver, namely full employment, while refusing to reward it for progress on inflation, where the government had made a pledge. No doubt, the public has not much shared the government's economic priorities, and Thatcher has resisted reordering them. It is perhaps this refusal that has especially irked the public and produced the asymmetric response to economic performance found in this analysis.

Note

I am grateful to Robert Worcester, director of Market and Opinion Research International (MORI) in London, for making available to me the data on British-government popularity and party preferences analyzed in this paper. For the few time points (June, July, and December 1979) for which no MORI data existed, the corresponding Gallup data were substituted. Of course, neither MORI nor Gallup bears any responsibility for the analyses and interpretations of these data. The data on unemployment come from the monthly reports, *Economic Trends*. The specific measure of unemployment used here is "registered wholly unemployed, seasonally adjusted, in the United Kingdom." The data on the retail-price index, on which the inflation index is based, come from the *Monthly Digest of Statistics*. For comments on earlier drafts of this paper I am indebted to Robert Worcester, Sam Kernell, Kathy Frankovic, John Mueller, Friedheim Meier, and Jorgen Rasmussen.

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NATIONALIZATION OF THE ELECTORATE IN THE UNITED STATES

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A current debate within the field of U.S. politics revolves around the extent to which the electorate is nationalized, that is, responds to national-level political forces. We provide additional evidence to help resolve this debate. Using the variance-components model of political effects developed by Stokes, we analyze presidential, senatorial, gubernatorial, and congressional races for the period 1962–1984. The results indicate that presidential races are highly nationalized, and congressional races largely localized. Senate and gubernatorial races are found to have a large state-level component to them. The analysis is also split into two segments to ascertain if a change in political effects is seen during this relatively short time frame; no such differences are found. We conclude that nationalization is not a process that can be understood from evidence provided by congressional races alone. Our results suggest that constituencies respond to political forces in a complex way, reacting to factors that are most relevant to a particular electoral contest.

An ongoing debate within the field of U.S. politics concerns the extent to which the electorate has become more or less nationalized (see Claggett, Flanigan, and Zingale 1984 for an overview of this research). A standard method for assessing nationalization, the variance-components model developed by Stokes (1965),¹ provides for the decomposition of variance in electoral results into national, state, and local components. Based on indications of decreasing local components for party voting and strongly nationalized turnout, Stokes (1965, 1967) concluded that the U.S. electorate had become increasingly nationalized. Claggett, Flanigan, and Zingale (1984) joined the debate and took issue with Stokes's conclusion. They suggested

that Stokes overestimated the amount of nationalization and that in large measure, local variation has continued to predominate in congressional elections.

Both sides of the debate have relied exclusively on evidence from congressional elections.² This emphasis on congressional returns raises an intriguing question: Might analyses of other races, such as presidential or important statewide races, show differential effects of national-, state-, and local-level forces on party votes for different offices? Our aim is to address this question by expanding the scope of the nationalization debate to include evidence from multiple offices. We will analyze not only congressional returns but also presidential, senatorial, and gubernatorial races. With this

broader look at electoral contests, we hope to be able to identify the races in which national-, state-, and local-level effects are likely to be most operative. We further hope to draw some conclusions concerning the extent of nationalization in the U.S. electorate.

A Multioffice Perspective

In this research, the variance-components model is used to analyze county-level electoral returns from 1962 through 1984 for the offices of president, U.S. senator, governor, and U.S. representative.³ These races were selected because, collectively, they provide the opportunity to observe a wide range of national-, state-, and local-level forces at work. The anticipated result of the variance-components analysis is that national forces will be most operative in presidential races, state-level forces will be strongest in senatorial and gubernatorial races, and local forces will predominate in congressional elections.

While it is known that electoral outcomes for a variety of offices are at least partly a function of national forces (see, e.g., Campbell 1986), only the president and vice president are elected from a national constituency. Many of the other factors suspected of influencing electoral results are constituency specific, for example, campaign spending, constituency service, and incumbency. While these factors may influence elections in a relatively uniform fashion across races, the exact degrees to which they are operative in a given race are clearly variable. The likely salience of these constituency-specific factors underscores the importance of the multioffice perspective, resulting in the patterns of nationalization hypothesized above.

In general, these expectations are consistent with recent suggestions concerning the differentiation between types of elections by the electorate. For example,

Schlesinger (1985) has found that in the "new party system" voters are able to divide their support between state and national party systems, resulting in a separation of congressional and presidential races. Similarly, Ladd and Hadley (1978) have argued that a two-tiered system of party politics exists, one in which state and national forces have differential effects on different types of elections. Both of these works point to the importance of analyzing multiple offices in order to come to a complete understanding of the U.S. party system.

Analysis

The conceptualization of nationalization consistent with the variance-components model has been discussed at length by Claggett, Flanigan, and Zingale (1984, 81-82). The mechanics of applying the variance-components model can be briefly summarized: the model decomposes the variance in local-level vote (in our case, the Republican percentage of the two-party vote within counties) by assessing the national-, state-, and local-level variations around the mean vote for the entire period; the result is an estimate of the amount of variance seen for the average county,⁴ as well as the decomposition of this variance into national-, state-, and local-level components. (A complete description of our procedures is contained in a methodological appendix, available upon request.)

Table 1 contains the results of the variance-components analyses for the four races—for president, U.S. senator, governor, and U.S. representative—for the period 1962-84. The size of the "normalized variance component" provides information concerning the relative impact of each of the levels of political forces, while the "variance component" indicates the absolute amount of variability across time for the average county during this period. As can be seen, the

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Table 1. Variance-Components Analysis for President, U.S. Senator, Governor, and U.S. Representative, 1962-84

Political Level	Variance Component	Square Root of Variance Component	Normalized Variance Component
President			
National	87.64	9.36	.56
State	40.87	6.39	.26
Local	27.32	5.23	.18
U.S. Senator			
National	11.33	3.37	.09
State	91.26	9.55	.68
Local	30.62	5.53	.23
Governor			
National	7.18	2.68	.07
State	63.90	7.99	.63
Local	30.44	5.52	.30
U.S. Representative			
National	7.64	2.76	.05
State	49.64	7.05	.31
Local	103.31	10.16	.64

results conform to our expectations. For the presidency, the national component of the vote is clearly the strongest, followed by some influence for state-level factors. For senatorial and gubernatorial races, the state component is most important. In both cases, local-level factors appear to be influential in a secondary fashion. Finally, for the congressional results, the local component is the largest, with state forces also being somewhat important.⁵

These results suggest the limitations inherent in nationalization research with a single-office perspective. Looking at changes in nationalization for congressional races, even over an extended period of time, may not provide a complete picture concerning the process of nationalization. In addition to the variations in relative sizes of variance components observable for a single office over time (see, e.g., Claggett, Flanigan, and Zingale 1984), there are substantial differences among offices. Moreover, there is little reason to suspect a priori that patterns of

longitudinal change are identical across these offices.

In short, the variance-components analysis shows that the U.S. electorate displays various degrees of nationalization depending on the electoral contest in question: presidential contests are highly nationalized, senatorial and gubernatorial races have largely state-level components, and congressional elections are primarily local in nature.⁶ These findings lend support to the multifaceted view of the electorate discussed by Schlesinger (1985) and Ladd and Hadley (1978). When viewed from this perspective, nationalization is not a simple, unidimensional process: rather, it would appear that multiple processes of nationalization are at work.

If this portrait of the electorate is accurate, we would also expect the outcomes of the various races to be more or less closely related depending on the similarity of constituency. The variance-components procedure breaks the variation in voting results into national, state,

Table 2. Correlations of Local Fixed Effects for President, U.S. Senate, Governor, and U.S. Representative for 1962-84

Office	President	U.S. Senator	Governor	U.S. Representative
President	1.000	.899	.878	.704
U.S. Senate	—	1.000	.926	.753
Governor	—	—	1.000	.763
U.S. Representative	—	—	—	1.000

Note: The statistic is Pearson's r-correlation coefficient.

and local effects constant throughout the time period (fixed effects) and specific to a particular election (random effects). By looking at the correlation between the local fixed effects for the various offices, we can gain insight into the relationships among the long-term partisan tendencies for these races. We would expect the correlation among the local fixed effects to be high. The pattern of correlation between the offices will show which offices are most similar in terms of their long-term local partisan tendencies.

As we show in Table 2, all of the correlations between these long-term, county-level effects are substantial. The strongest correlation is between the fixed effects for the two statewide races, for governor and U.S. senator, while the weakest correlation is between the fixed effects for the races with the most dissimilar constituencies, those for president and U.S. representative. The local fixed effects most clearly mirror each other for the races most similar in the variance-components results, gubernatorial and senatorial. The long-term local partisan tendencies are most dissimilar for the most nationalized race (for the presidency) and the highly localized congressional races. Overall, these results offer further evidence that voters distinguish between the partisan candidates for different offices and that the geographic scope of the race appears to be an important element in this process.

Thus, we have evidence of differentiated voting patterns from two perspec-

tives. First, the variance-components analysis shows differing amounts of nationalization for presidential, senatorial, gubernatorial, and congressional elections. Second, the pattern of correlation among local fixed effects also shows variable degrees of association between contests. In sum, responses to national, state, and local forces are not uniform for every race but, instead, are race specific.

Conclusion

The answer to the question whether the U.S. electorate is nationalized or not would seem to be, *it depends*. Just as Claggett, Flanigan, and Zingale (1984) have shown the importance of a historical perspective in assessing recent trends, the evidence reported above suggests the additional benefit of a multioffice perspective. Our results clearly show a substantial degree of variability across offices in the degree to which electoral returns are a function of national, state, and substate forces. In addition, there is a clear tendency for the salience of a particular level of force to be linked to the geographic scope of the constituency. From a comparative-office perspective, congressional races are largely localized. Presidential races, on the other hand, are nationalized. For statewide races, such as U.S. senate and gubernatorial contests, state-level effects are the strongest.

Thus, it would appear that the electorate has the *potential* to respond to elec-

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toral forces in a highly nationalized fashion if national forces are relevant to the contest in question. However, if factors such as the *national* media are irrelevant to a race, as is usually the case with congressional elections, then the electorate responds to the more constituency-related, localized forces.

The findings also show that nationalization must be viewed within the operative political system. In a federal system with multiple levels of effects and structures that reinforce the continued existence of such effects, an electorate that responds to national forces for all races is an unlikely eventuality. A large degree of nationalization is likely to be reflected only in races national in scope. While we do not have data to further test this proposition, it is very likely for even more localized races, such as state-legislative races, that variance in returns is even more heavily localized in nature.

Finally, these results have some implications for the party system as well. Schlesinger (1985) has observed that the "new party system" is one in which each political party has developed a national geographic base, while at the same time, levels of party activities—national, state, and local—appear to be increasingly separate in terms of their impact on elections. This observation appears to fit our results well. The U.S. electorate's complex response to electoral stimuli appears to be shaped by the interaction of federal structures, a presidential system, and the variety of representational schema employed. As long as the United States remains a heterogeneous nation with multiple positions of political power, electoral patterns are likely to continue to respond to a mixture of national, state, and substate forces that match political interests with the powers of particular offices.

Notes

Resources for the 1984 data collection were provided by the University of Texas, Dallas School of Social Sciences, the North Texas State University Faculty Research Office, and the University of Houston Center for Public Policy. We thank Steve Wright for assistance in preparing the data sets and Bob Brookshire and Claudia Lynch for their help with the variance-components analysis.

1. The variance-components model and various extensions of it have been used to assess the extent of nationalization not only for the U.S. electorate but also for many other countries (see, e.g., Browne and Vertz 1983; Bueno de Mesquita 1978; Jackman 1972; Katz 1973; and Mughan 1978).

2. Claggett, Flanigan, and Zingale (1984, 87) have indicated that they estimated the size of the national component for presidential returns, but they did not include the analysis within their article.

3. We obtained the county-level electoral data for the period 1962-82 from the historical archive of the Inter-University Consortium for Political and Social Research. The 1984 election returns were collected directly from each state (generally from the office of the secretary of state). None of these sources bears any responsibility for the analysis presented in this research.

4. We are using the expression *average* in the sense of *typical*, although mathematically, the overall estimates of national, state, and local effects are, in fact, arithmetic means of the effects in individual counties. Thus, particular counties may display greater or lesser relative amounts of national, state, and local effects than the overall estimate.

5. Our congressional results accord fairly well with previous studies, except that our estimates of the size of local-level effects are somewhat larger. We would note that the previous studies employing the variance-components model did not perform any analysis over periods of redistricting. Because Stokes used district-level data, it was impossible for his analysis to extend over these periods; redistricting obviously changes the units from which district-level data are derived. For comparability, Claggett, Flanigan, and Zingale also calculated their estimates by decade. Our analysis was performed over redistricting periods and suggests that a substantial amount of variability is caused by the redistricting process. We believe that this is one important structural reason why congressional races are unlikely to display the large amount of nationalization seen for presidential contests.

6. In addition to the analysis performed for the entire 1962-84 period, we split the data into two equal periods, 1962-72 and 1974-84 and reestimated the variance components. This permits some assessment of changes in the relative strength of the na-

tional, state, and local forces over time. The results for this split analysis (not shown) are straightforward. The two periods do not differ in terms of their level of nationalization or in any other substantial respect. The pattern of findings for the relative importance of national, state, and local effects parallels the one described in Table 1 for the entire 1962-84 period.

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POLITICAL THEORY

Revolutionary Politics and Locke's Two Treatises of Government. By Richard Ashcraft (New Jersey: Princeton University Press, 1986. xxii, 613p. \$65.00, cloth; \$15.00, paper).

When major studies are long and eagerly awaited there is always a danger of disappointment when they appear. There is no such disappointment with Richard Ashcraft's monumental study of John Locke. This is an ambitious work in that it offers not merely a study of a thinker but a detailed portrayal of the political world in which he moved. Furthermore, it both contains a statement about the way in which the history of political thought should be approached and is itself a paradigm of such a study.

The theme of Ashcraft's interpretation of Locke is that he was, in a repeated phrase, a "hard-line radical." Ashcraft is not content to develop this by textual exegesis. Indeed, his point is that such a claim can only be established by detailed examination of the political context in which Locke moved. That context, Ashcraft shows, was the most extreme radical dissenting tradition, which Locke joined upon his association with Shaftesbury and to which he adhered up to the publication of the *Two Treatises*. It was a policy of opposition that tipped over into a revolutionary conspiracy, attempted political assassination (Rye House Plot), exile politics, and abortive invasion (Monmouth Rebellion), ending with the Glorious Revolution of 1688 which apparently rapidly lost its glory in the eyes of the radicals. At every step, Ashcraft demonstrates, the philosopher Locke was in the thick of it. He tells a story not of detached scholarship but of plotting, spies, mail drops, aliases, codes, kidnapping and revolutionary fund-raising in the midst of which intellectuals like Locke were formulating the campaign materials for the radical movement.

Ashcraft thus looks at political philosophy as ideology, and *Two Treatises* is ultimately the "political manifesto" of the radical movement. Its meaning is rooted in the conception

of the political world Locke shared. The aim of Ashcraft's study is, therefore, to unfold that conception, which he does not merely through the writings of Locke and other radicals but through a detailed account of the personal relationships and dealings they had with one another. For Ashcraft's point is that an effective ideology has an organizational underpinning. Consequently this book has an interest not just to seventeenth-century specialists but as a model of the study of ideology and its organization and dissemination.

The story of Locke's radicalism begins with his incorporation into the Shaftesbury household. Indeed, Locke's status as "assistant pen" almost describes his entire achievement. Until this stage of his career, as is well known, Locke was no radical. Once with Shaftesbury, Locke began to tackle the intellectual issues that interested the Protestant dissenters. The first impetus to a defense came from Samuel Parker's *Discourse of Ecclesiastical Polity* with its vehement attack on dissent in politics or religion. Against this the dissenters had to develop a view of individuals as equal autonomous agents with natural liberty to use their reason. At the same time, they had to avoid the charge of moral anarchy. It was these concerns that set off Locke's thoughts on toleration as well as prompting the *Essay Concerning Human Understanding* (which is not Ashcraft's prime concern). They also prompted a host of other responses from the dissenters.

Politically, the major problem was the threat of royal absolutism and ultimately of the reimposition of Roman Catholicism upon the accession of James, Duke of York to the throne. Ashcraft demonstrates that the radical policy of exclusion of James from the throne was not a narrow technical matter but raised profound questions of the origin of government and of where the authority lay to change the ruler and the constitution in the face of a perceived threat to rights. The "Second Treatise" can then be seen as a specific contribution to these issues, which were to remain fundamental even after James had become king and had, in the opinion of the opposition,

commenced upon his design to undermine political and religious freedom. Ashcraft's method of establishing the relationship between Locke's politics and the radical movement is threefold. He points to those textual passages in Locke that deal with the central political issues for the radicals. Secondly, he shows in rich detail how these arguments are paralleled in other literature emanating from the movement, as in the parallel ideas and career of Robert Ferguson. Thirdly, he establishes, in a way Cranston's biography only touched upon, Locke's interactions with the prominent radicals during the period of the 1670s and 1680s in England and in his exile in Holland, including his involvement with details of conspiracy and rebellion. It is fortunate for Locke that Professor Ashcraft was not one of the government security police of the time because Locke's movements from hiding place to safe house would scarcely have escaped detection to the extent that they did.

The social basis of the radical movement lay not amongst the great proprietors but, Ashcraft argues, the merchants, tradesmen, and even the artisans. Locke's defence of property is a defence of the expansion of trade rather than of accumulated capital in land that might merely be waste. In this respect Ashcraft might have said more about the implied clash of economic class interests. From this it follows that Locke was a radical on political rights and an advocate of a wide suffrage. The rights of individuals and the people meant just that; though here, too, the limits of Locke's specific comments on the franchise in the "Second Treatise" could have received greater attention.

The climax of the radical movement was to have been the Glorious Revolution. But Ashcraft suggests, using recent evidence, that Locke and his fellow radicals believed it was a revolution betrayed. The old guard crept back. The revolution was treated by the establishment more as a coup, which maintained continuity, than a revolution in which the people would employ their constituent power to set the constitution upon new principles. The *Two Treatises* appeared as the manifesto of the radical cause. But the famous argument of the "Second Treatise" that the people had the right to establish government anew was rejected.

Ashcraft's documentation of his interpretation is a piece of outstanding scholarship that leaves us, nevertheless, with a question con-

cerning our view of John Locke. It is rather as if an admired Rembrandt were discovered to be one of many copies made by a member of his school. If Locke's politics are the politics of Shaftesbury and his school, are they of any interest to other than seventeenth-century historians? Are they of more interest than the politics of Robert Ferguson? Ashcraft rarely shows how Locke differs from, or formulates his ideas more ably than, his fellow radicals. The story of how Locke became a "master of political thought" would be another historical investigation. Perhaps once that development takes place, a masterpiece can legitimately take on a life independent of its origins. Meanwhile, Ashcraft's book remains an exceptional study of a thinker's intentions and of the nature of a political movement and deserves the widest readership.

GERAINT PARRY

University of Manchester

A Search for Public Administration: The Ideas and Career of Dwight Waldo. By Brack Brown and Richard J. Stillman II (College Station, TX: Texas A & M University Press, 1986. vii, 196p. \$22.50).

To Dwight Waldo, the search for public administration is unlike the search for the Holy Grail in that the latter does not exist at all, while the former does—"massively, centrally and often decisively for our individual and collective lives" (p. 164). But the search for public administration has been beset with difficulties and obstacles because of its organizational setting. It has been accorded the status of a step-child in political science departments, overshadowed by management in business schools and treated as a second-class citizen in liberal-arts colleges. Consequently, it has failed to mobilize political support and obtain adequate resources to buttress its existence. In Waldo's view, the best hope for the study of, and research in, public administration—if it is to do an effective job of preparing students for public-service careers—is in a professional school similar to that of the medical profession. Public administration is too big to be a part of an academic discipline and too anachronistic to be in a liberal-arts college whose

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academic objective is to liberate the mind from ignorance, arrogance, and intolerance.

Waldo is not alone in advocating the medical analogy as an essential prerequisite in giving public administration an identity. Before him was Henry Reining, Jr., who, in the 1950s and 1960s, spoke strongly in favor of a separate and independent organizational setting for the public-administration enterprise. In spite of the logic of their case, the study of public administration ended up in most cases either as a mere area of interest in political science departments or as a field of study in semi-independent centers, without clout and class. Even when located in more hospitable environments, much more needs to be done to make it close to the medical model. The internship phase of the medical program is more than a preparation for a doctor's career. It is a transition to the real world of medical practice. After the internship, the medical intern is ready to put his knowledge into practical application—quite a contrast to the public-administration programs whose internship component often bears little relationship to what happens after a student completes an internship assignment. With the exception of a few programs, such as the Presidential Management Internship Program of the federal government and the Legislative Staff Internship Program of the Illinois General Assembly, where interns are offered permanent employment after satisfactory performance, very few public-administration internship programs pay any attention to the intern's future. Internship programs that are structured, supervised, synthesized, subsidized, and selective are exceptions rather than the rule.

Waldo was one of the first to recognize that the study of, and research in, public administration cannot be confined to narrow national and parochial boundaries. An organized body of knowledge in public administration cannot claim to be a science unless it is universally applied across various cultural systems. Thus, Waldo and others, like Fred Riggs and Ferrel Heady, sounded the trumpet call for the comparative study of administrative systems, not only to test the validity, utility, and applicability of U.S. theories, concepts, and techniques of administration, but also to explain what accounts for the differences and similarities between them. Much has been learned from studies of bureaucracies of Third

World countries in terms of their behavior, functions, structures, and environments. Comparative public administration has broadened our perspective and enlarged our horizons about the way things get done in other nations. It has also enriched our understanding and deepened our insights on the influence of culture upon administrative realpolitik in developing nations. To the credit of scholars who took Waldo's bureaucratic model as a focus and framework of comparative research, we now know that if corruption continues to plague a Third World bureaucracy in spite of efforts to streamline its operations and reorganize its structure, it is a reflection of its people's culture.

For his vigorous defense of the public-administration enterprise in inhospitable environments, for his contributions to public-administration scholarship, and for his efforts in making public administration an exciting stimulating and challenging endeavor, Dwight Waldo deserves the heartfelt thanks and eternal gratitude of those engaged in the study and practice of public administration.

A. B. VILLANUEVA

Western Illinois University

The Theology of Freedom: The Legacy of Jacques Maritain and Reinhold Niebuhr.
By John W. Cooper (Macon, GA: Mercer University Press, 1985. ix, 185p. \$16.95).

In an age in which religion and its varied connections (and conflicts) with our liberal polity and culture are as obvious as they are controversial, there has been renewed interest in the subject of political theology. Charles Dunn's pioneering work on political theology throughout U.S. political history (*American Political Theology*, New York: Praeger, 1984) is one intriguing example by a political scientist. There has also been renewed interest and writing on the political theology of Reinhold Niebuhr, for example in Ronald Stone's *Reinhold Niebuhr: Prophet to Politician* (Lanham, MD: University Press of America, 1981).

John Cooper's *The Theology of Freedom* explores political theology by comparing Niebuhr and Jacques Maritain, undoubtedly

the two most influential religious theorists of politics who have had an impact in the United States in the twentieth century. Cooper's effort is an important one because of the significance of his two thinkers, especially in the light of current concerns about the role of religion in our society. By comparing and contrasting his two subjects he is able to provide us with a much richer analysis than he would have had he merely considered each separately, and he makes full use of this opportunity. Moreover, Cooper develops, at the same time, his own analysis of political theology in his brief chapter, "Toward an Ecumenical Political Theology." As a result, the reader is able to reflect on several similar, but not at all identical, ideas about the proper relation of religion and political life. *The Theology of Freedom* not only provides us with a great deal of information about Niebuhr and Maritain as theologians of politics, but stimulates us to think on our own about the very current issues involved.

Cooper sees Niebuhr and Maritain as "complementary opposites." Both address the issues of religion, freedom, justice, and practicality in politics and both stress the balances needed between and among these values. Cooper's Niebuhr leans toward an analysis that finds ambiguity and paradox between God and the human, between freedom and justice, between the ideal and the possible. Cooper's Maritain views these relationships in a more hierarchical fashion. But both address the same concerns, see similar problems, and grope toward reconcilable resolutions.

As this discussion suggests, the approach of the book is philosophical rather than biographical or historical, though Cooper neglects neither the story of these men's lives nor the historical setting of their thought. His approach to Niebuhr and Maritain as theorists is surely one they would appreciate. Cooper develops their thought in a fashion that invites us in to the discussion. There is little arcane-ness or esoteric method cluttering his pages. He lets Niebuhr and Maritain speak for themselves through generous and apt quotation, while at the same time developing his interpretive theme.

The result is a book that illuminates these voices of modern political theology. It neither leaves their thought formless nor so over-interprets it that their own version of political theology is lost. This is a nice essay, thought-

ful and thought provoking. I recommend it warmly.

ROBERT BOOTH FOWLER

University of Wisconsin, Madison

Metatheory in Social Science: Pluralisms and Subjectivities. Edited by Donald W. Fiske and Richard A. Shweder (Chicago: University of Chicago Press, 1986. x, 390p. \$35.00, cloth; \$16.95, paper).

This volume of 19 papers presents the products of the conference "Potentialities for Knowledge in Social Science" held at the University of Chicago in September 1983. Unfortunately, political scientists have little representation in this timely and intellectually stimulating collection. However, the issues raised and debated are pertinent to the soul-searching activities of the discipline as a scientific enterprise, and the quality of the arguments is compelling. Indeed, several contributors, in particular Donald T. Campbell, Philip E. Converse, Lee J. Cronbach, and Donald Fiske, have strongly influenced methodological developments and substantive research in political science.

The overall contribution of the volume is in rising above the "dashed expectations," "mid-life crisis" queries on the state of the social sciences to a more general and disciplined level of discourse structured around the fundamental epistemological assumptions inherent in contemporary postpositivist research programs. Disciplined discourse, of course, is an insufficient condition for achieving consensus and none is achieved in this volume. The assumptions of epistemological relativism are diametrically opposed to those of methodological unitarianism. Philosophy of science cannot mediate because, as Rosenberg, a philosopher of science, argues persuasively in chapter 15, there is no formal way to demarcate the empirical from the metaphysical. Consensus, in turn, is an insufficient condition for scientific progress, yet a necessary one for paradigmatic developments in a Kuhnian sense. Perhaps inevitably, and in the revived spirit of evolutionary competition, pluralisms-and-subjectivities is the strategy used to bring together radically different epistemologies under one volume.

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Most contributors either implicitly accept the inevitability of pluralisms or explicitly approve such pluralisms. Levin (chap. 11) even develops an explicit, comprehensive classification of the forms and functions of social knowledge and presents a strong case for "an irreducible plurality of privileged forms of knowledge" (p. 276). In a similar vein, Cronbach (chap. 4) argues for a pluralism of conceptualizations and explanations, bringing to bear multiple perspectives from different disciplines and schools of thought. D'Andrade (chap. 1) presents an ethnographic account of the ideals and the working paradigms of science. He argues that there are at least three very different scientific world views, that of the physical sciences, that of the natural sciences, and that of the semiotic or semantic sciences. Hempel's "covering law," the positivists' ideal, is incongruent with the natural and the semiotic world views: "the emphasis on predicting or explaining individual events as a scientific activity is misguided because of the difficulties of establishing the appropriate boundary conditions" (p. 30). Rather than pursuing Hempel's unattainable ideal, social scientists are better off with the semiotic world view, which seeks to understand "imposed order."

Converse (chap. 2) agreeing with D'Andrade that there is no one ideal law or equation to express progress in science, approaches the pluralisms of science in terms of their subject matter or "texture." Each science has its own texture, the historical predicament being the distinctive characteristic of the social sciences: historical events may rearrange key parameters in social and political processes. Notwithstanding the variety of textures, "a science devotes itself to the systematic decoding of observed regularities and the reduction of the regularities to more parsimonious and general principles that account for wide ranges of phenotypic detail" (p. 52). Indeed, no scientist of the methodological unitarianism persuasion would disagree with this characterization of science in general and the social sciences in particular. Nor would scientists of all persuasions disagree with the "specificity of method" problem as presented by Fiske in chapter 3. Knowledge in the social sciences is fragmented not only because of the multitude of textures but also because of wide variabilities in methods of observation and measurement: "Each method

is one basis for knowledge, one discriminable way of knowing" (p. 62). Measuring procedures do not derive from a general substantive theory; they are artifacts of pragmatic considerations and intuition. Specificity of method prevents the development of inclusive theories (i.e., knowledge), and the lack of such theories perpetuates the specificity-of-method problem and thus pluralisms and subjectivities as well. But would relativists agree with Fiske's fundamental assumption that "people's experience can be subsumed under behavior because we can know about that experience only from some behavior, usually verbal behavior" (p. 61)?

Certainly not by the relativist sociology-of-science position Gergen (Chap. 6) spiritedly and methodically presents. Grounded on the fundamental assumption that "patterns of human conduct are subject to continuous alteration across time," and on the central argument that there is "a very loose relationship between language and the patterns of conduct to which language ostensibly refers" (p. 136), he employs the principles of contextualism and deconstructionist hermeneutics to reject the correspondence theory of the relationship between language and reality and, by implication, the tenets of objectivity and rationality of the social sciences. The contrast between objectivity and subjectivity has, according to Shweder (Chap. 7), been overstated, preventing the emergence of a "science of subjectivity" in which "divergent rationalities" and a variety of meanings coexist, as is the case with cults and cultures.

Campbell's contribution (chap. 5) is fascinating. It is the only explicit attempt in this volume to bridge the seemingly unbridgable fundamental assumptions of the various persuasions. With the exception of hermeneutic nihilism, which rejects the validity of interpretation as a goal, he finds many of the hermeneutic principles (e.g., the principle of charity, the dialectic of guess-and-criticism) to be of great significance to the social sciences. However, the potential contributions of "scientific hermeneutics" can be realized only, as Campbell persuasively argues and illustrates, in the context of a unity of scientific method.

In sum, old theories never die, yet new theories are continuously being invented, the outcome of which is the coexistence of a plethora of divergent assumptions, theories,

methodologies and paradigms that occasionally compete over the direction and the interpretation of empirical research. This is both the strength and the vulnerability of a science of human behavior. Social scientists of all persuasions will find this volume stimulating and challenging.

DAVID NACHMIAS

University of Wisconsin, Milwaukee

Workplace Democracy: The Political Effects of Participation. By Edward S. Greenberg (Ithaca, NY: Cornell University Press, 1986. 259p. \$29.95).

This is a well-written book that deals lucidly with three issues of importance to students of democratic theory, pluralism, and politics of the democratic Left. Using comparative panel data from worker-owned plywood corporations and their conventional counterparts of the Pacific Northwest, Greenberg addresses whether workplace participation contributes to dealienation, enhances attributes of democratic citizenship, or increases class consciousness. Although strongly and positively assessing the workplace democracy in the critical areas of productivity, worker satisfaction, income distribution, and participative governance, Greenberg finds limited and mixed impacts where democratic and leftist theorists would have expected more in each of the three arenas cited above.

Greenberg concisely and engagingly places his research in the context of the issues popularized during the 1960s and later concerning the contention that workers' control in the workplace might have manifold positive outcomes, ranging from economic productivity to increased political participation in the community to increased worker solidarity to better mental health. The relevance to pluralist writers like Robert Dahl and critics like Carole Pateman is cogently drawn. Then data are brought to bear from samples (taken in 1978 and 1983) of several dozen workers in both worker-run and conventional plywood firms.

Greenberg tries to anticipate the criticism that his subject, the plywood co-ops, represent a special case. Though often listed, the co-ops—though well known through the studies of Berman and others twenty years ago—have

not figured importantly in theoretical debate. Proponents of workers' control like Vanek have pointed to the high cost of buying into the co-ops and have depicted them as dominated by a small-capitalist investment ethic, using them as a contrast with self-management, not an illustration of it. Indeed, Greenberg's findings very much corroborate this view.

As expected, Greenberg found work to revolve around a highly automated process, leaving little room for discretion. Ten-hour days and six-day weeks were found to be the rule in an environment that can only be described as stressful, especially in the higher-productivity but lower-safety co-op environments. This is pertinent when Greenberg comes to analyze his survey results. In discussing alienation, for example, he focuses on the "stunning" finding that one-quarter of co-op workers in 1983 would *not* want to look for another co-op if they were to leave the company (compared to 10% in 1978). Greenberg interprets this as lack of "stable solidarity with their fellows" and evidence of alienation (or at least lack of dealienation). But one could argue that given the stressful objective environment, the mentally healthy and nonalienated attitude would be an openness to leaving. At all too many points, this "eye of the beholder" problem leaves Greenberg's arguments unsecured, reflecting the shallowness of the survey technique—a point long emphasized by theorists of the Left.

The subjective-objective problem comes up, for example, in survey data showing co-op workers are not significantly different from their conventional counterparts in feelings of competence on the job. Yet early chapters had documented how co-op members engaged in more job rotation, more problem solving, and management decision making. Numerous studies have shown that attitudes are often not good predictors of behavior, and vice versa. There is a danger that casual readers will miss the subtlety of discussions of alienation behaviorally defined versus attitudinally defined, particularly since Greenberg flips back and forth between the two to maintain his theme, as in his discussion of safety or lack thereof, a behavioral measure.

In the discussion of political participation, Greenberg finds "exactly contrary to theoretical expectations" that co-op workers have a greater individualistic ethic. Actually, Vanek's

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critique of co-ops predicts this quite well. Again, also, the "eye-of-the-beholder" problem emerges, as the indicators here could be interpreted as reflecting increased sense of power. Greenberg's analysis assumes that democratic theory predicts consensual public spiritedness resulting from participation. Both pluralist and theories of the Left have emphasized the empowerment aspects of participation as potentially leading to less cohesion and more conflict. This theoretical background would be more explanatory of both the individualist ethos and the greater nonworkplace political activity of the co-op workers.

In the section on class consciousness, Greenberg again finds little evidence. In fact, co-op workers are more likely to be Republican and less likely to support unions. On the other hand, earlier in chapter 2 Greenberg had credited them as being high in solidarity, a finding in tension with the theme of this later chapter. Co-op workers are also more supportive of a working-class party.

In the conclusion as well as at various other points in the book, Greenberg strongly argues that theorists cannot expect extraordinary impacts on working-class politics from workplace democracy alone. The context of the economic marketplace, egalitarian culture, radical education for democracy, socialist ideology, revolutionary movements, and governments committed to economic democracy explain the greater effects of workplace democracy in such settings as the Israeli kibbutz or Mondragon. Without this context, Greenberg notes, the co-ops "drift inexorably toward enterprise egoism" (p. 168). This was predicted by Vanek twenty years ago, and the prerequisites for democratic socialism with regard to workplace democracy were summarized more comprehensively a decade ago by Bernstein, on the basis of that past literature.

Greenberg has made an important contribution to empirical democratic theory. If the book has a flaw, it is that democratic theory and leftist views on workplace democracy are more complex and more sophisticated than this work gives them credit for, more was known much earlier, and Greenberg's work gives a "corrective to fifteen years of literature" (as advertised on the book jacket) much less than is claimed.

G. DAVID GARSON

North Carolina State University

Mass Media and Political Thought: An Information-Processing Approach. Edited by Sidney Kraus and Richard M. Perloff (Beverly Hills: Sage, 1985. 350p. \$29.95).

A quarter century ago, social science rejected Orwell's vision of public gullibility. It became clear that when buffeted by political propaganda, individuals hang on to social and psychic anchors, with the result that they are not simply swayed by whatever winds dominate the public air. Following the main direction of the evidence, students of mass politics adopted the social-psychological paradigm of *Voting* and *The American Voter*. Having developed technological machinery and mined mountains of data, we now understand much more thoroughly the social and the psychic correlates of political attitudes, heterogeneity in the public, cross-national similarities and differences among mass citizenries, and how citizen views and capabilities may differ over time.

Our current paradigm offers a strong account of the current state of mass politics and shows why things mostly remain the same. We know less about how or why things change. A crucial, perhaps *the* crucial, need here is an understanding of how citizens take in information and change their beliefs and evaluations of the political world. During the past few years, a newly energized research effort, incorporating the ideas of cognitive psychology, has concentrated on just this problem. Not satisfied with noting the rarity of attitude change, the group has focused on the mechanisms that produce it. In this edited volume, Kraus and Perloff show off a number of today's young Turks working the field.

Miller and Asp, Iyengar and Kinder, Perloff, Lau and Erber, Schoenbach and Weaver, and Garramone look at the ways that individual motivation and skill modulate mass communication's information transmission and agenda setting, govern the robustness and the dynamics of value and belief schemas, and determine the types of schemas individuals activate in processing campaign commercials. The most striking work, that of Lanzetta, Sullivan, Masters, and McHugho on the power of television images, shows that subtle facial cues have an impact (sometimes direct, sometimes mediated by psychic state) on viewer emotions and cognitions. More generally, the survey

and experimental evidence indicates that motivation and skill matter (more or less). What is significant here, though, is that the collective effort begins to test different models of cognition and thus to develop our conceptual as well as our empirical knowledge. While the result still falls short of integrated theory, this is not "minimal effects" in disguise.

Other pieces focus more directly on the nature of political cognition. Neuman and Fryling illustrate critical distinctions in individual cognitive style. Tetlock shows that the "integrative complexity" of politicians' and citizens' statements varies as a function of political ideology (political extremists, especially the authoritarian rightists, are simplistic), as well as of political role (opposition is more simplistic than current policy makers). Fishbein, Middlestadt, and Chung argue for the notion of "intention" as an intermediate cognitive step between attitude and action. Tyler and Lavrakas suggest a powerful distinction between the citizen's mental images of the world experienced by the *self* and the mental images of a society populated by *others*.

The articles blend serious reviews of recent research with bits of the authors' current work. Rather than classic pieces, these are snapshots of science in progress. The collection is a professional's view of the warm, perhaps hot, trails to follow in mass politics.

This is not social psychology but, instead, cognitive psychology. This is not a politics of the neighborhood, with its chats over fence post and bar stool, but instead a politics of the living room, with its crumpled newspaper and flickering television screen. The focus here is on how the citizen and the media interact, on how the citizen incorporates the media fantasy into a personal fantasy of politics. One might guess that Orwell's truth was simply overdrawn; it is these scientists' purpose to define its real dimensions. For those concerned with the landscape of future politics, this is the stuff.

MICHAEL MACKUEN

University of Missouri, St. Louis

Nomos XXVIII: Justification. Edited by J. Roland Pennock and John W. Chapman (New York: New York University Press, 1985. xv, 386p. \$42.50).

Like clockwork, almost every year since 1958 has seen the publication of *Nomos*, the yearbook of the American Society of Political and Legal Philosophy. *Nomos* is the Greek word for law; but, being Greek, it naturally implicates the political order and customary ways of life beyond legal edicts and institutions as such. As aptly as any single word could, it captures the interests of the three disciplines of political science, philosophy, and law that comprise the society, which was founded in 1955 by Carl J. Friedrich. Each volume of *Nomos* is issued under a roman numeral and some pithy title indicating its conceptual organization, for instance, *Community*, *Public Interest*, *Representation*, and *Human Rights*. The all-time favorite *Nomos* is *Authority*, which was volume I, later republished, and soon to be volume XXIX. Each volume bears the stamp of considerable editorial labors. In this, Friedrich's legacy has been continued by J. Roland Pennock and John W. Chapman. An index furthers thematic integration, already virtually assured because many essays address one another. All this conspires to produce integrated volumes of intelligent essays in analysis that, truth be told, serve the specialist better than the general reader.

Nomos XXVIII: Justification fits the bill and follows the tradition. There are three sections devoted to justification in ethics, law, and politics, respectively. The philosopher Kurt Baier leads off the first section with a rationalist thesis that in matters ethical "the guidelines of morality point to behavior that would satisfy what we can in reason want or ever demand of one another, in the sense that it would be contrary to reason not to satisfy these wants or demands" (p. 3). This thesis is received with utter skepticism by Felix E. Oppenheim, whose coda reads: "there can be no justification in ethics" (p. 32). Margaret Jane Radin and Michael D. Bayles finish out in two essays that address justification in terms of risk-of-error rules and middle-level principles, putting off ideal theories of justice and foundational principles.

Legal scholar Frank I. Michelman begins the second, and probably the best overall, section

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by shifting ground from justification to justifiability. He then offers a cautious defense of liberal legalism in the face of accusations made by critical legal studies that laws, rights, and judges' powers are riddled with contradiction. This politically sensitive accusation awaits further response until Jeffrey H. Reiman takes it up in the section's last essay, where he too defends legalism. In an essay that deserved to be longer, given its suggestiveness, Amy Gutmann anticipates a defense of liberalism along contextualist, not legalist, lines; and she would have us look beyond judges to the "unity of moral labor" (p. 166) among legislators and judges. One suspects that this is an eighteenth-century use of *moral*, the present political realities of Washington being what they are.

The question of fundamental rights as a constraint on democratic majority rule is taken up by Jeffrie G. Murphy in a deck-clearing operation for later hoped-for constructive efforts. No defense of fundamental rights heretofore offered passes muster with him; and the prospects of there being a long and ambitious list of justified fundamental rights after future efforts appear quite grim. The objectivity of law—so denounced by critical legal studies and by legal realism before that—is defended by Christopher H. Schroeder, but only after he relaxes our expectations about what objectivity is. Martin P. Golding engages in a similar sort of effort, though in his case we are asked to follow the consequences of an analogy between legal and scientific justification, once science is looked at from the postpositivist perspective of Norwood Russell Hanson and others. This analogy, by the way, occasions what is a rarity in a *Nomos* volume: the essays do not quite connect. Baier criticizes related efforts as "moral scientism"; and Murphie observes, not altogether as facetiously as he makes it sound, "One could say that this shows that morality now has a clear bill of health because it is in no worse shape than science. One could also, of course, begin to sign the death certificate on the pretensions of science by arguing that it is in no better shape than morality" (p. 160). What *would* Paul K. Feyerabend be like as chief justice of the Supreme Court?

Political scientist James S. Fishkin gets the biggest rise out of the collected authors, for no fewer than three essays in the third section (and one in the second) respond to his efforts.

He charts seven ethical positions relevant to the justification of the liberal state. Rejecting subjectivism, absolutism, rigorism, and three others, he adopts a "minimal objectivism." Fearing rightly that liberalism is on the eve of self-destruction, he ends with the reasonable if plaintive call for a revision of our moral culture. We should expect (for we will surely receive) less from liberalism by way of what it can justify in absolute terms. Just how we are to secure this moral revision is discussed neither by Fishkin nor by those who take him up on other matters about justification in politics. Barbara Baum Levenbrook counters two of his skeptical arguments about the limits of jurisdiction and the "unforseeability" of future difficulties. A case for subjectivism and against allegations that it undermines liberalism is made by Gerald Gaus.

Four essays by political theorists bring the volume to a close, and collectively make for the most accessible reading. Things take a Kantian (re)turn in Richard Dagger's elegantly written defense of autonomy and a rights-based liberalism. In the face of many recent allegations to the contrary, he contends that no radical individualism need burden liberalism when it is rightly understood. In different essays, J. Roland Pennock and J. Patrick Dobel make out measured defenses of liberal democracy as a political and ethical ideal, thereby illustrating prudential justification. After recognizing the present disquiet, Thomas A. Spragens, Jr. ends his discussion—and the volume—with reminders that justification is an exercise of practical reason that may draw some solace from the Western tradition from which it issues.

As one surveys the whole of *Justification*, its steadfast commitment to analysis stands out, what with its several numbered distinctions, positions, levels, kinds, types, theses, and sorts. It is hard for a reviewer, and I expect it will be for a reader, to do justice to all the arguments because there are so very, very many of them. Attending this abundance, there is a complementary poverty of real practical examples, those in which justification is first called for and then made concrete in the defense of some particular action, law, or public policy. Analysis also tends generally to forget history. In this *Nomos*, the history of political thought is better served (thanks to Pennock, Dobel, and Spragens) than is the

social and political history that has brought us to this stand in our crisis of justification. The crisis of justification is not unrelated to the crisis of authority in our time. Perhaps the practical and historical dimensions of this crisis will be dealt with in volume XXIX as *Nomos* revisits the analysis of "authority."

Finally, the volume as a whole succeeds in nothing so much as lowering our expectations and hopes in virtually every arena, whether in the matter of liberalism, fundamental rights, ideal theories of justice, legal authority, objectivity, science, reason, or justification itself. What emerges politically is an exceedingly cautious, analytical, and minimalist defense of liberalism. Long gone are the proclamations of the Enlightenment, the Revolution, the New Deal, and the New Frontier. And the prospects that liberalism might regain its lusty confidence in itself or its foundations appear about as likely as its being saved by a return to justification *sola fide*. By no means is this a criticism of the book or the intelligence contained in it. But it is a sign of just how little we are confidently prepared to justify in our time.

JAMES FARR

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The Art of Political Manipulation. By William H. Riker (New Haven, CT: Yale University Press, 1986. xi, 152p. \$18.95, cloth; \$6.95, paper).

Among students of politics, William H. Riker is so closely identified with mathematical theories of collective choice that some in our discipline may be surprised at the appearance of a volume of political stories bearing his name. This compilation of twelve tales, spanning the ancient Roman Senate of Pliny the Younger and the contemporary U.S. Senate of Warren Magnuson, is rich with historical detail and personal anecdote. No equations intrude on the lively accounts of Lincoln's rhetorical victory at Freeport or Chauncey DePew's sly defeat of the Seventeenth Amendment, for the emphasis is entirely on the political art of exploiting agendas and rules. If one did not know better, the appearance of this small book might be taken as a sign that the well-known positivist had renounced his faith

in deduction to become a latter-day institutionalist.

But Riker's latest work is no reversion to traditional forms of political analysis; rather, it uses historical settings to illustrate the fundamentals of social-choice theory. The examples of political manipulation assembled here reflect the author's longstanding interest in grounding theory in observation, and many of them have figured in Riker's previous writings on Kenneth Arrow's Impossibility Theorem and Robin Farquharson's exposition of sophisticated voting strategies. While this diverse collection of episodes reveals Riker's extraordinary breadth of knowledge about political behavior, it also offers vivid pictures of how leaders in democratic regimes forge agreements when no underlying consensus exists.

Social-choice theorists like Riker have endeavored to discover lawlike regularities to explain political events, but they have deduced instead a world in perpetual disequilibrium. Under democratic norms of unrestricted preferences and nondictatorial rules, voting cycles are unavoidable, and strategic calculations determine the selection among rival alternatives. This systematic chaos means that political decisions that depend on voting schemes are inherently unpredictable and offer exceptional opportunities for leadership. As Riker observed in *Liberalism against Populism*,

The absence of political equilibria means that outcomes depend not simply on participants' values and constitutional structures, but also on matters such as whether some leader has the skill, energy, and resources to manipulate the agenda, or whether some back-bencher . . . has the imagination and determination to generate a cyclical majority. . . . These are matters of perception and personality and understanding and character. . . . [D]emocratic political outcomes . . . are an amalgamation that often operates quite personally and unfairly, giving special advantages to smarter or bolder or more powerful or more creative or simply luckier participants." (1982, p. 200)

The outgrowth of the indeterminacy Riker sees in democratic regimes is a type of political entrepreneurship he labels *heresthetic*. This is the art of creating successful coalitions by reframing alternatives, so that people are induced or compelled to join without necessarily being persuaded to the leader's point of view. Heresthetic is not scientific, but its

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power derives from the general laws of social choice that science has uncovered. Thus, it can be taught, and the lessons of one successful heresthetic experiment can be applied to different strategic situations whose underlying structures are similar. viewed in this context, *The Art of Political Manipulation* is not a scholarly work at all, but a manual "so that men of affairs may be instructed in this newly rationalized art" (p. xi).

Unfortunately, Riker's efforts to interest a commercial publisher in his text were rebuffed, and he probably will not reach the "men of affairs" who were his intended audience. This is regrettable at a time when the U.S. public is hungry for political leadership and the individuals who aspire to fulfill that role offer such an impoverished view of what it means to be a leader in a democratic society. As Riker's book illustrates in so many different ways, effective leaders are marked by their sense of timing, their understanding of historical precedent, and their capacity to redefine the dimensions on which public discourse takes place. More importantly, they take the trouble to master the intricacies of constitutional rules and legislative procedure in order to direct outcomes toward their political ends. Communication and persuasion are secondary to this view of leadership, and "new ideas" useful only insofar as they serve the leader's control of the agenda. One finds little in this book, therefore, to support the notion that a Ronald Reagan or a Gary Hart could be considered effective herestheticians, despite their skill in image making.

If Riker's book is unlikely to influence the general debate over the nation's political leadership, it nonetheless offers some useful lessons to political scientists. First, it is a welcome attempt to make the social-choice literature accessible to a wider audience. In addition, by presenting the problems of voting paradoxes in real-world examples, this small volume makes it more difficult for the critics of formal theory to dismiss the results of Arrow and Farquharson as mere artifacts of arbitrary and unreasonable assumptions.

In addition, Riker's stories offer a glimpse of the kinds of questions a mature theory of politics might begin to address. Instead of the sterile determinism of rational-actor models borrowed from economics, the discipline's most forceful advocate for positivist thinking in politics seems to be moving us toward a

more explicit evaluation of the influence of context on political outcomes. And as part of that context, he clearly sees the importance of art and innovation, which make the study of politics not a "dismal science" but an inquiry of perpetual surprise and variation.

LINDA L. FOWLER

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Analytical Marxism. Edited by John Roemer (Cambridge: Cambridge University Press, 1986. viii, 313p. \$39.50, cloth; \$11.95, paper).

Analytical Marxism is a volume in the series Studies in Marxism and Social Theory, edited by G. A. Cohen, Jon Elster, and John Roemer. Other books in the series include Jon Elster's *Making Sense of Marx* and Adam Przeworski's *Capitalism and Social Democracy*. These authors are also included in *Analytical Marxism*, in addition to sociologist Erik Wright, historian Robert Brenner, economist Pranab Bardhan, and philosopher Alan Wood.

Several themes link these essays together. One is the aim to restore economics to the central position in Marxian theory. While the contributors are philosophers, sociologists, historians, and economists, they uniformly spurn the temptation to take refuge in literary criticism, epistemology, the theory of the state, and chronicles of popular struggles to rescue Marxism from the weaknesses of Marxian economics.

But the economics of *Analytical Marxism* is surely not the economics of Marx. The labor theory of value, the centrality of class, the privileged position of the forces of production, the focal role of material interests in motivating social transformation, and many more traditional Marxian principles crumble before their analytical gaze. And while these writers are certainly not the first to confute these doctrines, their analyses are always fresh, insightful, and point to constructive research agendas.

The contributors to *Analytical Marxism* do not form a school. But they share enough principles to allow one to detect at least an "affinity group" that might possibly set the intellectual agenda for the next wave of progressive social theory. First, they have a uniformly hostile

attitude towards oppression and inequality and little tolerance for academic celebrators of the status quo and apologists for privilege. Second, they scrupulously avoid defending any particular part of Marxian theory as such and no doubt scorn those who waste their valuable intellectual resources sheltering Marx from his critics. Third, they all use modern analytical techniques, which Marxists have tended to reject as merely elegant obfuscation. Indeed, they not only use such techniques; they do so with obvious pleasure and consummate skill.

Technique in social science, like verbal skill in law, is as acutely capable of hiding as revealing. But abjuring technique is just as surely a kiss of death—one from which these “analytical Marxists” are carefully protected. John Roemer, whose major contributions to economic theory are carefully summarized in this volume, is a bold thinker whose craft could be ignored only by an economics profession in the United States dominated by an arrogant narrow-mindedness scarcely justified by the explanatory power of its cherished dogmas. Przeworski, Elster, Brenner, and Cohen are also represented by selections that, in addition to exploring traditional Marxian themes, reveal both their commitment to a research methodology little different from that of their non-Marxist colleagues and their considerable ability to work within it.

Perhaps the centerpiece of this book is Roemer's “New Directions in the Marxian Theory of Exploitation and Class,” in which he creates a general category of exploitation of which Marxian exploitation and neoclassical exploitation are special cases. Others are feudal, Socialist, and status exploitation. Roemer then proves what he calls the *class-exploitation correspondence principle* (CECP), which holds for economies with linear technologies and asserts that “every agent who is in a labour-hiring class . . . is an exploiter, and every agent who is in a labour-selling class is exploited” (p. 90). For more general convex technologies, if we attempt to define labor values independent from price (à la Morishima), then the CECP does not hold. Roemer shows that in order to ensure the CECP, the value of a commodity must be defined as the minimal amount of labor required to produce that bundle among all technologies maximally profitable at equilibrium prices. This shows

that Marx's political economy *logically requires* that price precede labor value in level of abstraction. Roemer proceeds to show that in a plausible array of cases, the transfer of surplus labor time through credit markets perfectly parallels the transfer through labor markets, thus undercutting Marx's assertion that capitalist exploitation occurs through the capitalist's control of the production process.

This combination of attachment to the traditional Marxian concern for “exploitation” in contrast to the more vogueish notions of alienation, domination, and heterogeneous forms of power and oppression, is characteristic of *Analytical Marxism*. It carries with it certain clear dangers. First, it entails the refusal to criticize some of the most bizarre aspects of Marxian dogma. For instance, class exploitation, even in its carefully extended Roemerian form and however important, is by no means the touchstone of social injustice and can scarcely account for the varieties of social action that make the world what it is. Consideration of race, gender, religion and the authoritarian state, not to mention war and nuclear destruction, are residual categories for these thinkers. Yet to avoid the heterogeneity of power is not “analytical” but simply selectively blind.

Indeed, the authors rarely give a spirited defense of their limited palette. They simply accept the single-minded preoccupation with exploitation and class as an inviolable “rule of the game.” Clearly, their silence on the broader issues means that the job of developing a political philosophy to replace traditional socialism remains to be accomplished. It is fair to say, I believe, that their willingness to borrow the *methods* of non-Marxist social theory is not paralleled by their willingness to borrow either the *moral concerns* or their *social content*. Yet I confess to being charmed by their insights, respectful of their theoretical openness despite a dedication to social justice that has led others to become hack apologists, and I am often dazzled by the sheer technical expertise that pervades these essays.

HERBERT GINTIS

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Destiny of Capitalism in the Orient. By N. A. Simoniya (Moscow: Progress Publishers, 1985. 252p. \$7.95).

From Afghanistan to China, Soviet-style socialism is a bad joke. The ascent of capitalist Japan and the newly industrializing countries (NICs) of South Korea, Taiwan, Hong Kong, and Singapore; the booming of China's partly desocialized economy; the stagnation of the state-controlled economies of Burma and Vietnam—for a Soviet observer bound to believe that socialism is superior and capitalism doomed, the recent history of the Asian-Pacific region is hard to explain away.

The Georgian scholar Nodari Simoniya, one of the brightest and most creative of Moscow's orientlists, resolves his cognitive dissonance by omission, assertion, and revision.

By entirely ignoring the reforms in China since 1978, Simoniya avoids the discomfort of imagining that socialism could be a reversible process. Citing stagflation and high oil prices (!), he finds world capitalism in "a wholesale structural crisis" from which it may not recover, and rules out on faith the possibility of its rejuvenation by the NICs (pp. 250-51).

The most interesting of Simoniya's methods, however, is to accommodate the reality by revising the preconception. Simoniya distinguishes between the "primary model" of capitalist development, exemplified by the earliest industrializers, such as Britain, Holland, and France (p. 117), and the "secondary model" represented by latecomers like Germany, Italy, and Japan (p. 122). In the latter case, unlike the former, "the political superstructure appears as the *chief system-forming factor*," that is, the state plays a much more active role, through revolution from above, in bringing about conditions of mature monopoly capitalism. It follows that the "tertiary model" now being forged by the Asian NICs may also reverse the vulgar Marxists' causal arrow from economic base to political superstructure.

Another of Simoniya's revisions is to blur and complicate the sharp distinctions of orthodox Marxism. He does this under the cover of an acceptably dialectical label: *synthesis*. For Simoniya multiple modes of production commonly coexist in the "compound" or "synthesized" society of a developing country, and

may do so, he implies, for long periods of time.

The consequences of this admission are potentially radical. For if each historical stage through which a country "must" pass is distinguished by one dominant mode of production—Asiatic, feudal, capitalist, whatever—a synthesis of capitalist and precapitalist modes endangers the whole notion that history moves teleologically through clear-cut phases. Simoniya of course reaches no such conclusion, but the net effect of his analysis is to undercut the historical optimism of official Soviet ideology.

The ubiquity and endurance of nationalism in developing countries makes it hard to explain as an epiphenomenon of a specific class. If in a synthesized society of coexisting capitalist and precapitalist modes of production no single class is clearly dominant, nationalism cannot be the superstructural projection of the bourgeoisie alone. Nationalism in developing countries becomes, for Simoniya, "an ideological reflection of an objectively existing universal interest in consolidating the nation and the state" (p. 83)—a tertiary-model, top-down, cross-class kind of "state nationalism," as he calls it, in contrast to primary-model, bottom-up, class-specific "bourgeois nationalism."

Ethnicity is another troublesome category for materialist Marxists, given its persistence independent of class pattern or historical stage (witness the anti-Russian riots in Kazakhstan in 1986). As with the state, Simoniya is willing to grant to ethnic identity a relatively autonomous role in the nation-state-forming process of late-developing countries.

Such revisions bring Simoniya to the brink of explicitly downgrading the importance of class. But he draws back at the last moment by asserting that "state nationalism" as a "universal interest" is "nearly always" used to cover up its "class essence" (p. 87). At this point, presumably, whoever vetted this manuscript for publication breathed a sigh of relief.

To summarize: This book illustrates both the opportunities for, and the limitations upon, the rethinking of Marxist categories in an Asian context from a Soviet perspective.

DONALD K. EMMERSON

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Seeds of the Seventies: Values, Work, and Commitment in Post-Vietnam America. By Arthur Stein (Hanover, NH: University Press of New England, 1985. 184p. \$18.00, cloth; \$8.95, paper).

Builders of the Dawn: Community Lifestyles in a Changing World. By Corinne McLaughlin and Gordon Davidson (Walpole, NH: Stillpoint, 1985. 372p. \$12.95, paper).

For those who believe that the end of the 1960s and early 1970s coincided with the extinction of progressive "movement politics" in the U.S., these two books offer an alternative perspective. For others, who have traced the process by which ex-movement political activists dropped out, were co-opted, or drifted, willy-nilly, into establishment political, economic, and social institutions, these books show that many individuals chose a different path, one that sustained the best of the (old) New Left, rejecting and abandoning the worst, while simultaneously struggling to create a new, post-Vietnam, post-Marxist vision still aimed at fundamental political transformation. And, finally, for those who have lost faith in the viability of a democratic, humane, flexible, and nondogmatic U.S. radicalism, where *radicalism* means "going to the roots" (psychological and cultural, as well as economic, roots), these books counter with portraits carrying the seeds—to use Arthur Stein's felicitous term—of hope.

Seeds of the Seventies is highly readable, thoughtful, and provocative—useful as a supplement in a wide variety of undergraduate courses and/or as a brief guide for political scientists curious about the "new-alternatives" subculture that has proliferated in the U.S. over the past 20 years. Stein provides a series of vignettes, concentrating on groups involved in producer and consumer cooperatives, active, nonviolent peace efforts, rural resettlement communities, appropriate technology, organic farming, holistic health centers, urban decentralist social-change collectives like the Movement for a New Society (MNS), human-growth communities, "deep-ecology" education, and "New Age" spiritual activities. He is a participant-observer in many of these efforts; accordingly, his work reflects the strengths and weaknesses of this approach.

As a modern-day Gandhian, Stein is sensitive to the egoistic tendencies in some of the

groups he examines, focusing on individuals and groups within the subculture who clearly are "other regarding" and politically engaged. Of the others—cultural rogues, grifters, naifs, failures—he has little to report. This bias is understandable, given the preponderance of ignorance and a Christopher Lasch-style conviction that all of it is egocentric and narcissistic. Still, the book is deficient in what usually is called balance. Nor does Stein seem comfortable with a more critical mode. His lengthy analysis of "The Farm," one of the largest and most clearly successful of the 1970s alternative communities, omits any but the most tepid criticism of the excesses that were going on there, and he does not provide any explanation of the community's virtual disintegration in the early 1980s. His intent is honorable—descriptive social science in the service of hope—but in his efforts to counter the largely mindless, "Rajneesh-is-coming" attitude toward the alternatives movement, his optimism undermines his capacity for detached and critical analysis.

What for Stein is only part of the alternatives movement (intentional communities), McLaughlin and Davidson examine in detail. *Builders of the Dawn* is a more comprehensive and useful study of postcommune communities available. It is well written, historically informed, and diligently researched. The authors, neither of whom is a social scientist, nevertheless are gifted field researchers. They spent 10 years traveling and visiting communities, mainly in the U.S. but also abroad, and here report the results, along with solid, practical advice to would-be or ongoing communarians. Like Stein's volume, this book is particularly appropriate for a variety of undergraduate courses and as a reference for scholars. McLaughlin and Davidson omit coverage of the large, cultlike communities of modern life, preferring to examine "mainstream" alternatives. They estimate that there were approximately 400 such communities functioning (circa 1984–85) in the U.S. They visited 105 and write in depth about 30.

Many of these communities see their role as broadly educative for people outside the community movement, and actively mount outreach programs, workshops, and the like concerned with experimental new forms in governance, ecology, energy utilization, conflict resolution, family structure, business manage-

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ment and so forth. The communities reflect enormous diversity, in the goals they pursue, their internal structures, and in the types of people attracted to them. In this and other ways, the authors assert, modern communities are not so much alternative as they are co-evolutionary with the more conventional institutions of advanced industrial society. Most, however, operate on the assumption of nourishing far-reaching changes in the values and behavior of people in this society, now and in the future. The seeds, as these books document, are well planted; indeed, many have long historical traditions. Whether they will continue to grow, and with what consequences, political and otherwise, is still an open question.

JEFF FISHEL

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When Marxists Do Research. By Pauline Marie Vaillancourt (Westport, CT: Greenwood, 1986. xvii, 205p. \$29.95).

Pauline Vaillancourt has tried to render a service to "non-Marxist social scientists" by providing an ambitious road map to the epistemological and methodological assumptions of, and views of science in, Marxist research. She also examines how these views affect Marxist "research strategies" and their relation to policy-making. Vaillancourt's beginning is promising: "Marxists possess a rich tradition, a body of experience, accumulated over a hundred years and more, which non-Marxists have for the most part ignored" (p. 1). Unfortunately, that richness does not emerge in the ensuing analysis.

Vaillancourt's strategy is to compare the assumptions of Marxist research with those of the "generally accepted model" in contemporary social science. She summarizes that model as materialist, determinist, empiricist, positivist, value neutral, objectivist, and atomist and as accepting the methodological priority of induction, empirical observation, direct causality, the unity of natural and social science, the fact-value split, and the ideological independence of science.

Against that model, Vaillancourt judges the efforts of four categories of Marxists: the *philosophics* (e.g., Korsch, Lukacs, Reich,

Sartre, Merleau-Ponty, Marcuse, Adorno, Habermas); the *materialists* (e.g., Hilferding, Kautsky, Bernstein, Plekhanov, Bukharin, Bottomore, Miliband); the *structuralists* (e.g., Althusser, Lacan, Godelier, Therborn, Poulantzas, Foucault, Harvey); and the *deductivists* (mostly Stalinist theorists). This categorization is not without merit, but one might just as well distinguish dialectical, critical, humanist Marxism, on the one hand, and positivist (both empiricist and ideological) Marxism on the other.

In any event, after repetitiously summarizing the general assumptions in those four categories, Vaillancourt's conclusion is unsurprising and simple: the materialist (read *positivist*) Marxists "do research" that is of most interest to non-Marxist (read *positivist*) social scientists. The "philosophics" are least interesting because they reject almost all the tenets of the "generally accepted model." The structuralists (at least the non-Althusserian ones, such as Wright and Poulantzas) are somewhat interesting insofar as their work becomes more empiricist and positivist. And the deductivists are without any value since all they do is dogmatically apply the doctrinal principles of Marx, Engels, and Lenin (here Vaillancourt is on the mark!). Only the materialists are unequivocally of value, because *only* they "accept causal explanations" or "direct causality" (p. 71), "conceive of practice as the application of theory" (p. 58), "accept the content of modern science" (p. 99), "do defensible research" (p. 148), and are "able to provide the tools needed . . . to formulate policy . . . to improve the human condition" (p. 178).

Vaillancourt's project is admirable but flawed. I will mention only a few of the book's problems. First, there is a circular, tautological (even self-fulfilling prophetic) character to the work. The standard of research is contemporary, positivist social science; only the materialist-positivist Marxists meet that standard; hence only their research is defensible and of interest to non-Marxist positivists. Second, Vaillancourt's positivist blinders narrow her vision to the point where she presents a caricature of the "philosophical" Marxists. She attributes to them such positions as that the "external world is merely an artifact of consciousness" (p. 20); it is possible to "will a situation into existence" (p. 24); research should emphasize "subjective personal experi-

ence" (pp. 33, 44); and data are an "arbitrary creation of the human mind" (pp. 55, 66). The philosophics, she says, "confound science with technology, reason and rationality" (p. 103). To characterize the thought of Gramsci, Merleau-Ponty, Kolakowski, Habermas, Marcuse, and others in this fashion is unfair and misleading. Related problems include a continual covert use of "real" or "reality" as something self-evident and indisputable (pp. xvii, 7, 18, 29, 65, 166), an unquestioned acceptance of the distinction between the normative and the empirical, and a portrayal of Meehan's instrumental conception of policy-making as beyond debate.

There is much that is worthwhile in Vaillancourt's book, such as her discussion of the materialists and deductivists, which clarifies both pragmatic and ideological Marxism. More importantly, she provides a service by subverting the myth of Marxist theory and research as monolithic. But the benefits must be measured against the problems, especially the distorted picture of nonpositivist Marxism, which has produced some of the most creative social and political theory of our century. In terms of research, we are told that non-Marxists have almost nothing to learn from deductivist, Althusserian-structuralist, or philosophic Marxists (pp. 179-80). What I fear most is that Vaillancourt's well-intentioned book might do more to close off serious dialogue in political theory than to stimulate it. And that would be a loss.

SCOTT WARREN

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Biology and Bureaucracy: Public Administration and Public Policy from the Perspective of Evolutionary, Genetic, and Neurobiological Theory. Edited by Elliott White and Joseph Losco (Lanham, MD: University Press of America, 1986. xvi, 633p. \$32.75).

For at least 15 years a small set of political scientists has been arguing at conferences and in print that political science—and especially behavioral political science—must pay closer attention to the research findings, methodologies, and theories of the biological sciences. Put most generally, the reason given for this

exhortation is that humans, like other species, are subject to general biological processes and that their behavior and institutions reflect the workings of those processes. Among the subsidiary points commonly found in one or another version of this argument are (1) that humans have a genetic heritage that influences their politics and government; (2) that animal models have served psychology and medicine well and may play a similar heuristic role for political science; (3) that brain research is yielding findings which are of significance for all students of human behavior; and (4) that field and lab methodologies from the biological sciences can be useful in the investigation of political phenomena.

The present volume, a collection of 18 papers, in large part continues these themes. Yet, like all that has gone before, this volume seems unlikely to have much influence on the course of political research. This is not, in my judgment, because the general views expressed in this collection and in this literature are rejected by the rest of political science. Actually, the main arguments are not really controversial at all. Who can doubt that humans have a genetic heritage, that animal models may provide suggestive insights, or that neuroscience offers important behavioral findings? But a very reasonable response to such arguments is, so what? And it is at that point that this literature often falters.

I believe there are three interrelated reasons for this. First, there is in this literature a preoccupation with (biological) independent variables. Concern about (political) dependent variables, with respect both to specification and measurement, too often seems an afterthought at best. And yet it is obvious that any discipline must focus on what it wishes to explain. Hence, we should expect research in political science to be launched by political questions which then may (or may not) lead to biological answers, not the other way around. Second, the biological argument offered in this literature is usually much too general to be of even theoretical utility to those doing empirical research on political life. Typically, only a long and speculative line of reasoning connects biological argument to individual or collective political phenomena. Third, by and large the contributors to this literature have yet to lead by example. Where is the systematic work showing the empirical links between a political

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dependent variable and biological independent variables (other than *age*, which is of ambiguous status as a biological variable)? Where are the studies demonstrating the value of animal models? And where is the research that offers convincing evidence of the importance of embracing any of the methods of the biological sciences?

Like the earlier contributions to this literature, the present volume presents many interesting ideas. (Despite the volume's title, those ideas have less to do with bureaucracy than with organization in general and less to do with organization than with human nature in general.) But, especially when taking a new direction in research, it can be a long and very difficult journey from interesting ideas to systematic investigation. Until that journey has been made—and, given the need to overcome disciplinary habit and inattention, probably made repeatedly—in persuasive fashion, I expect that political science will continue largely to ignore volumes like this one.

DOUGLAS MADSEN

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Classes. By Erik Olin Wright (London: New Left Books, 1985. 344p. \$25.00, cloth; \$9.95, paper).

Class, Politics, and the Economy. By Stewart Clegg, Paul Boreham, and Geoff Dow (London: Routledge & Kegan Paul, 1986. viii, 451p. \$59.95).

Toward the beginning of their book, Clegg, Boreham, and Dow highlight two predictions of Marx and Engels regarding classes in capitalist societies. The first is that the class structure will become increasingly simplified as intermediate positions between the two great classes, bourgeoisie and proletariat, disappear. The second is that the working class will inevitably evolve from a "class in itself" into an organized, self-conscious "class for itself," capable of challenging bourgeois dominance. Neither prediction has proven to be accurate in any unambiguous sense.

The failure of the class structure and class conflict to follow the trajectory anticipated by Marx and Engels sets the agenda for most current Marxist writings on class, including the books under review. Erik Olin Wright has

been working on the problem of the middle class in Marxist theory throughout his professional career. How should the large and heterogeneous category of occupations located between the shareholders and the workers on the shop floor be conceptualized? In earlier work, summarized in *Classes*, Erik Wright went through a variety of classificatory schemes whereby relations of exploitation were distinguished from relations of domination in production, and occupations in which the two sets of relations did not correspond were labeled contradictory. In the present book, Wright refashions his classifications in line with John Roemer's reconceptualization of exploitation in *A General Theory of Exploitation and Class* (Cambridge: Harvard University Press, 1982; for Roemer, cf. p. 983 above). At the heart of Wright's use (and revision) of Roemer's work is a distinction between four types of productive assets: labor power, capital, organizational assets, and skills. Feudal exploitation is characterized by inequalities in the ownership of labor power; capitalist exploitation by inequalities in the ownership of capital; statist exploitation (such as exists in the Soviet Union) by inequalities in the possession of organizational assets; and socialism will still be characterized by inequalities in the possession of skills. However, while capitalist exploitation is dominant under capitalism (and feudal exploitation is illegal), exploitation based upon organizational assets and skills also exists. Contradictory locations, therefore, consist of occupations in which occupants are exploiters in terms of one type of asset while being exploited in terms of another.

The theoretical advantage Wright claims for his framework centers on his ability to define contradictory class locations exclusively in terms of exploitation as opposed to domination. Yet both organizational assets and skills present problems, as Wright acknowledges. To take the case of organizational assets, the question becomes in what sense does a position of authority in a corporation give one control over resources for personal use distinct from ownership of capital or the supply of labor. Unless Wright is willing to subscribe to the thesis of the separation of ownership and control in modern corporations, a thesis persuasively criticized by Clegg, Boreham, and Dow, it is hard to interpret organizational-

asset exploitation as anything other than domination in production. Moreover, why limit the categories of assets to four? Why not add ownership of land to the list? There is less theoretical justification for the scheme than meets the eye.

So how does it do empirically? Wright contrasts his definition of the working class with two alternatives: a much narrower definition offered by Poulantzas and the commonsensical definition of workers as manual laborers. The test consists of comparing the degree to which the alternative definitions correspond to differences in income and in class consciousness (i.e., in responses to survey questions with a "clear class content" [p. 146]). Income is less relevant from a Marxist perspective: Marxists have never claimed that the poorest are the most exploited. My reading of the data on attitudes is that Poulantzas, not Wright, has the most compelling criteria for distinguishing between marginal and core members of the working class.

Nevertheless, reading Wright's book was both enjoyable and rewarding. Wright combines serious theoretical and empirical work on the class structure in advanced capitalist societies. There is, at the end, a fascinating comparative analysis of a survey on class attitudes in the United States and Sweden. In addition, Wright has the rare virtue of openly acknowledging and discussing the difficulties that

appear in his analysis.

While Wright is concerned only with determining the appropriate manner of mapping occupations into classes, Clegg, Boreham, and Dow attempt a grand overview in which the organization of classes into political actors with distinctive economic programs is given equal weight with the map of empty places in production. There are chapters on Smith and Ricardo, Marx, Weber, the petty bourgeoisie, the corporate ruling class, the new middle class, the working class, the state, and economic-policy options. Much literature is surveyed, often with intelligent comments. However, the book lacks a clear theme or analytic structure to tie the disparate material together. The writing is frequently difficult to read. Most importantly, the authors are ignorant of economic theory since the Kalecki and Harrod-Domar growth models. The authors urge adoption of a policy of securing full employment and growth through "enhanced purchasing power, enhanced real wages and enhanced participation in decisions affecting the dynamics of the economy" (p. 332). A Left government might accomplish any one of these goals, but the attempt to achieve them all simultaneously is a prescription for disaster.

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AMERICAN POLITICS

Bureaucratic Responsibility. By John P. Burke (Baltimore: Johns Hopkins University Press, 1986. x, 280p. \$28.50).

This work addresses what in the literature of public administration has often been designated the politics-administration dichotomy, though the author does not use this term or much refer to this literature. Grounded chiefly in moral philosophy and political theory, the work seeks a joining of politics and administration in a theoretically informed and defensible manner.

Self-aware public administration developed

in and after the progressive period as a body of procedures and techniques aimed at the effective and efficient realization of policies decided upon by the designated organs of constitutional and—increasingly but still limitedly—democratic government. However, by mid-century it was widely acknowledged that persons in administrative agencies "make policy" in various ways, and a varied literature was developing that sought to justify this policy role. But given the historical context, and especially a constitution that distributes power in a complicated manner and does not contain the word *administration*, the task of legitima-

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tion has been far from easy. No rationale has achieved more than limited acceptance, even within the community of public administrationists.

Bureaucratic Responsibility is addressed to the problem of the legitimacy and proper role of public administration—of *bureaucracy*, in its own terms. As I shall make some critical observations, I wish first to emphasize the positive. This is a work of high quality, a welcome addition to the literature and, more generally, to the stock of ideas with which we think about the part of government that is quantitatively the largest and for the most part does the governing. Unquestionably, it is a contribution.

The central argument of the book is that “individual responsibility, properly defined, offers a fruitful strategy for reconciling bureaucracy and democracy” (p. 216). Individual responsibility is “properly defined” by the “institutions and processes” (p. 216) of democracy. This mainly means representative democracy in the Western mode. In the U.S. national government, it means the predominance and authority of the three constitutionally ordained “political” organs. Beyond these, but not in contradiction to them, there is an obligation to the ethos of democracy, the “enterprise as a whole” (p. 216).

But it must not be presumed that this “background” obligation licenses a bureaucrat to act against, or even (if I understand correctly) in the absence of, the will of the political authorities. Bureaucrats have an obligation to assist and facilitate policymaking by such means as supplying information, calling attention to errors and problems, and promoting processes that embody democratic values. “The kind of intervention this conception of responsibility defines differs from the bureaucratic activism other approaches encourage. It does not seek to redefine the bounds of politics, impose some particularistic set of policy aims, or encourage bureaucrats to make policy decisions on their own. It attempts only to correct deficiencies” (p. 57).

Some themes and observations: (1) The author is at pains to distinguish his conception of democratic responsibility from the strict formal-legal approach to bureaucratic responsibility: “Unlike the conception of responsibility I will construct, a formalist approach defines democratic allegiance too narrowly”

(p. 11). The responsible bureaucrat is not a defensive rule follower, but a positive democracy promoter—always within the direction and control of the proper democratic authorities. (2) The “bureaucratic activism” referred to above includes writings that emphasize the representative nature and function of bureaucrats, the writings that promote “participatory” administration, and the writings that assert a policy-determinative role for professionalism and expertise. Representation, participation, and professionalism can perform useful functions, true, but only with authorization of, and within guidelines specified by, legitimate authorities. (3) There is much that is “conservative” in the argument. Bureaucrats must restrain their impulses to promote their own conceptions of either liberty or equality as against, or in the absence of, authorized democratic policy. Claims of morality and promptings of conscience derived from nongovernmental sources cannot normally be allowed to dictate bureaucratic authority. But at the margin, yes. Notably there is the Nuremberg exception. “Political ends, even if they result from ‘perfect’ democratic processes, cannot justify all means. Some moral principles are still needed as a check on politics” (p. 167). (4) A fair number of “cases” and examples are used to illustrate points and reinforce arguments. Their use to illustrate problems of conduct like whistle-blowing, appeal to higher authority, and resignation in protest, is skillful, and the arguments largely persuasive.

My reservations relate to the premise that democratic theory taken as a basis for bureaucratic responsibility provides “answers” in the manner presumed. Matters are, I think, more complicated; complicated by history, by constitutional architecture, by custom, by political dynamics, by social and economic factors. Of these there is too little recognition. The “prescriptions” may be basically acceptable and may provide a guide to a better future—I should like to think so. But they often seem idealistic, unrelated to reality. Things are as they are for *reasons*, and they will not be quickly changed, if indeed they can be changed, by counseling bureaucrats to do this or refrain from that on the basis of democratic theory. As the Constitution does not contain the word *administration* (much less *bureaucracy*), it does not contain the word *democracy*.

The work does not, I think, respond to the

complexities and perplexities, to the *density* of political-bureaucratic reality as it exists in the federal government (on which the discussion centers). The terminology moves easily and, to me, confusingly among *bureaucrat*, *official*, and *bureaucratic official*. It is not clear to me whether, for example, commissioners of regulatory agencies, the governors of the Federal Reserve system, administrative law judges, prime contractors, and the sizable throng of "in-and-outers" are part of the (democratic?) political order or are bureaucrats.

To this the author might respond that, to be sure, there are many positions in which the roles of political authority and bureaucratic function are mixed, but that the prescriptions offered will assist in distinguishing between the two and securing the proper relationship between them. Perhaps. But I have the feeling that the many "shoulds" are mostly unrealistic, rather like advising someone with sore feet to walk a little way off the ground. For example, if "political leaders" set conflicting objectives or make unreasonable demands, "bureaucrats ought not determine which of several competing demands they should follow, nor should they themselves 'scale down' impossibly high expectations. They should, however, refuse to be 'passed the buck,' and they should encourage higher authorities to make proper decisions" (p. 51). Indeed.

I leave open—as of course I must—the question whether this is another version of the politics-administration dichotomy or a promising formula for its elimination. Or, conceivably, both. In any case I reaffirm my opinion that this is a work that warrants careful reading and serious reflection.

DWIGHT WALDO

Syracuse University

Affirmative Action: Theory, Analysis, and Prospects. Edited by Michael W. Combs and John Gruhl (Jefferson, NC: McFarland & Co., 1986. 185p. \$19.95).

This book consists of 10 original papers that were presented at the Eighth Annual Hendricks Symposium sponsored by the Department of Political Science at the University of Nebraska, Lincoln in 1983. Because this is not a book by one author, there are variations in style,

approach, and emphasis, but more than the cover holds it together. In general, the authors support affirmative action and their papers are good.

The book is divided into four parts plus an introduction. The introduction defines *affirmative action* as a remedial process consisting of "specific and result-oriented procedures . . . to promote and achieve equal employment opportunity," a means rather than an end. Part 1 is the "theory" section and examines affirmative action in the context of economics and of "traditional" values like individual rights and the merit principle of employment. In what was for me the most thought-provoking paper in this part, Richard Young distinguishes between procedural and substantive racism as what might be termed *de jure* versus *de facto* racism and argues that "once procedural racism has been outlawed, the number of jobs available in the economy has far more influence than either applicant competency or employer bias on whether or not black workers can find decent jobs" (pp. 14-15). "Reverse discrimination" (p. 17), or goals and quotas (my words), will be counterproductive and will fail; what will work is a national economic policy "committed to the achievement of a full-employment economy and the elimination of poverty in the United States" (p. 18). In other words, a procedural solution can remedy procedural racism but a procedural solution cannot remedy substantive racism.

In another interesting paper in part 1, W. R. Newell tries valiantly, though with admittedly limited success, to reconcile affirmative action with the theory of liberal democracy, relying upon Locke, Rousseau, and Rawls. Space prohibits a synopsis of this analysis; suffice it to say that he finds Locke least amenable to affirmative action, and the others less than enthusiastic. "The American public's antipathy toward affirmative action thus has a formidable philosophical antecedent" (p. 57)—as does that of the Reagan administration, one might add.

Parts 2 and 3 are the "analysis" part of the book and are solid empirical studies that examine affirmative-action implementation at various points in time and at various levels of government. Of all the papers in the book, these are, perhaps, the most similar to other studies. They examine such variables as salary, position, local versus state versus national

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government, economic and population growth, veterans' preference, and the presence or absence of a centralized personnel system and an affirmative-action officer. The results are expectedly congruent with those of other studies of this genre but nevertheless represent a valuable demonstration of the multivariate nature of achieving affirmative action. Perhaps most significant is Saltzstein's finding that certain personnel policies and organizational arrangements do make a difference. Hence, the adoption of an affirmative-action plan may and often will need to be accompanied by changes in personnel policy and organization. Also worthy of note are Eisinger's findings that "the most important factor in explaining expanding black job opportunities is population growth" and that "there is no evidence . . . that reduction of unemployment would lead to more minority jobs in the public sector" (p. 124). This would not necessarily conflict with Young's advocacy of an economic solution because Young is thinking in terms of the economy as a whole rather than the public sector alone.

Part 4 is the "prospects" part of the book and deals with the future of affirmative action in light of the current political environment. It is clear the authors of these papers do not support the affirmative-action policies of the Reagan administration and that they are concerned about their residual effects after Reagan leaves office. The Plant and Thompson paper on the Equal Employment Opportunity Commission is a particularly insightful analysis of policy implementation by an administrative agency.

In sum, this book presents much valuable information in a concise and convenient format. It could be profitably used in courses in public policy, public personnel administration, ethnic politics, and civil rights. Its value is enhanced by good lists of references at the end of each paper and by a comprehensive index.

WINFIELD H. ROSE

Murray State University

Right Turn: The Decline of the Democrats and the Future of American Politics. By Thomas Ferguson and Joel Rogers (New York: Hill & Wang, 1986. 276p. \$19.95).

Recent changes in U.S. politics have not been kind to conventional theories of the democratic state. According to the received wisdom, majority opinion determines what government does. But at least since the late 1970s, the Republican and Democratic parties have pushed domestic and foreign policies further and further to the right of what most citizens want. How could a policy realignment this deep and profound have happened with so little mass support? And why have the Democrats—ostensibly the party of internationalism and social reform—helped to bring it about? In *Right Turn*, Thomas Ferguson and Joel Rogers take on these critical questions. In answering them, they provide a powerful reinterpretation of U.S. politics and a path-breaking work of theoretically informed, empirical political economy.

The analytic heart of the book is a simple but elegant model of party politics that stands most theories of electoral competition on their head. Borrowing from, while substantially modifying, the rational-actor approach to social action, they view politics in market terms; but reject the assumption that party systems are driven by voter demands. The market for political parties in the U.S., they maintain, is not individual voters, but "major 'investors'—groups of business firms, industrial sectors, or, in some (rare) cases, groups of voters organized collectively" (p. 45); only they have sufficient resources to contest effectively for state power and sufficiently well-defined interests in doing so. On this premise the authors build their "investment theory" of politics: shifting patterns of allegiance among groups structure the environment in which public officials choose policies and voters choose parties. These allegiances, not voter loyalties, precipitate policy realignments and account for the bulk of what political parties do.

The authors carefully examine a vast array of materials, including campaign contributions, to substantiate their claims. Based on these data, they persuasively demonstrate that changing investor coalitions do, in fact, explain the rise of the Right and the decline of

the Democrats. Ferguson and Rogers make clear that the United States' flight from liberalism does not derive from public antipathy to welfare, regulation, or peaceful coexistence with the Soviet Union but from corporate responses to changes in the U.S. and world economies. In the late 1970s—when the right turn occurred—the relative decline of the United States in an increasingly competitive and integrated world market and growing U.S. involvement in the Third World made the multinational, capital-intensive firms and internationally oriented investment and commercial banks that had supported social liberalism and detente more cost conscious and bellicose. These corporate interests, not the mass public, moved to the right and embraced conservative politics. These same interests have driven the Democrats to ape, rather than challenge, Reaganism.

Rigorously and subtly applied, the investment theory of politics yields a host of other important insights. Several heretofore opaque aspects of the 1984 presidential campaign are clarified, including the rise of John Glenn and Gary Hart as presidential candidates, Walter Mondale's self-crippling decision to campaign on a platform of tax increases, and the Democrats' failure to mobilize, let alone widen, their potential base. Other changes, such as the success of the environmental movement in the 1960s and 1970s and revisions of the tax laws are also explained. The future of the party system is also illuminated. The authors show that efforts to revive the Democratic party as a vehicle for liberal reform are likely to fail because its business supporters are no longer willing to make the concessions to its mass base necessary to mobilize it. In the absence of a Democratic upsurge from below, or a catastrophic economic crisis, the United States' right turn is likely to continue.

Right Turn succeeds on every level. It tells a fascinating political story and tells it extremely well. The theoretical approach is powerful. The interpretation of recent trends in the political economy is new and convincing. It will become a standard for election books—indeed, it stands head-and-shoulders above other work in the field—and should be read by anyone who hopes to make sense out of current U.S. politics.

CHARLES NOBLE

Rutgers University

The Dream of Deliverance in American Politics. By Mona Harrington (New York: Alfred A. Knopf, 1986. x, 308p. \$19.95).

Mona Harrington has written an interesting and challenging book: interesting because of the lively way in which she treats the familiar narratives of U.S. political history; challenging for two major reasons. First is the perspective she brings to bear on these narratives, and second is her willingness to deal with these in the context of the central issues of our time: war and peace, poverty and prosperity.

Harrington's argument is that the real goals of U.S. democracy are the promises of liberty and equality. Our realization of these goals is hampered, in part, by the division of the U.S. industrialized community into permanent and conflicting interests, managers of the great corporate concentrations, wage earners, and locally fixed producers (including, especially, agricultural interests). But the problems of U.S. democracy, as Harrington understands them, arise primarily from *misunderstandings*, from the propensity in all citizens to suppose, no matter what their real interests, that through some simple arrangement of the rules of the game or some equally simple alteration in policies or of personalities at the helm of state there can be a quick and final deliverance from evil. This "dream," that for every age there is some one quick and easy way out, Harrington calls a myth, and until we can put it aside, there can be "no escape from incoherence" (p. 274) in the national political dialogue.

The prospects of this happening are small, but Harrington is not without hope. Of the three interests into which she sees the U.S. industrial community divided, one, the wage earners, constitutes a genuine majoritarian interest and, moreover, one that has fundamental commitments to both a national interest and the forms of democracy. If this interest could be unburdened of myth, if it could be brought to a realistic understanding of the limits of modern life in which there are no real winners and all must share equitably in pains and losses, then, through presidential mandates and leadership, the United States might move toward a reasonable realization of its real goals.

This argument is persuasively and fluently set out in the book's opening and closing chapters, and massively sustained in the intervening

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chapters that define the nation's interests and examine their influence on the nation's politics through the years of the New Freedom and the New Deal, the pre- and post-World War II days, the failures of the "war on poverty" and in Vietnam, and the reactions of Carter and Reagan. But one measure of the interest and challenge in all this material is the questions it leaves hanging.

For a book so searching, it seems strangely willing to take uncritically the forms of U.S. political institutions as they stand. The brief and undeveloped reliance on presidential government as a possible hope for the future is an admittedly small but nevertheless telling symptom of this, and it smacks of James MacGregor Burns's contention of several years ago that the nation needs a president who would govern with the Congress if it would and without it if it would not. Where does Congress, with all that it is and represents in the standing U.S. democratic scheme of things, fit in Harrington's scheme of things? And what is to be done with the fragmentation represented by the U.S. system of state and local government?

Then there is Harrington's ready acceptance of what this reviewer would term *liberal* democracy as the norm for the U.S. polity. Liberty is the core value of that democratic vision, and Harrington has a firm grasp on its dimensions. But what of equality? Is it simply another value of liberal democracy? Is it to be understood only by reference to equality under the laws that give us our liberties? What does Harrington mean by the logically incongruous phrase "maximum equality" (p. 273)? Even in the predominantly atomistic U.S. liberal tradition, is there not an ancillary context of ideas that stress egalitarianism in terms of social membership and fellowship? Is there a possibility that at the level of what Harrington terms the United States' "real" goals, there are fundamental contradictions that transcend "interests" and that give shifting meanings even to the concept of *liberty* itself (freedom from, . . . freedom to, . . . freedom with . . .)?

Finally, there is the problem of myth, specifically of political myth and the needs and urgencies to which it responds. Harrington defines *myth*, "in its broad sense as a general system of belief—a system of values and attitudes that define the general political norms of a nation, its fundamental goals, national con-

ceptions of what is right, and also expectations as to what is achievable" (p. 15).

That is a reasonable definition, but it is clear that only its last clause is significant for Harrington in this book. Moreover, in the book's thematic argument, myth is mostly to be associated with misunderstandings, obscurantisms that conceal reality and truth, mental cobwebs that should be blown away. Even in this narrow sense, are we to suppose that in the range of U.S. politics, the myth of deliverance from evil is the only important myth needing to be blown away? That question raises others. If we go back and take the whole of Harrington's definition of myth seriously, can we be certain that the deliverance myth, for all its faults, does not also have important, socially constructive functions? If we rid ourselves of it, have we other myths to do its job?

H. MARK ROELOFS

New York University

The Ultimate Insiders: U.S. Senators in the National Media. By Stephen Hess (Washington: Brookings Institution, 1986. xviii, 151p. \$22.95, cloth; \$8.95, paper).

The Ultimate Insiders is a study of which senators are covered by the national media and *why*. Stephen Hess makes no pretense in this book—the third short volume since 1981 in his Newswork series for the Brookings Institution—of providing a complete analysis of the relationships between senators and the national media. The preface sets the tone with its caveat that "this book is essentially about the press, not about Congress." Despite this self-limitation, Hess's conclusions are sure to stimulate fruitful discussion among legislative specialists, as well as among students of the media.

Everyone who knows Congress will agree that power in the Senate and House is more widely dispersed now than it was three decades ago. The question Hess addresses is whether the media have contributed to this decentralization by giving today's junior Senators more coverage than their peers of the 1950s. Hess concludes, contrary to some conventional wisdom, that reporters do not run to cover every new blow-dried senator who tries to capture the press's attention. If anything, Hess says,

nonleaders get less coverage now than they did 30 years ago.

Hess measured coverage with a National Media Index for 1983 that ranked senators by the number of times their names were mentioned on a network-television evening news program or in five major national newspapers. (Additional points were awarded for personal appearances on the network news and on the networks' Sunday interview programs.) The rankings showed that the top 10 senators got half of the Senate's total national press coverage in 1983, the top 20 senators got two-thirds of the coverage, and the bottom half of the Senate got barely 10%. From the rankings, Hess concludes that

1. most senators receive so little attention "that the national news media are irrelevant in affecting their elections or promoting their policies."

2. the national news media concentrate on the Senate's leaders—party leaders, full-committee chairmen and ranking minority members—the only notable exceptions being presidential aspirants.

3. "rather than being a destabilizing and decentralizing force, scattering attention among a wide range of junior legislators, the national news media focus on those Senators who seem to wield institutional power." This conclusion and the one above parallel Timothy E. Cook's for the 1977–80 House in "House Members as Newsmakers: The Effects of Televising Congress" (*Legislative Studies Quarterly* 11(2): 203–26).

4. "the coverage of the national news media from 1953 through 1983 has increasingly directed attention to the definable leaders at the expense of the nonleaders, mavericks and others"; after comparing the top 20 senators in his own 1983 ranking with the top 20 in rankings prepared by some earlier scholars, Hess concludes that nonleaders made up half of the top 20 in the 1950s and 1960s, a third of the top 20 in 1974, and only a fifth of the top 20 in 1983.

My own reading of Hess's 1983 data would lead me to state these last conclusions somewhat less starkly. The top 20 senators included 4 presidential candidates (Glenn, Cranston, Hart, and Hollings) who received 27% of the Senate's total coverage. If we remove these 4 (2 leaders and 2 nonleaders) and look at the top 20 who were covered as senators and not as

presidential candidates, we find 5 (Kennedy, Helms, Moynihan, Mathias, and Laxalt) who generally were quoted for reasons unrelated to their formal leadership positions and 4 (Dodd, Tsongas, Kasten, and Metzenbaum) who held no leadership positions at all. That leaves only 11 of the 20 who clearly fit Hess's notion of a leader getting national coverage by virtue of a leadership position.

The contemporary importance of non-leaders becomes even more apparent when we look at some interview shows that appeal to a narrower audience—such as "Nightline" (ABC), "The MacNeil-Lehrer Nightly News Hour" (PBS), and the like. One of Hess's tables draws upon data compiled by the Senate Republican Conference and ranks senators according to their appearances on these shows. Four of the top 7 positions in 1983–84 were filled by the 4 presidential candidates, with the remaining 3 taken by Dole, Hatch, and Dodd. The fourth-place Dodd, a nonleader, ranked well above Howard Baker, the majority leader. Baker was tied for eighth and ninth place with Paul Tsongas, another nonleader. Positions 10 through 27 on this list included 8 leaders (counting Moynihan and Laxalt) and 10 non-leaders. The top 27, in other words, included only 8 senators who were getting national coverage by virtue of their formal leadership positions within the Senate.

This restatement of Hess's data does nothing to harm his conclusions that leaders are more likely to be covered than nonleaders, or that most senators get little national press coverage, whatever their press releases or other theatrics. But it does suggest that senators who want national reputations can get them—as Hess himself acknowledges—by becoming major players on issues the press considers to be important. The press may not be the reason why these nonleaders gain prominence; on this matter, Hess is surely right. The points, though, are (1) that gaining power inside the senate and gaining recognition outside no longer conflict and (2) that both are available to nonleaders.

When Hess tries to explain the change in press coverage from the 1950s to the 1980s, he speculates about why junior senators may no longer be as attractive to reporters as they once were. This may be the wrong place to look. Since junior members continue to gain attention when they earn it, the decrease in cover-

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age given to junior members may not need explaining as much as the increase in coverage given to leaders.

Hess seems to flirt with this observation in a single paragraph. The Senate of the 1950s, Hess notes, was populated with a number of "inner-club" powerhouses who received, and wanted, almost no coverage in the national press. Their behavior, by default, left a news gap reporters had to fill with the nonleaders. Today's most powerful senators, in contrast, almost all fall in the top sectors of Hess's rankings. This suggests a basic change in the way leaders do their work. Why has this happened? Here are a few competing explanations:

1. The press is better at identifying who should be covered.
2. Today's leaders are either recruited from among people who have a greater personal appetite for press coverage than their peers of the 1950s or socialized to develop this appetite in the contemporary Senate.
3. The new leaders are less likely to be satisfied than the old with their current positions—that is, more of them want to run for higher party office and/or for the presidency. In addition, the national press is a more important part of the strategy for such a race than it used to be.
4. Stating one's position in the national press, and using the press to channel information or mobilize networks of support in Washington, are more important to exercising power *within* the Senate than they used to be.

All of these explanations have some plausibility. Except for the first, they go well beyond Hess's concentration on the press. Hess should not be faulted, of course, for sticking to his subject. Nevertheless, his book raises questions legislative scholars ought to pursue to help sort out the competing explanations. Hess wisely left it for others to explore these issues. It is enough for one study to open the discussion. I hope we can look forward to seeing it continued elsewhere.

MICHAEL J. MALBIN

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The Supreme Court and the Decline of Constitutional Aspiration. By Gary J. Jacobsohn (Totowa, NJ: Rowman & Littlefield, 1986. ix, 182p. \$29.50).

This important and sober book assesses the theoretical distance that separates contemporary constitutional jurisprudence from that of the founding period. As indicated by the book's title, Jacobsohn maintains that the distance is great and the direction has been one of decline. The theme is developed through an analysis of the thought of five contemporary (or near contemporary) commentators, contrasted with that of the founding fathers. Though the five scholars selected for analysis—Pound, Dworkin, Berger, Grey, and Ely—might be thought so different as to make it perverse to insist upon their similarity, Jacobsohn does convincingly show that what is most significant and troubling in the thought of each is his rejection of the natural-rights philosophy that informed the founding generation.

Building upon a thesis of his earlier work, *Pragmatism, Statesmanship, and the Supreme Court* (Ithaca: Cornell University Press, 1977), Jacobsohn finds the critical turning point in the history of constitutional analysis to derive from "paradigmatic changes in scientific understanding" (p. 13). The shift from the fixity of Newtonian principles to the fluidity of Dewey's pragmatism brought a shift in jurisprudence from the securing of natural rights to the satisfaction of social interests, a shift so fundamental that Roscoe Pound's sociological jurisprudence declares "natural rights of man deduced from a social contract [to be] an alien conception in our law" (p. 18). Although Ronald Dworkin's jurisprudence expresses itself in opposition to Pound's sociological jurisprudence, Jacobsohn finds a more significant dimension uniting them. For while Dworkin famously purports to take rights seriously, he is shown in fact to build not from nature but, as with Pound, from "the dictates of conventional political morality" (p. 50). Paradoxically, this notion of rights built upon contemporary convention lies distant from the majoritarian political processes; these Dworkin portrays as a mere summation of utilitarian interests, typically "homogeneous and hostile" to individual rights. Jacobsohn skillfully contrasts this simpleminded notion of U.S. politics with Madison's brilliant, complex understand-

ing of an extended commercial republic, where the form of government itself and the political process it typically generates constitute a sturdy bill of rights—though not of the rights, such as free choice in sexual relations, that Dworkin would have us take seriously.

The historical works of Raoul Berger provide a powerful refutation of the claims of extratextualists like Dworkin, but according to Jacobsohn, Berger's claim that the framers were positivists is belied by their repeated statements that the Constitution was illuminated by reference to the natural rights it was intended to embody. The inability of Berger and others to understand the character of the framers' understanding of judicial authority is itself symptomatic of the distance that we have traveled since the founding. When Hamilton and Marshall spoke of judicial authority as characterized by its absence of will, they did not mean to rule out reference to natural rights, for these were a matter not of will but of discovery.

Conversely, while the historical works of Thomas C. Grey effectively refute the Berger positivist interpretation of the framers, they are equally misguided, for it was not a vague, unwritten "higher law" to which the early jurists appealed but the tradition of natural rights embodied in the text. The framer's Constitution, whose aspirations Jacobsohn finds best expressed by Lincoln, thus shows a middle course between unbounded notions of extra-textual approaches and dogmatic positivism; and it is a course superior in intellectual merit and moral authority to John Hart Ely's historically and philosophically ill-informed theory of "participation-oriented, representation-reinforcing" judicial review.

Jacobsohn's style is clear, often elegant, and the argument of the book as a whole is well crafted; he succeeds in showing the decline of constitutional aspiration. Still one cannot help being disappointed that Jacobsohn's own aspirations were not higher. While he convincingly portrays the theory of the framers as superior to those of contemporary commentators, Jacobsohn does not take the additional step of showing that the framer's theory is the Constitution's. It is noteworthy that the framer's theory is hard to place in the Constitution by resort to simple notions of contract, for as Jacobsohn acknowledges, the framers sought to embody in the terms of the

Constitution aspirations that the electorate and ratifiers did not fully share (p. 53); in short the framers' theory diverges from the adopters' understanding. In places, Jacobsohn even hints of a certain historical inevitability to the decline of constitutional aspiration (p. 2), and thus of an impossibility of effecting the framers' vision. He does suggest that the terms of the Constitution still can and should be illuminated by such Lockean treatises as *An Essay Concerning Human Understanding* and *The Second Treatise of Government*. But the wildly divergent conclusions of two recent works relying on Locke for their constitutional exposition—Rogers M. Smith's masterful *Liberalism and American Constitutional Law* (Cambridge: Harvard University Press, 1985) and Richard A. Epstein's provocative *Takings* (Cambridge: Harvard University Press, 1985)—raise doubts that such reliance can restore the coherence of the framers' vision. Finally one must at least question the nobility of the aspiration that Jacobsohn speaks of, for the Newtonian universe in which it finds its place is a cosmos of motion, but not of purpose; it is devoid of human telos. One can still speak, I think, of the mobility of the constitution, but then one has to hold that the act of constituting the U.S. polity is larger than the Newtonian and Lockean universe in which Jacobsohn locates it.

STANLEY BRUBAKER

Colgate University

From Nation to States: The Small Cities Community Development Block Grant Program. Edited by Edward T. Jennings, Dale Krane, Alex N. Pattakos, and B. J. Reed (Albany: State University of New York Press, 1986. viii, 258p. \$44.50, cloth; \$14.95, paper).

The Community Development Block Grant (CDBG) program was created in 1974 from an amalgamation of seven categorical grant programs administered by the Department of Housing and Urban Development (HUD), including urban renewal and Model Cities. Approximately 80% of the program's funds were allocated on an entitlement basis to cities of over fifty thousand population and to counties with populations of two hundred thousand

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or more. The bulk of the remaining funds were distributed on a competitive basis to smaller jurisdictions through the Small Cities CDBG program. As part of the Reagan administration's "New Federalism" design for devolving responsibility to the states, in 1981 the president proposed and Congress authorized the states to assume responsibility for the Small Cities program. By 1983, 46 states had done so.

Guided only by broad national principles that projects should be developed to eliminate slums and blight or to benefit low- and moderate-income persons, prior to 1981 CDBG was used to support such activities as the rehabilitation of housing for low- and moderate-income households and to undertake public-works projects that were supposed to improve communities. Toward the end of the Carter administration, projects to promote economic development were added to the list of preferred national activities.

When the states assumed responsibility for designing the mechanisms through which awards to local applicants would be made and then for supervising local program implementation, it was problematic how much the national framework for project design and program administration would alter. The present volume is essentially a monitoring report reviewing the experiences of seven states (Maine, Wisconsin, Mississippi, Missouri, Nebraska, Texas, and California) in that regard. The nine contributors to the study have obviously worked from a common outline, since the case studies and the three general chapters (two introductory ones treating the history of the program and the process of transferring program responsibility and a final chapter assessing the experience) read much more as a collaborative enterprise than one might have expected.

On the whole, contributors appear satisfied with the results of the transfer. While states selected somewhat different procedures for mobilizing local program participation and varied in the emphases they placed on different activities—putting less emphasis on housing-related activities (save for California) than HUD and more on public works and economic development—they evolved program approaches that appeared to be as professionally run as HUD's.

In addition to funding more communities

than HUD (with consequently smaller grants), the states funded smaller communities than HUD had. Contrary to the fears of some opponents of devolution, the authors find that local winners under state programs had slightly larger poverty populations and slightly more substandard housing than HUD grantees had. Nonetheless, the level of "distress" of a local jurisdiction did not mean that as large a share of resources necessarily went into projects that benefited low- and moderate-income households. The information provided by Jennings and his associates on this issue remains somewhat ambiguous, but a shift from housing activity (where direct benefits could be more easily demonstrated) into public works and economic development (where the benefits might at best be indirect) appeared to result in some real decrease in the benefits provided to lower-income groups. At the same time, this shift was already evident at the end of the Carter administration and was consistent with the Reagan administration's preferences in the matter so that such changes in project activities might well have occurred even if responsibility had remained at HUD.

Structured in the tradition of earlier monitoring research on the CDBG program by Richard Nathan, Paul Dommel, and their associates, the Jennings volume leaves unattended questions that might have been raised in a fuller-scale evaluation of the Small Cities program. There is no information provided, for example, about what it is that local recipients of CDBG did with the funds they received. Essentially, the authors work only within project categories contained in the applications submitted by local officials. Focused at the state level as this volume is, it does not look at important questions of program impact, including the capacity of localities to deliver on the different types of projects they undertook. Nor do the contributors go beyond these data to indicate whether any gap existed between the claims made by localities about prospective project benefits and benefits actually delivered. Finally, this volume shares with others of its kind a reluctance to undertake an evaluative critique of the program itself. While it may be of some comfort to know that the states can do as responsible a job of delivering resources to localities as the federal government can, the report leaves unexamined the question whether projects car-

ried out under the Small Cities program truly serve some national interest or whether that interest might be better served through alternate program vehicles or, perhaps, need not be served at all by the expenditure of scarce federal resources.

DONALD B. ROSENTHAL

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From Access to Power—Black Politics in Boston. Edited by James Jennings and Mel King (Cambridge, MA: Schenkman Books, 1986. vii, 196p. \$18.95, cloth; \$11.25, paper).

This collection of essays on Boston politics examines the racial dimensions of black participation from an historical and a contemporary perspective. Twentieth-century politics is analyzed by considering the ways in which the often overlooked variables of race and class have affected black participation. Using Mel King's recent unsuccessful campaigns for mayor as a framework, the authors examine the level of black participation, the need for coalition building, and the paucity of benefits received by blacks when Kevin White was mayor.

The book adds to the literature on urban governance because it addresses the fact that blacks have always played a role in Boston politics, albeit not a dominant one. The historical treatment covers the stages of black political development that culminated in the institution-building stage in the 1970s. According to Jennings and Mel King, blacks have now developed a sense of community, enabling them to mobilize around the issues of progressive politics—jobs, housing, and education.

Political development did not result in incorporation into the political process. Although blacks were mobilized, they still had to overcome class divisiveness, the fear of reprisals from Mayor Kevin White, and the longstanding habit of not fully supporting black community leaders. Although Boston's black community has not been politically passive, it has not received the scholarly attention given to other larger black communities. Yet the dynamics of political changes among Boston blacks deserve scrutiny as an understudied case of northern racism. The black

struggle for access and incorporation has taken place in a political system where the reputation for liberalism has historically concealed the institutional racism. The authors should be applauded for their effort to analyze this complex situation. For example, Jennings and King seek to probe why King did not become mayor by looking at the problems of black mobilization. They go beyond a superficial view of Kevin White's machine-style politics to examine the tactics he used to control and suppress black participation to ensure his four electoral victories.

This collection of essays succumbs to the usual problem of an unevenness of quality in a collected work. Jennings provides the more insightful contributions, but he fails to connect King's stages of development to the impact that the variables race and class have on black politics. The work might have presented a broader perspective on Boston politics by giving more attention to the numerous attempts to elect blacks to the school committee and the city council, all of which were key efforts in the drive to realize a measure of black power in Boston. This work deserves consideration because it seeks to go beyond the facade of Boston's historical image to analyze the nature of the social and political fabric that shapes the black effort to move from mere access to incorporation in Boston's political structure.

TONI-MICHELLE C. TRAVIS

George Mason University

Controversies in Environmental Policy.

Edited by Sheldon Kamieniecki, Robert O'Brien, and Michael Clarke (Albany: State University of New York Press, 1986. vi, 322p. \$39.50, cloth; \$12.95, paper).

One has high expectations for a volume that promises coverage of major controversies in contemporary environmental policy. Since the late 1970s and especially during the Reagan administration, federal environmental policy has been subjected to widespread criticism for a variety of alleged failings, which has prompted some reappraisal of both policy goals and means. Increasingly, scholars have turned their attention to such issues as the use of benefit-cost analysis and risk assessment in regulation, the virtues of market-based approaches as an

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alternative to regulation, the impact of environmental policy on energy production, and the conflicts between environmental protection and economic development, among many others. Although the essays in this book do not provide exhaustive coverage of these controversies, they do address a fair number of them, and they may well encourage others to engage in more extended and systematic research to provide more definitive answers. As is often the case with an edited collection, some of the essays are especially provocative or insightful, and these merit careful attention.

The editors have brought together 11 essays (by 16 authors in all) and have grouped them into four major sections accompanied by a long and perceptive introduction by Dean Mann. Mann reviews and assesses the way in which the characteristics of the U.S. polity have shaped the formation of our "jerry-built structure" of environmental policy. Rejecting the more apocalyptic vision of some (e.g., William Ophuls), he finds that the U.S. political system has responded rather well to the challenge of environmental damage and resource scarcity. Most of the contributors share his confidence in present institutional and political arrangements, although a strong dissent is lodged by devotees of the "new resource economics."

In the first section, which focuses largely on the privatization issue and public lands, the argument in two of the essays (by John Baden & Dean Lueck and William Dennis & Randy Simmons) is fairly predictable: government regulation has failed and market regulation is the answer. Resources would be utilized more efficiently, they say, under the incentives of private management. Regrettably, both essays are marred by a proclivity toward unsupportable generalizations on regulatory policy and "government failure." In contrast, Howard McCurdy's essay is more persuasive in arguing that neither privatization nor the New Federalism's decentralization of program responsibility provides a sufficient response to the complexity of environmental problems and the need for intergovernmental cooperation. Michael Sheehan's essay completes the section by offering a well-deserved criticism of the inadequacies of economic approaches like benefit-cost analysis, using the case of hazardous-waste control.

The second section (policy-evaluation stra-

tegies) focuses chiefly on the Environmental Protection Agency (EPA). Steven Cohen finds the EPA to be a qualified success, particularly given a continuous record of underfunding, and Alfred Marcus credits the agency with more success in conflict resolution than in achieving environmental-quality goals. Both find that during Anne Burford's reign as administrator the agency suffered serious losses in funding, personnel, morale, and effectiveness. A brief yet helpful critique of benefit-cost analysis by Joseph Biniek is also included here, with special attention given to problems of benefit estimation and dealing with uncertainty.

A short third section returns to the theme of market incentives versus government regulation. In two quite different essays, Thomas Ingersoll and Bradley Brockbank—and Benjamin Walter and Malcolm Getz—critically examine the proposition that environmental quality can best be protected by increased reliance on the marketplace. Both suggest innovative ways to improve upon present regulatory approaches with careful and selective use of market mechanisms and thus reject more ideological views of the market's potential.

Essays in the last section focus on politics rather than current policy approaches and alternatives. Mark Kann offers a critique of environmental policy from an elite-theory perspective; current policy, he says, is far too responsive to corporate ideology, and he favors decentralization and increased "environmental democracy" as a solution. Although unpersuasive, his essay challenges the mainstream views of most contributors. Finally, Henry Kenski and Helen Ingram offer a useful assessment of the Reagan administration's effort to alter the direction of environmental policy in the early 1980s, concluding that the political marketplace (the continued popularity of environmental programs and strong congressional opposition to Reagan's proposals) constrained what might have been a far more damaging assault. Their faith in the political market parallels Mann's and stands in contrast to Kann's perspective.

This volume's potential as a textbook is somewhat limited by the narrow range of controversies included, the organization of the topics, and the lack of a common analytical framework or set of themes. These problems

are difficult to avoid in any edited collection. In this case, Mann's introductory chapter and the editors' comments (both in their section introductions and in their brief concluding chapter) partially compensate for the inevitable disparity of approaches and perspectives. The datedness of some of the material is a further problem; although published in early 1986, the essays seem to have been written in mid-1983, prior to some significant changes in the Reagan administration's environmental policy. All of these weaknesses are unfortunate because the book is designed more as a textbook than as a contribution to scholarship. Despite these limitations, however, environmental-policy scholars may well find selected essays useful both for the classroom and for their own enlightenment. A subject index and a name index will assist readers in locating coverage of specific topics and individuals.

MICHAEL E. KRAFT

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Institutional Disability: The Sage of Transportation Policy for the Disabled. By Robert A. Katzmann (Washington: Brookings Institution, 1986. ix, 211p. \$28.95, cloth; \$10.95, paper).

Robert Katzmann traces federal policy on public transportation for disabled persons from its emergence as a public issue in the early seventies as an example of continuous vacillation in public policy. He attributes the lack of clear policy direction to a combination of a failure to define the problem precisely, inadequate mutual understanding and cooperation among the three branches of the federal government, and weaknesses in the internal operation of each.

Katzmann argues that a principal reason for the unsteady course of federal policy in the area for over 10 years was the conflict between the values of efficiency and equity latent in the issue, which caused fluctuations in the definition of the problem. On the one hand, some political actors defined the problem as one of "effective mobility"—getting disabled persons where they want to go through the most cost-effective means. On the other hand, others defined the problem as one of the right to

"equal access"—making regular public-transportation systems accessible to disabled persons and thus avoiding the perceived stigma of separate service. The book is organized into chapters describing the activities in the legislative, administrative, and judicial processes.

Like many before him, Katzmann notes the fragmentation of congressional policymaking. The first statutory recognition of the transportation problems of disabled persons was, Katzmann notes, initiated not by pressure from the prospective beneficiaries but by an individual member of Congress acting as a policy entrepreneur and adding rights-oriented language during the floor debate on a general mass-transit authorization bill. After the issue had become a highly visible struggle between transit-industry forces and disability-rights activists in the late seventies, the problem arose of different committees taking conflicting approaches to the same issue.

An even more striking example of congressional action without deliberation is Katzmann's finding that staff members retroactively created legislative history without the knowledge or approval of legislators. The committee report stating that regulations were to be written to implement the provision that came to be the linchpin of the rights-based claims to accessible transportation was drafted to accompany later amendments to the act, even though the original language made no reference to regulations and the amendments did not alter that language.

The administrative process was marked by competition between bureaucrats favoring the rights-based equal-access approach and those favoring the effective-mobility approach, whose relative power in policymaking was affected by shifting external pressure from Congress, the courts, and top administrative appointees. Both direct regulation and technology forcing were tried as means to increase accessible transportation. Katzmann finds that the rule-making process was hampered by bureaucrats' inability to trust the reliability of information presented by either side and their inability to conduct informal negotiations that might have been able to break through both sides' hardened public positions to find common ground. The technology-forcing strategy—inducing the industry to develop a new bus design that would be more accessible—failed

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because the commitment to it was not stable enough for long enough to convince the industry to make the significant investments required.

Katzmann's portrayal of the judicial process belies the image of activist federal courts reaching out to impose a policy agenda of their own. Some lower-court judges were, in Katzmann's eyes, "fooled" by the dubious legislative history into ordering the promulgation of regulations. Nevertheless, it was ultimately the courts that pulled the rug out from under the rights-based approach to accessible transit when the DC Court of Appeals struck down the more demanding rights-based regulations following a Supreme Court ruling that the law did not require affirmative action efforts. Ironically, the courts were less inclined toward a rights-based approach to the problem than were many legislators and bureaucrats.

Katzmann's most provocative conclusion is that accessible-transportation policy represents the end of rights-based interest-group politics and its replacement by politics dominated by cost and efficiency arguments. President Reagan ended the wavering between the equal-access and effective-mobility approaches in favor of the latter and imposed that view through tighter control of the regulatory process. The decline of rights-based appeals also reduced political unity within the disabled community by making it necessary for people whose disabilities required different types of accommodations to compete against each other for scarce resources rather than being able to support each others' "right" to have all their needs met.

While Katzmann's fine book is more about the policy process as a whole than about interest-group litigation, his analysis does not bode well for those who continue to view the courts as a vehicle for social change. On the other hand, there is little support in the book for those who argue that the other branches of government have greater institutional capacity for policymaking than courts. Katzmann concludes with several recommendations for procedural changes in the legislative, administrative, and judicial processes that might help avoid such a policy morass in the future.

SUSAN M. OLSON

University of Utah

Voting Behavior. Research in Micropolitics, vol. 1. Edited by Samuel Long (Greenwich, CT: JAI, 1986. x, 301p. \$26.25).

This volume inaugurates the Research in Micropolitics series. Its nine chapters review research on voting and elections, primarily research conducted within the quantitative history and social-psychological paradigms.

The first two chapters were written, entirely or in part, by quantitative historians. Peter H. Argersinger and John W. Jeffries begin the volume by taking the long view and summarizing historical research on U.S. elections, especially investigations of "critical elections" that are argued to have established new party systems and realignments of popular partisan attachments. The authors point out that however useful studies of realignments/party systems may be in some respects, such work tends to be essentially "taxonomic and descriptive" and often devotes insufficient attention to devising and evaluating generalizable theories of the causes or consequences of political system change. The second chapter, by Jerome M. Clubb, William H. Flanigan, and Nancy H. Zingale, reviews how some scholars (including the chapter's authors) have sought to mitigate this deficiency by linking partisan realignments to the concept of "political generations." Given the volume's emphasis on empirical research, the review of the political-generations literature focuses almost exclusively on investigations of the New Deal realignment, the one for which the most data are available (except, perhaps, for the post-Vietnam realignment/dealignment).

The remaining seven contributions deal primarily with research on contemporary U.S. national elections and voting, with such titles as "Primaries and Nominations" (Thomas R. Marshall), "Electoral Campaigns" (Paul A. Smith), "Money and Elections" (Anne H. Bedlington and Lynda W. Powell), "Mass Media and Political Images in Elections" (Doris A. Graber), "Partisanship and Voting" (James E. Campbell, Mary Munroe, John R. Alford, and Bruce A. Campbell), "Economic Determinants of Voting" (M. Stephen Weatherford), and "Turnout in American National Elections" (John H. Aldrich and Dennis M. Simon).

The order in which I have listed the authors' contributions is one that seemed to me to be a natural one—proceeding chronologically from

primaries through the campaign and into elections and their determinants. That progression, however, is virtually uncorrelated with the chapters' actual order of appearance in the book. And because the volume contains neither introductory nor concluding commentary by the editor, the reader is left to wonder what editorial logic governed its organization. While on this subject, the reader may also wonder why these particular nine themes—important though they are—were selected for inclusion while others that would seem essential to a debut volume on voting (e.g., ideology and mass belief systems or issue voting) were not.

What about the quality of the brief review chapters themselves—by what criteria should they be judged? In my opinion, each chapter should (1) demarcate the main themes around which research in a subfield may be organized—for example, substantive, methodological and paradigmatic themes; (2) identify and describe concisely the conclusions of the most significant pieces of research in the subfield; (3) provide a reasonably comprehensive bibliographic reference to other relevant work; (4) indicate major questions or controversies most in need of further investigation; and (5) suggest why research in the subfield being reviewed is worthwhile (or not).

By these standards, the chapters in *Voting Behavior* are of high quality, and a few are exceptionally good. As examples of the latter, the chapter by Weatherford does an admirable job of synthesizing the notoriously confusing and often seemingly contradictory literature on the connections between economics and voting, and the contribution by Campbell and company provides as complete a summary of the conceptual, methodological, and empirical issues surrounding the study of political partisanship as I have seen anywhere. On the other hand, the otherwise commendable chapter by Aldrich and Simon fails to consider essential studies of U.S. election turnout conducted from a comparative perspective. And the Marshall piece on primaries and nominations risks becoming instantly obsolete as a result of the latest research stemming from the year-long 1984 National Election Studies materials (by, among others, Larry Bartels and Henry Brady).

Taking a step back, I have one reservation about the proposed Research in Micropolitics series, and that is a nagging sense that we may

be nearing a point where we spend too much time "reviewing" our own work and not enough time actually doing it. Consider that the APSA has already established an annual series of papers on the "state of the discipline" that covers many of the themes in the proposed series edited by Long, as does the monograph-length chapter by Donald R. Kinder and David O. Sears in the newest edition of the *Handbook of Social Psychology*. And a brochure accompanying my copy of *Voting Behavior* announces the publication of a new *Annual Review in Political Science* to be edited by (who else?) Samuel Long and to contain in volumes 1 and 2 chapters on (surprise!) political participation, generational politics, and electoral behavior. Should someone consider editing the *Deja Vu Review*?

GREGORY R. MARKUS

University of Michigan

Homeward Bound: Explaining Changes in Congressional Behavior. By Glenn N. Parker (Pittsburgh: University of Pittsburgh Press, 1986. xxi, 206p. \$22.95).

Homeward Bound represents the culmination of Glenn Parker's diligent research on the causes and consequences of congressmen's travel to their home states and districts. Specifically, he (1) documents the mid sixties' increase in the amount of *time* senators and representatives spent in their constituencies; (2) shows that these changes can be attributed to period and, in the House, cohort (generational-replacement) effects; (3) claims that members boosted their travel in order to solidify their electoral margins and thus gain leeway from their constituents; (4) demonstrates that the change in *home style* was facilitated by increases in travel allowances and accommodative changes in legislative scheduling; (5) infers that the availability of other "perks" led to the rise of other constituency-service activities; (6) empirically tests the hypothesis that increased *attentiveness* to constituents generated greater electoral margins for a narrowly defined group of incumbents; and (7) concludes with a discussion of other consequences of the change in home styles: greater individualism, a swing of power within the parties, increased freedom from party

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leadership, increased policy activity, more multiple-committee assignments, and a decline in policy representation.

Parker is more convincing when documenting the rise in travel home and analyzing its proximate causes than when dealing with his other arguments. Can we be confident that *time in district* is a good surrogate for *home style* or even *attentiveness*? Does the demonstration that increases in travel allowance preceded the growth in travel time prove incumbents' intentions? Does strenuous use of the perquisites of office actually increase incumbents' freedom? By how much? Not only is there a raging dispute over the electoral effects of constituency service, but much recent literature on policy voting by constituents suggests that voters seem to cue on the aggregate policy decisions of their senators and representatives. From which constituents do members seek freedom?

Some of the key theses of the book are not fully explored. While his data clearly show no impact of redistricting on travel, might they yield different results if separated into favorable and unfavorable redistricting? The demonstration in chapter 2 that previous electoral margin does not affect travel raises the question: would *changes* in recent election results matter?

In chapter 5, Parker reopens the "Case of the Vanishing Marginals," arguing that greater constituency attentiveness, as measured by time spent back home, leads to popularity, which in turn builds increased vote margins. By framing his hypothesis narrowly (focusing only on representatives who, entering Congress in the 1960s, won elections by narrow margins and were atypical of their districts and on northern Democratic senators in certain time periods), Parker is able to show that changes in travel had an electoral consequence. But surely the important question is whether attentiveness (home style, trips to the constituency, casework, use of office "perks," etc.) pays off in general and over time. (To his credit, Parker acknowledges in a footnote that nothing more general could be found.) Moreover, might there be other explanations for his limited finding? For example, might this small group of congressmen have changed their voting behavior, or might they represent states in which the underlying political climate was changing? Are there other activities, highly

correlated with travel time, that might be at work? Why should *changes* in—rather than the absolute level of—travel be so important? Finally, why would increased attentiveness improve the images of all congressmen but produce votes only for marginal atypical members?

Given its bearing on important questions and its use of important data, the book is certainly worth reading. Its drawback is not necessarily that Parker is wrong; rather, he fails to explore alternative explanations for many of his findings and to provide data for and analyses of his speculations.

JOHN R. JOHANNES

Marquette University

White House Operations: The Johnson Presidency. By Emmette S. Redford and Richard T. McCulley (Austin: University of Texas Press, 1986. xii, 274p. \$30.00).

This is the fifth book in a series on the Administrative History of the Johnson Presidency in which there will eventually be 10 to 12 similar special studies and an overall volume. The series is directed by Emmette S. Redford and James E. Anderson. The books in the series are based primarily on documentary materials (files and oral histories) in the LBJ library and on interviews with participants in the Johnson presidency. The authors of this study state that their objective is to produce "an authentic and adequate historical record . . . presented from a social science perspective [that] will amplify knowledge of administrative processes and of the tasks and problems of the presidency" (p. ix). They succeed admirably in the accomplishment of their goal.

Redford and McCulley provide a comprehensive view of White House operations in the Johnson presidency. Their concept of administration encompasses organizational structuring, staffing, budgeting, policy development and implementation, and management. They examine in detail the abrupt transition from the Kennedy to the Johnson administration, staffing and structuring the presidency, Johnson's management style, the development of the legislative program and executive policy, direction of the executive branch, and repre-

sentation of the president to other elements of the political system.

This is, however, far more than a narrow, highly idiosyncratic case study focusing on one aspect of a presidential administration. Redford and McCulley link the Johnson material to the large body of general presidential scholarship and to work that focuses on organizing and managing the presidency. Their study is theoretically relevant. They employ as an analytical framework two alternative models for studying the organizational presidency, institutionalization and personalization. Institutionalization focuses on structure and process and regards them as the product of internal and external influences, of which the president's management style and personality are but two. Personalization regards White House organization as primarily a reflection of presidential personality and style and interpersonal relations within the White House. Closely associated with the institutionalization and personalization models respectively, are distinctions between centralized and decentralized organizational structures and formal and informal processes. Redford and McCulley argue, correctly I believe, that both institutional and personal factors impose constraints and create possibilities in organizing White House operations. Their analysis supports this position.

Redford and McCulley also make extensive use of the concept of the *subpresidency*, which they define as "all those who serve the president, on a continuing or an ad hoc basis, in an institutional capacity or otherwise in the exercise of his responsibilities" (p. 1). They further refine the concept by identifying a "triadic" subpresidency consisting of White House assistants, executive-office units, and departments and agencies. Members of the president's inner circle hold positions in the executive branch. Another book in this series, *Managing Macroeconomic Policy: The Johnson Presidency*, by James E. Anderson and Jared E. Hazleton, makes extensive use of the concept of an economic subpresidency.) The subpresidency concept should prove useful to students of presidency as there has been no term that quite as accurately characterizes the individuals who serve and influence the president.

The portrait of White House operations that emerges from the book is a highly personalized one. Johnson's drive for accomplishment, his

involvement in virtually all policy areas, his insatiable thirst for information, and his enormous capacity for work "produced a highly individualized staff operation" (p. 184). The staff was "lean in size and heavily worked" (p. 182) and the "distinction between personal and institutional staff aid," which recent analysts of White House organization have stressed, all but disappeared. That distinction, Redford and McCulley maintain, is far from absolute.

Presidential assistants who were heavily involved in core policy-related functions also performed representational tasks. The areas of activity that were most effectively performed were legislative program development and legislative liaison. This is hardly surprising given Johnson's ambitious domestic-policy goals. Much less effective was the coordination of executive-branch activities and press relations. Johnson lacked an explicit strategy for managing White House functions. Relations between the president and White House aides, executive-office units, and agencies were "responsive to the circumstances peculiar to [each major policy area]" (p. 185). The steady deterioration of relations with the media resulted from rising public disenchantment with Johnson's Vietnam policy and his overinvolvement in the details of press relationships. Redford and McCulley also maintain that Johnson's ability to demand and receive total loyalty to himself and his policies saved him from the problems of public dissension within his staff and aggrandizement of staff that has plagued later administrations.

With respect to structure, Redford and McCulley found that it evolved in response to the performance of three primary White House functions—management and legal services, the core activities of policy development and implementation, and representation of the president to external constituencies—and to the personal capabilities of staff aides. The structure was flat, there was no consistent pattern of decision making, and many advisers had access to the president. Redford and McCulley summarize Johnson's pattern of operations as "one of minimum hierarchy and loose integration that distributed counsel for the president through a triadic subpresidency composed of White House, executive offices, and departments and agencies. It exhibited definite features: multiple points of integration within the White House; fusion of personal

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and institutional aid in executive office directorships; habitual use of specialized, coordinative, and generalist service from the three divisions of the subpresidency; and special instances of policy decision and implementation. The constitutional and political responsibility heavily concentrated on him personally" (p. 200).

Although their analysis of the Johnson presidency provides more support for the personalization than the institutionalization models of presidential organization, Redford and McCulley make no claim for its primacy. They conclude that no future system for managing the White House will conform fully to either model nor will it be either fully centralized or decentralized. What their analysis strongly suggests is that personal factors—presidential goals, work habits, and decisional styles—and a triadic subpresidency that is constantly in flux will determine the pattern of White House operations in future administrations. This means that prescriptive studies of the management and organization of the presidency should be viewed with skepticism. What works for one president in circumstances peculiar to his presidency may not be appropriate for his successors.

To conclude, this is one of the most important studies of the Johnson presidency to date. However, its value extends beyond the immediate events, the personalities, and the organizational arrangements described and analyzed in it. It provides a useful synthesis and application of much recent thinking on the organization and management of the presidency and it reveals again how deeply personal is this most important political office in the democratic world. Finally, it is an exemplary demonstration of the use of presidential libraries as primary source materials for empirical research. The authors are to be commended for their efforts.

NORMAN C. THOMAS

University of Cincinnati

To Run a Constitution: The Legitimacy of the Administrative State. By John A. Rohr (Lawrence, KA: University Press of Kansas, 1986. xv, 272p. \$24.95, cloth; \$5.50, paper).

The power that administrative agencies exert in modern U.S. society has not always been regarded as being rightfully exercised. As John Rohr sees it, the bureaucracy's illegitimate status in the eyes of many citizens stems from the misapprehension that its characteristics and mode of operation do not conform to the expectations of the framers of the U.S. Constitution as to how the governmental system they designed should work.

The topic is certainly a timely one, in light of the fact that the Constitution's bicentennial is being celebrated this year along with the somewhat less visible event that the public-administration community is itself commemorating, the 100th anniversary of the publication of Woodrow Wilson's essay on the study of public administration in 1887. Rohr's choice of this topic reflects his belief that the widespread perception of bureaucracy as an illegitimate pretender to power has had an adverse effect upon its effectiveness today. He also raises the possibility that establishing the legitimacy of administration in the U.S. governmental scheme of things might deter misbehavior on the part of bureaucrats. A bureaucracy that can trace its ancestry to constitutional principles might be less inclined to abuse its power than one with no such legitimate parentage.

The book has an admirable clarity in its design. It should not be altogether surprising in view of Rohr's very evident affection for the Constitution that his analysis of the problem of bureaucratic legitimacy has a tripartite structure. Part 1 attempts to demonstrate the compatibility of a highly administered state with the intentions of the Constitution's framers. Part 2 examines the theoretical underpinnings of the administrative state as they emerged in the heyday of civil-service reform during the late nineteenth century through the work of such notables as Woodrow Wilson, Frank Goodnow, and Thomas M. Cooley.

According to Rohr, these late-nineteenth-century theorists were founding the administrative state in words. As compared to what it has since become, public administration at

that time had only a very small presence on the U.S. scene. In part 3 Rohr shifts his attention to the New Dealers in the 1930s and analyzes the views of those who founded the administrative state "in deed" rather than words.

After examining all these sources, Rohr concludes that the power and presence of bureaucracy within the structure of modern U.S. government is compatible with original constitutional principles. As might be expected, some aspects of his argument are more convincing than others. Especially good are chapters 4 and 6. Chapter 4 presents a strong case for the striking proposition that bureaucracy heals a "defect in the constitution" by making the national government more representative of the population it serves than it would otherwise be. Rohr here turns the tables on those who would challenge bureaucratic power as being antidemocratic in character. Chapter 6 first articulates a theme that is to recur later in the book—that the powers exercised by the president and other officials flow not from their election but from the authority vested in them by the Constitution. Consider, by way of contrast, the current tendency of presidents and their White House staff to see elections as providing an all-purpose mandate legitimizing even the illegal use of power.

Less compelling is Rohr's attempt in chapter 3 to justify the role of higher civil servants in national policymaking by contending that it is analogous to the part that the framers of the Constitution initially (though not ultimately) intended the Senate to play in executive deliberations. Much of the argument covered in chapters 8 and 9 on the New Deal seems less to legitimize bureaucracy than legitimize an expansion of presidential power. Indeed the Brownlow report discussed in chapter 9 spawned a White House establishment that has since taken over functions that bureaucrats might legitimately be expected to perform. Rohr might also have given more attention to the role that its expertise has played in legitimating the power of U.S. bureaucracy—especially in the eyes of the courts.

But in overall impact this book makes a very substantial contribution to our understanding of the uneasy relationship—at least in theory—between the Constitution and bureaucracy over the course of U.S. political development. It invites comparison with other distinguished books in this area: James O. Freedman's *Crisis*

and *Legitimacy* and Stephen Skowronek's *Building a New American State*. It is also a tribute to the work of Herbert Storing, whose student Rohr was and on whose insights he frequently draws in building his own analysis.

FRANCIS E. ROURKE

Johns Hopkins University

Urban Policy Problems: Federal Policy and Institutional Change. Edited by Mark S. Rosentraub (New York: Praeger, 1986. vi, 261p. \$36.95).

This volume includes an editor's introduction and 10 chapters that focus on several current urban-policy issues from various disciplinary perspectives. The editor's introduction reviews the changes in U.S. presidential domestic priorities, and then discusses the impact of the Reagan administration's policies and symbolic messages on various U.S. institutions.

The book is organized into three sections. In part 1, "Domestic Policy and Policy Images," the three articles rely on quantitative analyses to examine fiscal issues. Robert C. Rodgers and Jeffrey D. Straussman test the assertion that municipal employee unions have contributed to cities' fiscal stress, as well as testing whether fiscal stress may have moderated union activity. Using data from the mid to late 1970s, the authors find little support for the contention that unionism and fiscal stress are significantly related. David R. Morgan and Robert E. England also examine the causes of fiscal stress by developing a model that includes both long-term socioeconomic factors and short-term political factors in sequential order.

Taking a different approach to the fiscal-stress issue, James C. Musselwhite, Jr. and Lester M. Salamon argue that in recommending social-welfare cuts, conservatives fail to recognize the key role state, local, and particularly voluntary agencies have had in implementing federal programs. Due to funding reductions under Reagan's New Federalism, the nonprofit sector has been pressured to provide more services with fewer resources. Since these agencies have responded by developing user fees for services, a fundamental equity issue arises: will services for the poor and dis-

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advantaged be made available primarily to those who can pay for them?

Part 2, "Domestic Policy and Institutional Reactions," includes four areas that have been significantly affected by recent policy developments. Myrna R. Pickard summarizes the changes in health-care delivery as part of the effort to contain costs but fails adequately to address the book's themes. Patricia A. Huckle's chapter examines the institutionalization of equal-employment programs in Los Angeles, and suggests that the Reagan administration's efforts to weaken affirmative action are unlikely to be successful where local political pressure and bureaucratic support are strong. Lyke Thompson argues that planned economic-growth centers, primarily in the Sunbelt (e.g., Research Triangle Park, Dallas-Fort Worth Airport), concentrate resources in certain areas, but also drain resources from older areas. Mitchell L. Moss discusses the impact of the reorganization of the telecommunications industry on urban governments, particularly in economic development, municipal management, and land use.

Three views of how the academic disciplines should respond to changing urban policies and priorities are offered in part 3. In one of the most interesting pieces in this collection, Scott Cummings reviews how the fiscal crisis and the Reagan program have affected higher education by enhancing some disciplines at the expense of others. The author contends that social scientists will have to support the Reagan agenda if they wish to influence the policy process and survive in the university. In a related theme, Jeffrey I. Chapman, Gary J. Reid, and Donald R. Winkler focus on two areas where applied economists have made some intellectual strides in policy analysis (i.e., research on fiscal limitations and education) but have not always had much impact.

In the final chapter, Robert Warren offers some significant insights into the issues of efficiency and equity in urban-service delivery. He argues that when citizens' consumption and participation costs are considered as the total costs of delivering services, efficiency and equity can be understood as complementary values.

In general, this collection is a solid addition to the urban-policy literature. The authors attempt to assess the impact of policy changes, but not all of them focus on the effects of the

Reagan administration's policies specifically. The failure of some to develop the themes of the editor is a common weakness in volumes of this nature. However, part of this problem could have been avoided if the editor had been more general in his treatment of urban-policy changes.

RUTH HOOGLAND DEHOOG

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Political Crisis/Fiscal Crisis: The Collapse and Revival of New York City. By Martin Shefter (New York: Basic Books, 1985. 270p. \$21.95).

Fiscal crisis reemerged as a compelling issue in urban politics in the 1970s. For the first time since the Great Depression, major U.S. cities teetered on the brink of financial collapse. The most spectacular—and most extensively examined—flirtation with fiscal disaster came in New York City in 1975, where municipal default was avoided only by massive state and federal intervention.

Previous studies have focused largely on the immediate antecedents to New York's fiscal crisis. They have attempted to explain financial collapse primarily in terms of the response of particular political leaders to contemporary economic and social changes in the nation's largest city. Martin Shefter's richly detailed treatment is far broader in scope and much more satisfying for students of urban politics. His analysis reaches back into the nineteenth century and covers the decade following the 1975 crisis. Shefter frames the issue squarely in political terms, with fiscal crisis illuminating "the boundaries within which the game of urban politics is played and the imperatives confronting the players in that game" (p. xii).

For Shefter, recurring municipal fiscal crises are the result of political crises, which in turn arise from the periodic inability of urban political regimes to meet the imperatives of maintaining electoral support, sustaining the local economy, preserving the municipality's credit, and moderating conflict. Complicating these primary political tasks is the city's lack of control over many of the key parameters that shape its political economy. Cities depend on state and national governments for legal authority and fiscal resources. They rely on

private capital markets to borrow funds. And their prospects are critically affected by social and demographic changes, trends in the national and international economy, and developments in state and national politics.

Satisfying these imperatives within a changing set of external opportunities and constraints involve what Shefter terms a process of serial bargaining among key actors in the urban political system. Dominating this process until recently were machine politicians who bargained to adjust the demands of business interests, urban newcomers, and middle-class professionals. Breakdowns in this process led to political crisis and often fiscal crisis, resulting in the election of reform administrations. To regain power in the wake of crisis and reform, machine politicians made concessions that "altered both the balance of power among the leading political actors in the city and the structure of the political organizations and institutions through which political conflicts were conducted" (p. 29).

These concessions, in turn, strengthened other participants in city politics, leading to the development of a highly pluralistic system in the years after World War II. The inability of this regime to accommodate intensifying political demands and conflicts within the boundaries of the city's financial resources ultimately produced the fiscal crisis of 1975. Shefter details these developments most skillfully, covering an impressive range of topics but never losing sight of his central theoretical and analytical concerns. Especially effective are his treatments of the emergence of public employees as central political actors, the limited political influence of blacks and Hispanics despite their growing numbers, the role of professionals and experts in city politics, and the new political equilibrium achieved under the leadership of Mayor Koch in the decade following the 1975 crisis.

Throughout, Shefter concentrates heavily on political factors, illuminating brilliantly the importance of what goes on inside the "black box" of politics. In the process, however, developments such as suburbanization and demographic change within the city are given less detailed attention than they deserve, particularly in Shefter's assessment of future political developments in New York.

Although mindful of the limitations of a study of the nation's most complex urban pol-

ity, Shefter makes a persuasive case for his general conclusion that fiscal crisis is an integral aspect of the cyclical process of bargaining and adjustment and that "the way these crises are resolved establishes the boundaries and practices of political contestation in American cities in more normal times" (p. 225). The importance of these fiscal boundaries and practices for democratic politics are underscored by New York's experience. When solvency conflicted with democracy, local self-government was temporarily sacrificed.

Shefter has produced an outstanding contribution to the literature of urban politics that deserves wide attention. Few case studies, indeed, match Shefter's accomplishment in significantly enlarging our understanding of central issues in the governance of U.S. cities.

MICHAEL N. DANIELSON

Princeton University

Enforcement or Negotiation: Constructing a Regulatory Bureaucracy. By Neal Shover, Donald A. Clelland, and John Lynxwiler (Albany: State University of New York Press, 1986. xi, 193p. \$39.50, cloth; \$12.95, paper).

This book examines the "early development and implementation of the federal program for regulation of surface mining (1977-1981)" in the United States. More broadly speaking, the book is concerned with regulation as a process and, relatedly, how a regulatory bureaucracy (the Office of Surface Mining in particular) is constructed. Although the subject matter deals with an important environmental issue, the theme of the book is not environmentalism. Environmentalists, however, would be well served by adding this volume to their collection, because it documents in considerable detail the issues, actions, and thinking of federal regulators in the Carter administration who were responsible for this controversial program.

Students of regulation are likely to find the book less informative and suggestive, especially from a theoretical perspective. The authors, who are sociologists, tell the reader that they rely on an array of state theories, "particularly

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those revolving around theses of the relative autonomy of the state and the role of the state managers" (p. x), to aid in understanding regulatory bureaucracy. Chapter 1 discusses various neo-Marxist theories (e.g., state-manager theory) and non-Marxist theories (e.g., special interest-pluralism theory) that the authors believe have relevance to understanding regulation in general and, when refashioned into a typology of regulatory style, to understanding the surface-mining regulatory experience in particular. The refashioning, however, requires employing concepts drawn from the policy-implementation literature (i.e., *implementation stages*) and the organizational literature (i.e., *loosely coupled*). The result is a typological scheme that permits an analysis both of the regulatory process and (in the case of coal surface mining) of why this adversarial and conflict ridden regulatory experience unfolded as it did.

Their typological scheme has two principal dimensions: (1) stages of the regulatory process defined in terms of statute formation, the bureaucratic process, rule making, regulations, and rule application, and (2) regulatory styles defined dichotomously as enforced compliance and negotiated compliance. This framework begets a set of characteristics associated with one style or the other (e.g., the regulatory process is rigid, precise, formal, and coercive with an enforced-compliance style, whereas it is flexible, vague, informal, and educational with a negotiated-compliance style).

This framework is rather simple and, unfortunately, adds little to our knowledge of regulation. Indeed, the authors' discussion of state theories in conjunction with their eclectic use of concepts drawn from the implementation and organizational literature leaves the reader with the feeling that there is really very little theory guiding their study. The authors should have been more candid in telling the reader that this book chronicles an interesting story and if read carefully, may provide useful insight into regulatory processes and experiences.

Students of public policy may find the book equally wanting. The authors cannot seem to decide whether or not the surface-mining regulatory experience, which they argue was enforced compliance in the Carter years, worked well or poorly. They draw on a wealth of original data (interviews, surveys of inspec-

tors, inspection/enforcement statistics, and documents) to argue that enforced compliance did not work very well. Indeed, this style had a boomerang effect as movement toward a negotiated style had begun before the Carter administration was displaced by the Reagan administration. Surprisingly, then, the authors, following a critique of the surface-mining program in which a number of deficiencies are highlighted, conclude that enforced compliance was effective (p. 146). The authors cannot have it both ways, unless what the reader is seeing here are the ideological biases of the investigators.

The authors, who are citizens of a state (Tennessee) that has turned over its regulatory program to the federal government, seem to prefer the more direct, federal-centered approach in contrast to the more accommodative, state-centered approach. But the period they studied was not kind to the approach they prefer. The Office of Surface Mining, as is amply documented in their book, became just about everyone's favorite example of how things can go wrong when the federal government is too heavy-handed in state affairs.

Political scientists, particularly those who have an interest in intergovernmental relations and/or policy implementation, will have mixed feelings about this book. The interview-based material richly portrays the anxieties and fears that federal and state regulators often have in carrying out a complex intergovernmental program. A serious-minded intergovernmental student will be disappointed, however, with the generally scanty treatment of state-by-state behaviors. Rather, the authors place a much greater emphasis on East-West regional differences which, in this reviewer's judgement, is made far more important than it deserves to be. The policy implications the authors draw are also unlikely to spark much interest. Among the key implications, the authors claim that "flexible regulations are not necessarily bad regulations" (p. 145); that "discretion is a necessary and unavoidable component of an effective implementation program" (p. 146); and that "an effective regulatory program must be based on a recognition of political forces" (p. 143).

Two final points deserve comment. First, the book is somewhat uneven and disjointed in style, no doubt the result of multiple authorship and weak editing. Second, the print is

unusually small thereby making the book more difficult to read than it should be.

DONALD C. MENZEL

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Engineering Disability: Public Policy and Compensatory Technology. By Sandra Tanenbaum (Philadelphia: Temple University Press, 1986. 167p. \$24.95).

The relationship between public policy and the diffusion of technology is explored by Sandra Tanenbaum in the context of compensatory devices to aid disabled persons. The book's two primary premises are that (1) disability is a loss of function that reduces mobility, employability, and other opportunities; and (2) governmental disability policy, in distributing compensation for losses associated with disability, redistributes loss itself. The impact of governmental policy on diffusion of compensating technology is explored and described within the context of the Boston Elbow, a prosthetic device used to aid persons with above-elbow amputations.

Tanenbaum describes traditional models used to explain the diffusion of technology—the "life-cycle" and "communications" models—and rejects them as inadequate for explaining the diffusion of rehabilitation technology. She offers, in their place, a "contextual framework" as a means to explain technological diffusion. The framework outlines the variety of alternatives that can be employed to compensate for losses in opportunities and abilities as the result of physical handicaps. The alternatives, whose applications are heavily influenced by public policy, include financial-support payments, modifications to the environment (e.g., installation of ramps), retraining, and prosthetic devices. The framework is used to explain the diffusion of the Boston Elbow technology to various groups, including disabled veterans, workers, citizens, and privately insured persons.

Tanenbaum's descriptions of technological diffusion of the Boston Elbow make it clear that the application of technology to diminish the impact of physical disability is *not* simply a match between professional diagnosis and technology at hand. Instead, the relationship between the two is mediated by the influence

of public policies, governmental institutions, and political processes. As one example, the author explains that diffusion of the Boston Elbow to veterans has been mitigated by (1) the array of compensation policies offered by the federal and state governments (e.g., cash benefits, retraining, prosthetic devices), (2) the preference of many in the Veterans Administration for a different prosthesis developed by the VA itself, and (3) the persistent pressure from interest groups representing disabled veterans for cash benefits as the primary mode of compensation for physical disability.

The real strength of the book is in documenting the multitude of ways in which public policies—including rehabilitation, welfare, and income-support programs—affect decisions about the diffusion and application of technology. It is clear from Tanenbaum's analysis that *who* receives the Boston Elbow is the complex result of many factors including the implementation of governmental rehabilitation policies and varied political pressures exerted by the disabled community.

In some respects the book might have been stronger. The framework offered by the author is a simple decision tree of the alternative responses that can be made to diagnosed disability. It does not directly establish a clear set of hypotheses relating various public policies to strategies for technological diffusion. Discussions in the book focus more on the development and application of rehabilitation technology than on how public policies were devised and how they are implemented so as to affect the diffusion of compensating technology. And, although it seems warranted, no systematic *evaluation* of rehabilitation policy and its impact on technology diffusion is offered.

Overall, readers of this book will gain important insights into the mediating influence of public policies on the distribution of technological compensations to physical disability. The findings of the book suggested that further research on this topic is warranted, both within the context of rehabilitation technology and in other areas of technological innovations.

STEPHEN L. PERCY

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Between Citizen and City: Neighborhood Organizations and Urban Politics in Cincinnati. By John Clayton Thomas (Lawrence, KS: University Press of Kansas, 1986. xii, 196p. \$25.00).

The literature on neighborhood organizations can in some ways be likened to the neighborhood movement itself: both evidence much enthusiasm while marching on spindly legs. Most of those writing about neighborhood organizations have been partisans. Those associated with the political Left have portrayed neighborhood organizations, especially those in low-income and minority communities, as building blocks to a broader movement to challenge existing political and economic institutions. More recently, some on the political Right have celebrated neighborhood organizations as a self-help vehicle that may help wean needy communities from their dependence on expanding governmental aid. Regardless of ideological bent, these authors have tended to build their case on anecdote, not analysis.

This book joins a rather select cadre of serious, empirically based, assessments of neighborhood organizations. Thomas has studied more than 40 community councils in Cincinnati. These community councils, most of which were born in the years between 1956 and 1980, operate in all parts of the city and have come to play a relatively well defined role in certain types of municipal decision making. Using various data sources, including a survey of over two thousand residents and interviews with 44 neighborhood leaders, he attempts to come to grips with such questions as: What factors account for the emergence of these organizations? Are these organizations more likely to form, or to take more sophisticated shapes, in affluent areas of the city? Do these organizations emerge and operate as alternatives to government, challenges to government, or extensions of government? And do they have a demonstrable influence on urban policies?

His answers are interesting. Thomas—like others—finds that affluence is important to understanding the emergence and institutional development of community-based organizations, but he documents, more convincingly than any other analysis that comes to my mind, the considerable role of additional

factors not directly associated with socioeconomic class. One such factor has to do with residents' stake in the neighborhood. In predominantly white neighborhoods, the importance of stake is revealed in the strong association between homeownership and organization, and in the tendency of organizations to form in neighborhoods confronting the perceived threat of racial change. In black neighborhoods the role of stake is tied to ownership and residents' reliance on neighborhood-level services. In addition to residents' stake in the neighborhood, Thomas finds that federal war-on-poverty programs, as well as some more contemporary efforts by private foundations, have played a major role in stimulating organizational development in neighborhoods lacking in indigenous resources.

Thomas is sympathetic—but not blindly so—to the organizations he studies. He recognizes that neighborhood organizations might threaten a city's interest by aggressively defending parochial or racially exclusionary concerns, but his reading of the Cincinnati evidence suggests that the community councils have played a generally constructive role. And he recognizes that the institutionalization of the neighborhood movement that seems to have occurred in Cincinnati potentially could co-opt neighborhood leaders, deflecting grassroots energies into symbolic or administratively burdensome undertakings, but he presents evidence that the gains of these organizations are substantial, particularly when it comes to influencing the city's distribution of discretionary funds.

In light of the fact that he considers Cincinnati a leader in the neighborhood movement, Thomas should have speculated a bit more thoughtfully about the differences that might be encountered in cities marked by sharper conflicts and histories of confrontation among neighborhoods, elected officials, and administrators. In the cities with which I am most familiar, it is not quite so easy to identify existing neighborhood organizations and neatly assign them to communities. As often as not, there are competing organizations, anchored in distinct constituencies and revealing different organizational structures and political styles. The tidiness of the Cincinnati situation surprises me a bit, and I wonder whether some messier realities may have been overlooked or underplayed. Finally, while Thomas acknowl-

edges the possibility that federal policies of the 1980s might deflate the neighborhood movement a bit, one could build out of his own data and findings a case for a substantially more pessimistic outlook.

JEFFREY R. HENIG

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The Attorney General's Commission on Pornography, Final Report. 2 vols. By the U.S. Department of Justice (Washington: GPO, 1986. 1,960p. \$35.00, paper).

Sexually explicit themes and imagery that arouse, attract, frighten, or offend are at least as old as civilization itself. It is instructive that no society has ever been completely "free" about sexual communication and representation and equally instructive that liberal societies, freer than most, have almost always had continuing pornography controversies. Now, for the second time in sixteen years, an ad hoc federal commission has studied the problem and reported. (During the same period, similar bodies have done so in Britain, Canada, and several other Western states.) If rising public concern and rapid changes in communications technology, distributive capacity, and marketing patterns are criteria, the attention is appropriate. The present U.S. generation is probably the most communicated with and pornographically bombarded in history. What the advantages and hazards of this may be, whether the hazards are at all related to crime and other social pathology, and how, where, and when the "new" pornography should be controlled, are enormously complex questions, clearly worth asking though not at all easily answered either in social science or in public policy.

In spite of the libertarian censure and generally unflattering press the attorney general's Commission received for its post-publication activist antics, the two-volume report itself is calm in tone, measured in most of its conclusions, and generally sensitive to First Amendment concerns and the diversity of values in the country. Unfortunately and perhaps inevitably, the aspect of the report receiving most attention upon publication was its finding of a causal relationship between exposure to sexually violent representation and sexual crimes and other aggressive

behavior towards women. The determination was based on the commission's analysis of existing experimental, clinical, and survey studies and on anecdotal data presented at its hearings. Unlike the 1970 Commission on Obscenity and Pornography, which reached substantially the opposite conclusion, the contemporary body was severely restricted in both time (a one-year life) and budget (a mere five hundred thousand dollars) to commission new work. Most of the existing research, however, as not designed to evaluate the possibility of a causal link between pornography and sexual crime, and the commission's reading of it is somewhat selective and often not unfree of the familiar error of interpreting correlation as causation. The studies are in fact inconclusive, often contradictory and, for the most part, lacking standardization in categories of analysis, types of stimulus material, and measurement of "effects." This has not stopped partisans on both sides of the pornography issue from seeking support from scientific investigation of effects; conclusive proof of harm or of harmlessness would presumably be the decisive hammer on the anvil of public policy. But social and psychological reality are not cooperative here. Paraphilic behavior is multicausal and complex, and even under ideal conditions of inquiry, it would be difficult to isolate pornography sufficiently to demonstrate its harmful or unharmful effects conclusively (or perhaps even tentatively). Moreover, the psychodynamics of human sexuality may be such that proof of harm would not make pornography much less attractive or compelling for those who find it so, or would proof of harmlessness make it much less offensive or disturbing for those who find it to be that.

Fortunately, most of the report is on sounder ground and makes important contributions to our understanding of the contemporary pornography explosion and aspects of its social control. Its analysis of the immensely profitable pornography industry, the latter's production, distribution, and marketing patterns, and the role played by new communications technology is detailed, and what is revealed may surprise those who have not been paying close attention to these developments. The same may be said for the probing of links to organized crime. Special attention is given to "child pornography," the very production of

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which almost always involves child abuse, in both a clinical and a legal sense. Its fourfold categorizing of explicit sexual theme and imagery—violent, nonviolent but degrading, nonviolent and nondegrading, and simple nudity—recognizes the complexity of the contemporary genre and offers considerable analytic utility in the study of effects as well as in possible policy discrimination.

In urging tighter controls, the report reaches a governing conclusion exactly opposite that of the 1970 commission, which was firmly in the hands of constitutional and media libertarians and faced a much different communications environment and a less widely distributed and somewhat less explicit pornography. The report offers 92 specific recommendations, including 49 on child pornography. Included are those for more vigorous prosecution under existing federal and state anti-obscenity laws, coordinated use of tax, business, and public-health laws that are often violated by proprietary interests, a higher priority for pornography on prosecutorial agendas through larger and earmarked budgets and additional personnel, special training for prosecutors, who are often overmatched in court, and stiffer penalties for offenders, including mandatory sentences for second and subsequent convictions. New federal and state law is recommended to allow for forfeiture of any proceeds involved in the violation of anti-obscenity laws. New federal legislation and Federal Communications Commission regulation is recommended to bar the transmission of obscene communication by telephone or by cable or satellite television. State legislatures are urged to enact racketeer-influenced corrupt organizations (RICO) laws that have obscenity trafficking as a predicate act. The discussion of the social control of pornography through law and law enforcement is probably the most systematic and sophisticated we have to date.

Paradoxically, no change is recommended in the current legal doctrine of obscenity. One can well understand a reluctance to enter that formulaic labyrinth, yet the Supreme Court's work has been one of the major shaping forces of the present communications environment that the commission indicts and is the chief reason why prosecutorial control is so often moribund.

The report recognizes both the limits of law enforcement and the political and moral role

that popular values and preferences inevitably play in underwriting formal rules and in filling the interstices of social control in democratic systems. Where citizens find sexually explicit materials to be offensive or believe them to be harmful, they are urged to exercise their own First Amendment rights to study, complain, lobby, lawfully boycott, directly protest and, above all, persuade. At the same time, the report cautions against "extreme or legally unsound positions or actions" (p. 1319) and urges recognition of the rights of those with opposing views. The line dividing healthy collective private action, pressure, and value articulation from coercive social conformity is often a thin one in liberal democracy, and, generally, the report treads it carefully.

Pornography probably defies any final social solution, libertarian or censorial, just as it may defy a conclusive psychological one for many, if not most, individuals. Management of the problem through public policy requires a balancing of both popular and libertarian values. In its broad though selective inquiry, the present report makes a plea for somewhat greater control in a superpermissive communications environment undergoing radical technological change. Thoughtful libertarians will find little that is reactionary in that.

RICHARD S. RANDALL

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Congress and Policy Change. Edited by Gerald C. Wright, Jr., Leroy N. Rieselbach, and Lawrence C. Dodd (New York: Agathon, 1986. xii, 292p. \$32.00, cloth; \$15.00, paper).

With all the research and writing on the behavior of individual legislators and the intricacies of legislative structures, it often seems that policy consequences do not get adequate attention. What difference does it make whether or not, for example, members of Congress are fixated upon reelection, or whether their careers are long or short? What is the result of "subcommittee government" upon policy generation? And how can the admittedly momentous Capitol Hill innovations of

the 1960s and 1970s be incorporated into a convincing theory of legislative change?

This collection of papers addresses the problem of linking individual lawmakers' behaviors, institutional dynamics, and policy outcomes. Many of the contributors are associated with Indiana University, and these papers were first presented at a 1983 conference held there (though the title page affiliates the co-editors with a nonexistent "University of Indiana.") The result is a happy mix of studies, approaches, and levels of analysis. The contributions range from theoretical frameworks (Dodd's opening essay on cyclical change of Congress and Rieselbach's closing survey of the state of research) to elaborate descriptive surveys (David Brady on electoral realignments, Wright on House elections, Barbara Sinclair on party leadership, and Roberta Herzberg on procedures). There are even some instructive case studies of policies (Edward Carmines and James Stimson on civil rights and John Ferejohn on food-stamp legislation, a textbook example of the logroll).

One virtue of the collection is its sensitivity to what I have called elsewhere the "two-Congresses" problem—complex interaction between individual and institutional forces. This problem is reflected in two interesting essays—by Richard Fenno and Roberta Herzberg—that explore learning on the part of individual senators and representatives.

In making policy, Congress is an active shaper of agendas and outcomes, not merely a reactor to external or internal stimuli. In civil rights, for example, Carmines and Stimson find Congress leading, rather than following, the public mood—an intriguing finding that stands on its head the classic 1958 formulation

of Miller and Stokes.

The difficulty of linking elections to policies is that most elections are local in content and "ordinary" rather than realigning in their results. Nonrealigning elections, as in 1946, 1958, 1964, and 1974, can produce substantial policy shifts. And apart from the actual returns, how elections are reported or interpreted can alter the policy agenda, as happened in 1980 and again in 1986. The papers of Wright and of Carmines and Stimson, especially, cast light on this complex linkage between electoral results and policy outcomes.

In his useful summary, Rieselbach lays out the parameters and identifies gaps in the cumulative body of research. More attention needs to be paid to the relationships between external actors and forces on the one hand and such internal actors as leaders on the other. Also required is study of the role of public opinion or "national mood" (p. 270). We also need, as Rieselbach observes (p. 282), more probes of specific policies, preferably in some quantifiable or at least comparative fashion. My own studies of legislative "productivity" over time suggest contours that would further complicate, and perhaps illuminate, the processes of change discussed in this volume. While different analysts would doubtless emphasize things somewhat differently, the research strategy embodied in this volume—that of commissioning experts to reconsider their subject matter in light of a given dependent variable (in this case, policy change)—is extremely valuable.

ROGER H. DAVIDSON

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COMPARATIVE POLITICS

Race and Politics: Ethnic Minorities and the British Political System. By Muhammad Anwar (London: Tavistock, 1986. x, 182p. \$39.95).

Ideologies and Institutions in Urban France: The Representation of Immigrants. By R. D. Grillo (Cambridge: Cambridge University Press, 1985. xv, 328p. \$44.50).

The volumes under consideration employ the diverse disciplinary perspectives of political sociology and cultural anthropology to investigate the integration of immigrants in two Western capitalist nations. The first charts the progress of New Commonwealth and Pakistani immigrants in exercising the rights of British citizenship they formally enjoy. The

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second identifies the ideological processes involved as immigrants without citizenship interact with the dense institutional networks of French society.

As head of research for the Commission for Racial Equality, Anwar has followed ethnic politics in Britain closely and is responsible for initiating a number of the most helpful studies he summarizes. His presentation is comprehensive, lucid, and despite the obvious sympathies of the author, remarkably balanced and dispassionate. He demonstrates that although minorities make up only about 4.2% of the British population, their impact on politics is much greater than numbers alone would indicate. Anwar estimates, for example, that the ethnic vote significantly affected the outcomes in 85 (out of 600) constituencies in the February 1974 election as a result of the combination of residential concentration and the marginality of the seats. Minorities, he shows, are slightly less likely than whites to register to vote (78% compared to 81%), but the gap appears to be closing. There are no national figures on ethnic turnout, but studies of selected constituencies show that Asians have a consistently higher turnout rate than non-Asians, though one must take into account the socioeconomic characteristics of whites living in constituencies of high ethnic concentration. Minorities vote for Labour in overwhelming numbers—usually around 75%. Anwar's book is especially valuable for its detailed account of the fate of ethnic-minority candidates (none has yet won a parliamentary seat) and its treatment of the policies of the parties toward immigration and their own immigrant members.

The chief criticism I would offer of *Race and Politics* is that it fails to address the issue of the real or potential costs to the major parties of supporting immigrant causes. The author is eager to point out the benefit parties (especially Labour) derive from the ethnic vote, but his chapter on the antiimmigrant movement deals exclusively with the National Front, which he tends to discount, and skirts the specific question of whether there has been a significant defection of antiimmigrant voters from the Labour to the Conservative party. This is part of a more general reluctance to consider how tensions over immigration may have affected the ideological context of British politics.

Grillo's absorbing study employs anthropological field methods to investigate the inte-

gration of immigrants in the highly industrialized setting of Lyon, France. He departs from normal practice by taking as the primary subject of his study the society of immigration rather than the immigrants themselves. This leads him to systematic observation of those institutions, mostly created and run by native French, that receive, process, serve, and manage immigrants: the housing system, the schools, adult language centers, the social services, and the workplace. Notably sparse in his text, however, is discussion of the police or immigration authorities proper. Whereas Anwar may be accused of treating the integration of immigrants as an unquestioned good blocked only by the pigheadedness and occasional venality of white Britons, Grillo subtly elucidates the controversial and ambiguous meanings of the very ideas *immigrants* and *integration* and the deep ambivalence both immigrants and French exhibit toward them.

If nothing else, the book would be valuable as a virtual treasure trove of judicious observation and analysis of the daily experiences of immigrants with the institutions of the receiving society and of the behavior of street-level bureaucrats toward their clients. But the book is more than that. It sensitizes the reader to the political implications of the categories and theories employed by ordinary French persons when they think about immigrants. The most compelling section of the book is a trenchant discussion of the two modes of discourse implicit in choosing to talk about *immigrants* (*immigrés*) as opposed to *foreigners* (*étrangers*). The book as a whole produces a heightened appreciation of the complex and problematic nature of any conception of immigrant interests and the consequent difficulty of designing policies to respond to those interests. Grillo uncovers the tensions built into such seemingly innocuous and simple objectives as teaching French, or providing adequate housing and social services. These tensions arise as much out of the incompatibility of the values and goals of particular segments within the immigrant population as they do out of conflict between immigrants and their French hosts. This more or less ensures that the policies will fail, according to someone's criteria, no matter how much good will or technical and financial resources are invested in them.

Setting the British and French cases side by

side, one is led to ask how important the presence or absence of citizenship is to the status of the immigrant groups included in the studies. Citizenship, one might assume, should eliminate some of the uncertainty among the host populations as to the intentions and likely role of immigrants. In fact, citizenship does not put an end to the political struggle over the terms with which immigration is discussed. The subtitle of Anwar's book provides the clue: he prefers to write of *ethnic minorities*, while most Britons continue to speak of *immigrants*.

GARY P. FREEMAN

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The Qashqa'i of Iran. By Lois Beck (New Haven: Yale University Press, 1986. xvi, 384p. \$30.00).

The Elementary Structures of Political Life: Rural Development in Pahlavi Iran. By Grace Goodell (New York: Oxford University Press, 1986. vii, 362p. \$45.00).

Rural Iran is a rich subject of inquiry for all social scientists. It is an area of varied cultural settings, diverse forms of economic and social organization, and differing political significance. Although the rural society has rarely set the tone for national politics in Iran, it has always been of critical importance in accepting, modifying, or rejecting central-government dictates. The two books considered here are major attempts by two fine anthropologists to understand the complexities of Iranian rural life.

Beck's book is about the major tribal confederation of the Qashqa'is in southern Fars province, Iran, with an estimated membership of three to four hundred thousand. It reviews the Qashqa'is' origin, history, social organization, and their complex relationship with the Iranian state. Goodell's book is about a traditional village and a model "rural" town in the southern Khuzestan province. It is an in-depth microanalysis of these two communities and their relationships with the national political authority during the reign of the last Pahlavi monarch. Both books are based on extensive field research and varying degrees of participant observation. They also benefit from the respective authors' broad knowledge of Iranian culture and history. They have a lot to

say to political scientists interested in the interaction between rural society and national government in Third World countries.

Beck begins with a long historical account of the Qashqa'i confederation using many rich and varied historical sources. This is perhaps the first comprehensive history of the Qashqa'is in a Western language. The account begins with the initial formation of the tribe in the eighteenth century and its expansion and absorption of various elements, and proceeds to deal with the confederation's role vis à vis both domestic dynasties and foreign governments with a foothold in Iran. The bulk of the historical material concerns the Pahlavi period. The book, however, ends with a detailed discussion of the Qashqa'is plight under the Islamic republic. Throughout the book and in well-organized special sections, we are provided with a highly useful analysis of the confederation's social organization, economy, customs and mores, genealogy, and economic and political life. In short, this is an exceptionally rich source that sheds light on all aspects of this major tribal confederation in Iran.

Goodell begins with a description of the Dez irrigation project, which envisaged making use of waters of three major local rivers for purposes of development of the Khuzestan basin. She describes how the idea of Dez development was initiated by David Lilienthal, the architect of the Tennessee Valley Authority, and the Iranian government as a way to tap the resources of the province. The project was to be a showpiece of the shah's program to transform and even revolutionize rural Iran into something modern. Goodell then devotes several chapters describing a traditional village in an area untouched by the project. These chapters give detailed accounts of the village's corporate life, touching on its personal, socioeconomic, and political aspects. She then applies a similar overreaching framework to analyze life in one of the modern towns built by the project planners. She concludes with a long chapter that compares and contrasts the two localities and evaluates their respective place in Iranian rural life. More importantly, the chapter makes many bold attempts to discuss agriculture and rural life in Iran in comparison to the European and Japanese models. Her final and clear conclusion is that the Dez irrigation project was a colossal error.

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Beck and Goodell strongly identify with the populations they study. Beck's point of reference is often how certain historical and political developments have affected, favorably or adversely, the Qashqa'is. One sometimes gets the impression that whatever is good for the Qashqa'is is also good for the country as a whole. While this may or may not be true, it tends to color the author's analysis, particularly when she discusses the Qashqa'is' relationship with the state.

Goodell's identification with her village is exceptionally strong. She portrays a highly successful corporate village where the values of "moral economy" predominate. Everything seems to work in the village in spite of poverty or other problems. She laments the destruction of the corporate village through the Dez or similar ill-conceived projects. Goodell's final chapter and her discussion of how hierarchy and authority can develop in a manner responsive to the base of society, are controversial. Even though one may not agree with everything that Goodell says, it is clear that she is dealing with an important subject in a very interesting manner.

Beck and Goodell have given us two valuable books on rural Iran filled with detailed information and sound analysis. They deserve credit for their successful and scholarly undertakings.

FARHAD KAZEMI

New York University

Communism and Development. By Robert Bideleux (New York: Methuen & Co., 1985. x, 315p. \$39.95).

Classical Marxism identified the goal of communist revolution with the rise of the urban-based industrial working class and assumed its achievement would be possible only after lengthy capitalist development brings an advanced level of industrialization, urbanization, and technological sophistication. Yet, by an irony of history, Communist parties have thus far come to power by their own efforts only in countries still seriously underdeveloped or semideveloped, where the largest social class has been neither *proletariat* nor *bourgeoisie* but rather the tradition-bound peasantry. Communist regimes have found

themselves in the theoretically anomalous position of creating the "objective" developmental preconditions for their own existence. The book under review represents an attempt both to describe the main outlines of the developmental strategies undertaken or debated in the various Communist countries and to reappraise the concrete historical consequences of these strategies from a radical Socialist (but non-Marxist-Leninist) perspective. In this analysis, the author's key variable is the political-economic role assigned to the rural sector in the various developmental strategies.

The author details at length and vehemently condemns both the theory and the practice of the widely applied Stalinist and neo-Stalinist strategies of forced-pace industrialization and coercive rural collectivization, with their fetishistic emphasis on the development of urban-based producer-goods industries at the cost of impoverishing the rural economy and suppressing the living standards of the population generally. He argues that in addition to fostering unconscionable wastage of resources, forced industrialization has inevitably made a mockery of Socialist values by entrenching new forms of socioeconomic inequality and political hierarchy and by giving free rein to both corruption and the terroristic abuse of power.

The author does not wish merely to recapitulate the familiar thesis that the human and economic costs of Soviet-style developmental policies have been exceedingly high wherever they have been employed. He wants to convince us that there were (and are) workable alternative approaches to Socialist development for mainly agricultural societies that would be *both* more economically efficient *and* more consistent with attaining the egalitarian, communitarian, and democratic ideals of classical Socialist thought. Consequently, much of the book consists of a near-encyclopedic survey and reevaluation of arguments in favor of an historical miscellany of certifiably Socialist (if sometimes obscure) alternatives to the dominant Stalinist approach.

At the level of theory, primary attention is given to two traditions: (1) decentralized "village communism" as variously elaborated by Herzen, Kropotkin, Chayanov, Gandhi and (perhaps) the elderly Marx; and (2) "market socialism," as represented in the ideas of Proudhon, Bukharin, Lange, Sik and many

of the Yugoslav theoreticians. In analyzing the historical experience of actually existing socialisms, the author ranges widely indeed, offering a running commentary on the evolution of Socialist rural-development policies in the USSR, China, Yugoslavia, Poland, Vietnam, North Korea, Hungary, Cuba, and Mongolia, with frequent allusions to the experiences of rural development in such capitalist or mixed economies as those of South Korea, Taiwan, Japan, Denmark, Ireland, Tanzania, and India.

The author is up against some familiar but major methodological problems, because his goal is to demonstrate the practical superiority of one or more "ideal-type" systems whose total configurations have never (yet) been closely approximated in practice over a set of Stalinist institutions and policies that are unfortunately all too historically real. It is an inherently speculative enterprise at best, and this particular effort unfortunately cannot be recommended on the basis of any special analytical rigor in its approach, which is both discursive and eclectic. Specialists with expertise on particular countries or thinkers will notice inadequacies in certain details. Nevertheless, the book is lively and at times extremely thought-provoking, while displaying considerable ingenuity in marshalling secondary source materials with a bearing on its larger comparative themes.

PAUL M. JOHNSON

Florida State University

Crisis in the Philippines: The Marcos Years and Beyond. Edited by John Bresnan (Princeton, NJ: Princeton University Press, 1986. 284p. \$30.00, cloth; \$10.95, paper).

The assassination of Benigno Aquino provoked the Asia Society to commission this book. The collection went to press shortly after Benigno's wife, Corazon, became president of the Philippines, ending 20 years of the Marcos government. What happened in the Philippines during and because of the Marcos government are questions that have been addressed many times before. The usual answers are by now familiar to those who read regularly about the country's national politics and economy. Someday deeper analyses that

penetrate and inspire will appear, but today is not that day.

Favorable examples of the usual accounts constitute six of the chapters in this book. They are informative reading for anyone who wants to become acquainted with this subject. Lela Noble's "Politics in the Marcos Era" is a factual chronology that begins with Marcos's first term as president (1966-1969) and ends with his demise in 1986. She touches on all the accepted high points, such as problems of governance preceding and leading up to Marcos's declaration of martial law in 1972, many people's high hopes and Marcos's big promises during the first three or four years of his "constitutional authoritarian" rule, and the economic and political decline in the late 1970s leading to numerous disasters between 1981 and 1985, the rise of opposition groups, and the growth of armed rebellion.

Subsequent essays elaborate on major events and problems. Carl Lande concentrates on legal and illegal organizations that opposed the government and its policies. Carolina Hernandez emphasizes the destruction of governing institutions and government credibility during the Marcos years. She also reports on steps President Aquino and other prominent persons are taking to reconstitute democratic political procedures. How the U.S. government began to be disillusioned with Marcos is the main subject of the chapter by William Barnds.

Two economists, Bernardo Villegas and Jesus Estanislao, separately explain the economic crisis facing the new government. Both say that one important cause was excessive government intervention in the economy. That predated Marcos but was taken to extremes during his one-man rule. A second cause on which they agree was an overemphasis on import substitution and high protective tariffs. Again, these began before Marcos but were not reversed. Their recommendations for recovery include, logically enough, reduced government interference in the market, expansion of export-oriented manufacturing, and lower tariffs and other ways of opening the Philippines to the world market.

The remaining three chapters will be useful for those trying to understand deeper forces influencing Philippine society. Coauthors Ricardo Abad and Wilfredo Arce argue that central to the problems in the Philippines—

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Marcos or no Marcos—is the gaping inequality. This will continue to block progress and fuel unrest so long as the rich and powerful minority insists on having all that it now has, surrendering nothing, and so long as the government, knowingly or not, fails to do anything against such a stance or anything substantial to assist the poor and weak majority to bring fundamental changes.

The roots of this inequality, as suggested in the chapters by Theodore Friend and David Steinberg, lie in an ideology that lauds ostentatious consumption and wealth. Earlier colonial rule, and now neocolonialism in the form of ties to and influences from the U.S., have helped to perpetuate this orientation and the wealthy minority's power. Friend worries that the "illusion of American omnipotence and the reality of Philippine oppression could fuse" in a Philippine civil war. "Because of the major American bases there, a creeping [U.S.] involvement in a deteriorating situation is imaginable, and could turn into a nightmare."

BENEDICT J. TRIA KERKVIJET

University of Hawaii

Apprentice Revolutionaries: The Communist Movement in Laos, 1930-1985. By MacAlister Brown and Joseph J. Zasloff (Stanford, CA: Hoover Institution Press, 1986. xiv, 463p. \$13.95).

This, the eleventh volume in the Hoover Institution series, *Histories of Ruling Communist Parties*, is Zasloff and Brown's third book on Laos and Indochina. Part 1 traces the rise of the Lao People's Revolutionary Party (LPRP) to power in 1975; part 2 describes the leadership, institutions, policies, and external relations of the Lao People's Democratic Republic (LPDR). Though focused on the Communists, the book includes enough material on other actors to serve as a concise political history of modern Laos. Thorough research, scholarly detachment, clear writing and nearly a hundred pages of appendices make this book a valuable basic source on what may be the world's most forgotten revolution.

The principal task Brown and Zasloff set for themselves was to review the special relationship between the Lao and Vietnamese Com-

munist movements from the founding of the Indochinese Communist Party in 1930 to the present. This focus causes the authors to pose such main questions as why the Lao Communists were much more submissive "apprentices" than their Khmer Rouge counterparts, how the LPRP conducted its revolutionary struggle against the Royal Lao Government, how the seizure of power differed from that which occurred in Vietnam and Cambodia, whether the Vietnamese harbor a design to create an Indochina Federation, and whether development policies based on Marxist-Leninist doctrine can be viable in this extremely poor small country.

In answering these questions, the authors stress four "decisive elements" in the evolution of the LPRP. The first of these was the emergence of Lao *nationalism* in response to the Japanese occupation during the Second World War and expulsion of the French colonial administration in 1945. The elite nationalists who seized this opportunity to rid themselves of the French quickly found that their survival depended on allying with the Vietnamese Communists. Thus, nationalism drove the first generation of Vietnamese. The second important element was Laos's *ethnic diversity*. Although few nonethnic Lao were to attain leadership positions in the party, the Lao Communist movement successfully rooted itself among the border-straddling minorities of the eastern mountain villages. This experience imprinted the Communist leadership with a distinctive outlook and helped to consolidate ties with the Communists in Vietnam.

The third element has been the constant presence of *Vietnamese advisers*, training, economic aid, and military operations on Lao soil. Dependence on the Vietnamese has constricted LPRP foreign-policy options to those approved by Hanoi, causing Laos to suffer uneasy relations with Thailand, hostile relations with China, and the loss of U.S. aid. However, the authors speculate that the LPRP leadership is so thoroughly accustomed to collaboration with the Vietnamese that it can scarcely conceive of loosening ties to Hanoi, and Hanoi in turn neither needs nor wishes to formalize its dominance in a federation. Last, the authors argue that the *low level of economic development* actually benefitted the LPRP in wartime. The lavish U.S. effort overwhelmed existing economic and social relation-

ships, supplying the party with issues around which to mobilize a dislocated and resentful population. But the peacetime "Socialist transformation" of a society that never had serious problems of landlordism or land shortage has alienated the support of a people still largely dependent on family-based subsistence agriculture.

The book's principal shortcoming is an effect of its strength. Primarily an internal history of the party, it makes no pretense to profound analysis of the interaction between the party and its social environment. Thus the authors maintain that the classical Asian Communist themes of anticolonialism and land-to-the-tiller had weak appeal among ordinary Lao, but they offer little to explain how, then, the Communists were able to secure broad popular support as the war progressed, despite the enormous sacrifices giving this support required people to endure. An implied thesis, that the movement drew strength from the defensive reaction of traditional rural communities to war and change more than from popular nationalism or demand for egalitarian redistribution, is not fully explored. The authors also perpetuate the stereotype of the Lao people as "gentle," "joyous," and "complaisant" yet provide contradictory descriptions of "grim, self-disciplined" Communist troops and low-level cadres who, though incompetent, are zealous and persistent in implementing their orders. This book does not really tell us how LPRP leaders, made dynamic, disciplined, and energetic by formative years spent at work and study in Vietnam, succeeded in motivating a significant number of their "complaisant" countrymen to become hardened revolutionaries. But for readers seeking a reliable introduction to the politics and problems of contemporary Laos, this book is highly recommended.

WILLIAM S. TURLEY

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Federalism and Federation in Western Europe.

Edited by Michael Burgess (Dover, NH: Croom Helm, 1986. 227p. \$31.00).

In large part a result of a workshop on comparative federalism and federation in Western Europe held at the University of Salzburg in

1984, this collection of essays is divided into two parts: "Federalism" (part 1) and "Federation" (part 2). Following a brief introduction, Michael Burgess in chapter 2 distinguishes between these concepts. Borrowing from the work of Preston King (*Federalism and Federation* [London: Croom Helm, 1982]), Burgess suggests that "federation is a specific organisational form which includes structures, institutions and techniques" (p. 17). A crucial element in the institutional arrangements is constitutional autonomy for regional units. *Federalism*, on the other hand, is related to the concept of *pluralism*. It can be "taken to mean ideological position, philosophical statement and empirical fact" (p. 20). Thus it can "mean the recommendation and (sometimes) the active promotion of support for federation" or "viewed as empirical fact to the extent that it recognises diversity—broadly conceived in its social, economic and political contexts" (p. 20). According to this distinction, federation is one means of dealing with federalism. Other means might include not only some form of regionalism, decentralization, or devolution but also corporatism and the Catholic doctrines of personalism and subsidiarity.

The remaining chapters of part 1 provide case studies of federalism in Belgium (Frank Delmartino), Spain (Antoni Monreal), France (John Loughlin), Ireland (Neil Collins), and Britain (Michael Burgess). Many readers might have expected some discussion of Belgium, Spain, or even Ireland in a book on federalism and federation, but the inclusion of Britain and especially France is surely surprising. Yet given their definitions of *federalism* and *federation*, the authors have little difficulty fitting many British ideas regarding the empire, Ireland, and Europe or French *de jure* centralism and *de facto* toleration of local cultures into their conceptual scheme. The decentralization that has taken place under Mitterrand can also be seen as a movement "toward some kind of internal federalism" (p. 93).

Part 2 deals with the three German-speaking federations in Europe: Austria (Richard Luther), Switzerland (Ulrich Kloti and Kurt Nussli), and West Germany (Tony Burkett). The chapter on the revitalization of federalism and federation in Austria is really about the Pro-Vorarlberg Movement in the westernmost *Land* and its effect, together with other factors including party conflict, on federal reform in

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Austria. In contrast to many studies of Austrian parties and political economy, this chapter demonstrates the limited usefulness of corporatist theory in discussing Austrian federalism and federation. The chapter on Switzerland focuses on the efforts in recent years to reform the federal system, during which time there has been a change from an emphasis on greater cooperation, coordination, and efficiency to more task differentiation and autonomy for the cantons. Indeed, the review of recent proposals in Switzerland is strikingly reminiscent of the "New Federalism" of the Reagan administration. As in the U.S. case, however, little fundamental change in task distribution has actually occurred. Nevertheless, the centralizing tendencies found in most other federations seem to have been arrested. Following a review of important distinguishing features of German federalism, the chapter on West Germany focuses on the Bundesrat and the crucial role it plays. (For a review of Gunlicks's book on German Federalism, see p. 1029.)

In summary, this book makes an important contribution to the study of federalism/federation, both in terms of the conceptual framework employed and the quality of the individual country studies. Given the competence of the editor and authors, one wonders how and why a number of careless errors were not corrected. In the chapter on Belgium (p. 47), a two-column comparison of regionalism and federalism is made virtually incomprehensible by missing lines; a subtitle in the chapter on France has been attached to the end of a sentence (p. 78); the Sozialdemokratische Partei Deutschlands (SPD) in the chapter on Germany is referred to as the "SDP" and Konrad Hesse is called "Karl." In all three chapters of part 2 there is a most irritating failure to provide umlauts or, in lieu of them, *es* where required in numerous German terms. While there is a brief index, there are no maps, which would have been very useful to those readers not familiar with all of the regions and areas mentioned in the various country case studies.

ARTHUR B. GUNLICKS

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Mexico's Political Stability: The Next Five Years. Edited by Roderic A. Camp (Boulder, CO: Westview, 1986. ix, 279p. \$23.00, paper).

As editor Roderic Camp sets forth in his introduction, the six authors analyze the critical issues confronting Mexico by focusing on five major themes: stability, legitimacy, economic viability, societal change, and Mexican-U.S. relations. Though not explicitly recognized as such, the two independent variables are *economic viability* and *societal change*. William Glade examines the economic challenges confronting Mexico and astutely emphasizes three constraining factors: the high rate of population growth, the external-debt burden, and the collapse in oil prices. Stressing a theme found throughout the book, he describes the economic liberalization of the present administration as seen in more open trade policies and more flexible foreign-investment regulations.

Daniel Levy and Peter Smith outline several changes in societal values and elite recruitment patterns. The widespread conclusion of the growing dominance of technocrats is reiterated by Smith, though he minimizes any threats to stability this trend might pose. Levy raises a more ominous issue: a growing gap between elites and the masses (also linked to urban-rural cleavages), which has been due in part to a "denationalization" of education, that is, more emphasis among elites on private and foreign education.

Stemming from the discussion of these independent variables, one of the most attractive qualities of this book is the focus by several authors on important but often overlooked interest groups. In particular, John Bailey analyzes the private sector and its relations with the state, and Edward Williams explores the changing role of the military. Bailey's conclusion that greater business input is evidenced in current policies of economic liberalization is one of the overriding themes of the book. Williams describes the military as a more prestigious pillar of the Mexican system (due to its important functions of policing elections in the North and protecting national security along the southern border), but he also stresses the persistence of civilian superiority over the military.

The authors collectively address three

dependent variables: *stability*, *legitimacy*, and *U.S.-Mexican relations*. Though they recognize several threats and challenges to both the stability and legitimacy of the regime, they wisely refrain from dire predictions of impending doom. The decline of the Partido Revolucionario Institucional (PRI) and the growth of the opposition are obviously problematic areas for the dominant coalition. But the authors argue that opposition victories (and charges of fraud) have been exaggerated by the U.S. media, that the PRI continues to be strong nationally, and that the presidential/bureaucratic alliance has the capacity to initiate changes to restore legitimacy to the ruling party. The book consistently deprecates predictions of violent destabilization or radical change. The only institution capable of forcefully challenging the existing regime is the military, but it remains subordinate to the civilian elites.

As a volume of particular interest to U.S. policymakers, several issues relevant to U.S.-Mexican relations are addressed: trade and investment, border and immigration, and Central America. While admitting the undeniable differences between the two countries on most of these issues, the contributors generally recognize the common benefit to both nations of preserving the status quo in Mexico, namely, a benignly authoritarian system under a centralized presidency and hegemonic party with a flexible approach to economic policies.

In conclusion, this is not a book for those readers wanting explosive predictions of unmanageable political and economic crises threatening the Mexican system. The authors provide a balanced, accurate, and current analysis of present problems and probable scenarios of change. Policymakers and scholars alike are much better served in our quest for understanding the Mexican dilemma by this objective presentation than by the more sensationalist efforts frequently found in the popular press.

DALE STORY

University of Texas, Arlington

The Working Class and Welfare. By Francis G. Castles (Sydney: Allen & Unwin, 1985. xxxiii, 128p. \$A24.95, cloth; \$A13.95, paper).

Australian Politics: Theory and Practice. By Bill Brugger and Dean Jaensch (Sydney: Allen & Unwin, 1985. xiii, 258p. \$A29.95, cloth; \$A15.95, paper).

Poor Nation of the Pacific: Australia's Future? Edited by Jocelynne A. Scutt (Sydney: Allen & Unwin, 1985. xii, 137p. \$A19.95).

There is no shortage of good general texts on Australian politics (Dean Jaensch himself has published one), but in attempting to write of political ideas and their influence on policies and institutions in Australia, the authors of *Australian Politics* may fairly claim a first. The authors' major intellectual point is that the operative range of political ideas in Australian politics is so narrow that Burkean conservatism, classical liberalism, and coherent socialism were never serious political influences. In their stead a vaguely formulated populism has combined with an equally vague utilitarianism to produce the regulatory state. Under its aegis Australia produced "marsupial capitalism" and a regulated labor market. The first two sections—on ideology and power—are dense, but they are not the usual stuff of textbooks on Australian politics. Unfortunately the second half of the book, on institutions, is fairly routine and does not amount to an account of the influence of ideas on institutions and policies. The account the authors offer is not inadequate, but it simply does not seriously demonstrate the relationships in Australia between ideology and policies or institutions.

In that respect Francis Castles is much more successful. His focus is the weakness of the welfare state in modern Australia (New Zealand is also considered), given their relative leadership towards the end of the nineteenth century. By 1950, despite being a pioneer in enfranchising the working class, having a strong trade-union tradition and an established Labor party, Australia was well down the world list in terms of percentage of GNP devoted to public social security. This fact Castles attributes as "substantially due to the exclusion of the Left from office and to the fact that rightist parties . . . were subject to remarkably little pressure for welfare state reform" (p. 68). Castles's account of the structures leading

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to the failure of the Left to achieve office is brief (Brugger and Jaensch are more helpful), as is his explanation of the "failure" of labour to press for a welfare state. If brief, though, Castles's account is convincing: the unions went for "wage security for the worker rather than social security for the citizen" (p. 87). Provided one is employed, the regulatory state ensures that payment is sufficient to ensure a reasonable living standard. He is less successful, perhaps less interested, in *why* Australian labor opted for "economism" rather than "socialist doctrine, whether of a social democratic or any other kind" (p. 104). However, Castles is surely correct in one thing: whatever the past benefits for the working class of its policy of wage security via the state, the chickens are coming home to roost in an era of intense international competition and growing national unemployment. The era seems bound to produce a substantial body of unemployed workers, who will be excluded from what Castles calls "the wage earners' welfare state" (p. 103).

It is this context of a high-wage economy within "marsupial capitalism" that Scutt's team addresses without, it must be said, coming to any apparent agreement beyond the affirmation in Scutt's opening essay that "becoming the poor people of Asia would be only too easy. All Australia has to do is continue along present tracks and time will take care of the rest" (p. 16). Together with the inflexibilities outlined by Castles and detailed by Brugger and Jaensch (see especially the latter's chapters on bureaucracy and federalism), the numerous disagreements among Scutt's contributors suggest that no radical change of political course is possible. All three volumes suggest that the structural rigidities of the Australian political economy will not yield to piecemeal, incremental, change since, in Scutt's words, "Australian unions and professional and industrial organisations typically exert pressures" (p. 16) against change. One of the politicians contributing to the collection, admittedly out-of-office, concludes, "Perhaps [Australia] has now reached a point where a process of decline is irreversible" (p. 92). Relying since the mid nineteenth century on a series of lucky breaks—discovering various resources—Australia may have run out of luck.

Contrary to the general tone of all three volumes, however, the various deregulations

of the last two years may indicate a greater flexibility than they suggest. The New Right has certainly added a wider intellectual dimension to the rather narrow Australian ideological spectrum. And the growing assertiveness of Japan in both international trade and politics gives added reason for Australia to awake. It requires a skill less than that of Nostradamus to see interesting times ahead in the Pacific rim!

ROBERT DOWSE

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Post-Revolutionary Nicaragua: State, Class, and the Dilemmas of Agrarian Policy. By Forrest D. Colburn (Berkeley: University of California Press, 1986. xi, 145p. \$17.50).

The majority of countries that have attempted a revolutionary rupture in the post-World War II period have been small, trade-dependent economies, principally primary producers. As Colburn reminds us, smallness (whether in population, natural resources, or preexisting industrial base) severely constrains development options. Autarchic development strategies are out of the question.

Colburn argues that structural constraints, particularly trade dependence, not only limit policy options but make it difficult to rapidly improve the welfare of the majority of the population, the rural poor, which is the stated goal of most revolutionary processes. In the Nicaraguan case, he shows how the inherited model of accumulation, based on agroexports, forced the Sandinista government both to concentrate state resources on the generation of foreign exchange to the detriment of basic grain production and to limit wage increases. Moreover, he documents the overwhelming importance of the private sector in Nicaraguan agriculture and demonstrates how the Sandinistas were forced to offer concessions to capitalist agro-exporters, particularly cotton producers, and reaffirm their commitment to private property and efficient capitalist farming. More controversial is his claim that "subsistence" producers and rural workers were "squeezed" in order to keep the revolution economically and politically solvent (pp. 1-2).

Colburn well illustrates many of the tensions involved in maintaining a mixed economy in

the context of a revolutionary transition. He fails, however, to come to terms with the Nicaraguan dilemma: Will capitalists play by the rules of the game of an economic and political system being redesigned to serve the interests of the majority if Uncle Sam beckons with promises of state power on condition they join the counterrevolution?

Colburn largely disregards the role of international political and economic factors by arguing that his analysis is confined to the "intentions of the Sandinistas" and to demonstrating why "their rosy expectations were dashed even before confronted with a U.S.-financed counterrevolution" (p. 2). He all but ignores the Reagan administration's campaign of economic destabilization against Nicaragua (which began promptly with Reagan's inauguration in 1981) and the sabotage and economic impact of the Contra war (which began in 1982), as well as the influence of U.S. policies on Nicaraguan internal politics.

While Colburn's theoretical framework highlights the difficulty of structural transformation in trade-dependent economies rather than analyzing structural factors (such as the effect of falling export prices on policy options), Colburn's agenda is to demonstrate that almost everyone in rural Nicaragua was worse off before the Contra war began. Neither his data nor economic analysis are up to the task. To begin with, he often relies on 1983 data (pp. 54, 96) to make his case; by 1983 the combined effect of the war against the Contras, U.S. economic-destabilization policies, and deteriorating terms of trade were, indeed, shaking the Nicaraguan economy.

Particularly problematic is his analysis of the situation of rural workers (chap. 6). He correctly identifies a trend of declining real wages. However, he fails to investigate whether rural consumption levels might have risen through such government programs as consumption subsidies on basic goods, improved rural distribution systems or increased access to land for subsistence production of basic grains. Moreover, rather than analyzing the effect of improved health-care delivery and education on rural welfare, Colburn simply asserts that "rural Nicaraguans clearly perceive increases in public services as inadequate compensation for a deteriorating economy" (p. 119).

One of several errors in his economic

analysis is his charge that besides lower real wages, rural workers faced "rising unemployment" as a result of Sandinista policies (pp. 116, 122). This is inconsistent not only with his earlier analysis of the falling profitability of coffee production (which he attributes to rising labor costs) but also with Nicaraguan reality. Witness the growing number of urban volunteers trucked to the coffee and cotton harvests each year because of severe labor shortages. Precisely as government officials feared, one of the effects of the agrarian reform—giving landless peasants access to land—was to dry up the seasonal labor market. But Colburn gives the Sandinista land-redistribution program only cursory treatment, ignoring a crucial element for a rigorous analysis of rural labor-market conditions and potential income levels, not to mention, of popular support for the revolution.

Colburn's political analysis, too, is often deficient. By dismissing the role of the mass organizations as vehicles of popular participation, Colburn misses the important ways these have influenced Sandinista policy decisions. For example, he considers the peasants' organization, Union Nacional de Agricultores y Ganaderos (UNAG) to have little political importance (claiming its lack of independence from the Frente Sandinista de Liberación Nacional [FSLN]) and thus does not bother to analyze its growth (to seventy thousand members) or its role in creating a strong cooperative movement. But by ignoring UNAG, Colburn cannot provide a reasonable explanation of why the Nicaraguan government was forced to cancel peasants' debts in 1983 or speed up the course of the agrarian reform, favoring individual land titling over collective property—both significant departures from earlier Sandinista policies.

Even more damaging, by not taking into account the role of popular participation in the Nicaraguan revolution, Colburn is eventually left with no explanation of why the alleged "disenchantment of the rural poor" has not translated into broad popular support for the counterrevolution (p. 120). Derogatory quotes reportedly made by workers and peasants against the Sandinistas are no substitute for *analysis* of the complex interrelationship between economics and politics in shaping the allegiances of the rural poor.

By limiting his analysis to 1979–82 and treating it as if it constituted one cohesive period,

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Colburn fails to capture the important changes continually taking place in Sandinista agrarian policy. As a result, the book sheds little light on how revolutionary regimes, through "trial and error," attempt to face the overwhelming "structural" problems of underdevelopment. Moreover, the author's "temporal focus" makes the book outdated, and this is compounded by the annoying use of the present tense to describe institutions and policies that subsequently changed.

In sum, Colburn provides neither a very scholarly account of the transformations in Nicaragua's agricultural sector nor a very useful analysis of the interaction of economics and politics in transitional societies.

CARMEN DIANA DEERE

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The Economic Ideal in British Government: Calculating Costs and Benefits in the 1970s.

By Phyllis Colvin (Dover, NH: Manchester University Press, 1985. 232p. \$30.00).

In an era when slogans like "efficiency" and "value-for-money" are fashionable, Phyllis Colvin's book stands as a salutary warning to those who would shape public policy according to these dogmas. Colvin's study focuses on two major issues, the entry of professional economists into the British bureaucracy and the impact of economic methodology on the making of public policy. The author's particular interest is in the use of cost-benefit analysis (CBA) in differing policy areas; her overall concern, to investigate how economic doctrine shapes administrative practice.

The major part of this work consists of an account of the entry and uneasy integration of professional economists into the Whitehall structure in the post-World War II era (chap. 4), in the assessment of vocational training (chap. 5) and in the health field (chap. 6). Based on numerous interviews and the painstaking study of diverse literature, these case histories offer insights not only into the way CBA was introduced at the macro-organizational level but also into the detailed application of the methodology itself. Indeed one of Colvin's objectives is to expose the strengths and weaknesses of economic analysis (as applied to public-policy issues) through refer-

ence to the intellectual roots and the sociology of knowledge that underpins it (see chaps. 2 and 3).

While the case histories highlight the differences that exist among different policy areas, they also reveal integrating themes concerning the operation of economists in British government and the application of economic analysis to policy problems. Thus economists struggled to legitimate their position in bureaucratic organizations, frequently making trade-offs to achieve influence. Further economic analysis (in the form of CBA or other applications) was used by other administrative groups as a weapon in the battle for control and influence between and within government departments. Consequently economists were often politicized or trimmed their ambitions to participate in the power play of the bureaucracy. As the author forcefully points out, these factors had serious implications both in the way questions were selected and defined and in the conduct of the analysis itself. For example, a search for manageable issues often led to a sanitization of problems, while the application of economic analysis to option appraisal and policy formation proceeded with some disregard for questions of policy implementation (see chap. 6).

Phyllis Colvin is aware of the perceived advantages of a technique like CBA, especially in a climate where resources are scarce. Such a technique is seen to bring certainty and control to an uncertain administrative world. It also appears resilient as a methodology and influential in conceptualizing policy problems. Yet, these advantages may come at a price seen at its most extreme in the spurious simplification of issues and a failure to conceptualize the full complexity of the policy process (see chap. 7).

There are minor points of weakness in this book. Strong on the detail of economic analysis and the impact of economic ideas in British public administration, it often fails to give full weight to the administrative culture of Whitehall and to the political pressures that operate there. Further, good as it is on the individual case histories, the comparative dimension of the study is not developed in detail. Yet these comments should not detract from an excellently researched administrative history of significant importance for students of British public policy. What is demonstrated clearly is that here there was little "analysis of

analysis" or any great appreciation of the political constraints operating in the policy sphere. The study also indicates that the very rigor developed to strengthen an academic discipline may lead to serious errors when applied to the more ill-defined arenas of the political world. In particular, the search to place a "value" on policy options remains one of the more intractable issues of applied public policy.

WILLIAM I. JENKINS

University of Kent

Elections and Democratization in Latin America, 1980-1985. Edited by Paul W. Drake and Eduardo Silva (San Diego: Center for Iberian and Latin American Studies, University of California, 1986. xi, 335p. No price listed, paper).

This useful volume consists of 14 essays on the impact of recent key elections on processes of liberalization and democratization in Latin America. The book is implicitly divided into two groups of chapters. The differences one finds between the two groups hinge on the authors' treatment of the role of the United States in these elections. The topic of the first few essays is really elections and U.S. foreign policy, and, inevitably, the demonology of U.S. foreign policy in Central America impregnates the argument.

The essays on El Salvador and Nicaragua, in particular, are long on debate and short on the analysis of election results, but despite these intricate detours, some of the contributors to this section have useful things to say. For example, Wayne Cornelius states the most important conclusion found in the book when he writes that "Mexico's experience shows that political reforms which serve mainly to create additional space for opposition parties within the political system do not really promote the democratization of the system. And the more competitive elections made possible by liberalized election laws do not even necessarily help to legitimate the government, so long as the basic authoritarian features of the system . . . remain intact" (pp. 137-38). John Booth concludes that the 1984 Nicaraguan election was "free and open" and that it was "neither sham nor fraud," but he admits to a "demonstration effect" intended for the election by the San-

dinista regime (pp. 58-60). Unfortunately, Terry Karl's discussion of recent Salvadoran elections is kept at a supranational level in which domestic forces are overdetermined by a foreign-imposed script. Having assumed that only a negotiated scheme of power sharing will put an end to the Salvadoran civil war, Karl deplores that this "electoralism" has blocked the formation of a democratic compromise (pp. 34-35).

The essays in the second section of the book are more akin to psephology, that is, to the kind of treatment one expects in analyses of electoral systems and voting behavior. Contributors to this section could rely more extensively on the traditional repertoire of the discipline because they were dealing with cases in which elections were the efficacious instruments of political transition. As a result, the section is replete with valuable insights into the indeterminacy of electoral contexts in situations of political transition. The essays by Marcelo Cavarozzi and by Manuel Mora y Araujo should be read by everyone interested in postauthoritarian politics in Argentina and particularly in the mechanics of how the Radical party put a winning coalition together. The contributions by Glaucio Soares and David Fleischer on Brazil, and Howard Handelman's and Charles Gillespie's essays on the constitutional referendum of 1980 and the party primaries of 1982 in Uruguay, respectively, show how the unintended and unanticipated consequences of these contests contributed to undermine the military's blueprint for the consolidation of a "protected democracy" in both countries. The essay by Juan Rial is probably the most successful attempt to explain the outcome of one Uruguayan election in relation to the infernally complicated system of the "simultaneous double vote."

Many factors stand behind the differences in the tone and tenor of the essays contained in this volume, as many as there are differences between Central and South America. This being the case, perhaps Drake and Silva should have been more ambitious and present their own conclusions on why Mexico and Central America remain particularly impervious to peaceful transition through elections.

ENRIQUE A. BALOYRA

University of Miami

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Local Government in the German Federal System. By Arthur B. Gunlicks (Durham, NC: Duke University Press, 1986. xii, 247p. \$42.50).

Municipal self-government has a long history in Germany and it is still of great interest and importance. Local authorities are the principal agents for the provision of many local services and contribute massively to the well-upholstered infrastructure of West German social and economic life. They sustain the dispersion and decentralization of political activity and influence, and fit without excessive friction into a federal pattern of government that has shielded West Germans from some of the grander illusions about the capacities of transient politicians that have beset more unitary societies. For these and many other reasons, German local government deserves more attention than it has had outside the Federal Republic. Gunlicks's study seeks to make good this neglect.

The main emphasis in the book is on providing a careful account of local government structures and of the chief legal and financial conditions under which the municipalities (*Gemeinden*) and counties (*Landkreise*) operate. The author starts with a historical survey from vom Stein down to 1949, followed by some remarks on the German administrative tradition with its preference for decentralization through a hierarchy of spatial units. There is a summary of the measures of territorial rationalization effected in the period 1968-78, followed by consideration of the legal framework for local authorities, the distribution of functions at local and *Land* level, and the financing of local government. The comments on the staffs employed at local level are fairly brief, but there is rather more on intergovernmental relations. Here Gunlicks draws perhaps too uncritically on a German literature devoted to "policy interlocking" (one possible translation of *Politikverflechtung*) that tends to overestimate the novelty of this phenomenon. There is, too, a chapter on local councillors, elections, and parties.

Overall this is an admirably thorough and accurate account of contemporary German local government. The author deserves special praise for coping so successfully with the complexities of German legal terminology, and there is no doubt that as a result he presents a

clear and fairly comprehensive picture of the institutional conditions of local administration. There is, however, a price to be paid for an approach so reminiscent of the style of that excellent administrative academy at Speyer from which the author has had generous support and guidance. This consists in some neglect of evaluation of results (What does the German taxpayer get for his money from local government?) and in some reluctance to get too deeply involved in an assessment of politics in local government.

It is, of course, true that analysis of the outputs of a branch of government is extremely difficult, particularly if an effort is made to go beyond general impressions. But I think that rather more should have been done to convey to the reader some indication of the service priorities of local authorities (e.g., their very major role in public transport), of the efficiency or otherwise of their service provision, and of relative costs. (I am sure German refuse collection is generally better done than in Britain, but what does it cost and how far does it all depend on legal compulsion to use standard bins or garbage cans?)

As to local politics, Gunlicks provides some basic facts and figures, but there is little life or color in his treatment of the elected member, his activity and his ambitions. Comments on particular towns or cities, on notable *Oberbürgermeister*, on party patronage and back scratching, or on the importance of local political success as a stage in the career of an upwardly mobile politician might have helped to give readers some idea of what local political life is like and of the varied characteristics it may reveal. As it is, the study is too reticent about these matters. The author, perhaps surprisingly for a U.S. political scientist, has stuck too closely to the formalism that is such a marked feature of German administrative law and science. Formal rules and powers are immensely important in the Federal Republic, but this is because the system bearers have internalized them and can live comfortably with them. Whoever writes about German institutions must certainly present the formal conditions carefully but needs also to push beyond that to the use made of them and their political effects.

In summary, Gunlicks has, with this reliable and detailed account of German local government, succeeded in filling a gap in

the English-language literature about the German system of government. But his work might have gained in interest and penetration if he had just been a shade more adventurous in the choice of issues presented and material used.

NEVIL JOHNSON

Oxford University

Influencing Mass Political Behavior; Elites and Political Subcultures in the Netherlands and Austria. By Joseph J. Houska (Berkeley: Institute of International Studies, University of California, 1985. xiv, 198p. \$11.50, paper).

The study of organizational linkages between elites and masses in societies with deep ideological, cultural, or ethnic divisions is an important part of contemporary comparative politics. This book seeks to contribute to this study via an examination of the twentieth-century politics of the Netherlands and Austria. In this context, the author sets himself three main goals: (1) "to illuminate some of the historical patterns of mass organizational growth and decline in terms of both mass participation and elite activities," (2) "to delineate the effects of the organizational connection on mass voting behavior," and (3) to "analyze how changes in behavior in organizations, either by elites or masses, affect long-established voting patterns" (pp. 2-3). The key units of analysis are *political subcultures*, defined as "segments of a national population . . . [with] . . . a common political orientation and organized into separate educational and communications systems and networks of public and leisure-time groups" (p. 3). What follows is largely descriptive and focuses primarily on Dutch and Austrian elite and mass political behavior in Catholic, Protestant, and Socialist subcultures. This is understandable for a work by a political scientist, but unfortunately, it goes over ground largely covered by others. However, two chapters—dealing with critical theoretical and empirical issues—stand out and could have been the kernel for a much stronger work.

In chapter 3, the author asks: Why are people in the Netherlands and Austria "segmented"? In essence, his answer is that sub-

culture elites (political entrepreneurs) form organizations that offer an array of economic, political, social, and symbolic rewards (public goods) to subculture members. However, three very important questions do not get enough attention. First, what objectives motivate subculture elites? Some mention is made of "offensive" and "defensive" goals (p. 15). The former involve obtaining political power in order to implement a policy agenda, the latter to protecting subculture strength and identity. Second, on what resources do elites draw when building subculture organizations? Occasional reference is made to church support for Catholic and Protestant elites and organizations, but the reader is not given a clear indication of how extensive or crucial such support has been. Third, do organizations segment a society or do organizations appear that simply reflect the latent divisions that already exist and thus formalize or reinforce these divisions? The author leans toward an organizations-segment-the-society explanation. For instance, he observes that if "most of an individual's acquaintances are made within subculturally sponsored organizations, most of his friends will turn out to be members of the same subculture" (p. 39). Could it not be the reverse? Perhaps most individuals join the same organizations because these are the ones joined by their family, friends, and coreligionists? Thus, an argument can be made for the reverse; that is, that in a segmented society, organizations appear that are based on its segments and that the crucial linchpin is the political entrepreneur, who recognizes the latent divisions and pitches his organizational efforts and political appeals accordingly.

The preceding discussion distracts one from the author's main theoretical contribution, namely, his development of the concepts of *education* and *encapsulation* as elite strategies designed to perpetuate subculture identity and political behavior. Subculture organizations actively seek to educate members in the "ideology" of the group and, at the same time, carry, articulate, and interpret its collective memory of important historical events, as well as organizing commemorations of these events. *Encapsulation* is defined as "a process in which elites isolate followers by attracting them to a broad network of subculturally linked organizations and thereby prevent member contact with competing organizations or even polit-

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ically neutral groups" (p. 36). What does this mean? The author does not directly say so, but, as noted, subculture organizations offer public goods that exclude nonsubculture members and cannot be obtained elsewhere. What do the masses give in return? They give their consistent political support. Thus, campaigns, elections, and voting become extensions of subculture identity and simultaneously contribute to education and encapsulation. This intriguing theoretical setup is followed by a description of subculture organizations in the Netherlands and Austria (pp. 41-56) and an exhaustive documentation of the political behavior connected with these organizations.

Chapter 7, however, raises another central question: Why have Dutch and Austrian subculture organizations and their political extensions experienced a major decline in the 1960s and 1970s? The author suggests several reasons, none of which is explored in full detail. They include: (1) generational changes in the elites and masses, (2) rising costs of organizational maintenance, (3) preemption of public-goods provision by the state, (4) economic trends, (5) the hierarchical or federated nature of interorganizational relations within a subculture, (6) intrasubculture conflicts among elites on political goals and strategy, (7) actual achievement of major subculture political goals, and (8) declining elite and mass perceptions of hostility from other subcultures. In large part, these reasons and their accompanying discussion do not answer but transpose the question of decline. For example, why and how did the state(s) preempt the provision of public goods that were once the primary domain of subculture organizations? Thus, while equally intriguing, this chapter is less satisfying than Chapter 3; but it does raise a number of provocative theoretical and empirical questions.

In sum, this book's contribution to the study of organizational linkages between elites and masses in deeply divided societies is uneven. Its strongest contribution is theoretical, both in the concepts that are developed and the questions that are raised. More attention to these theoretical questions could have made this a stronger work, but, nevertheless, the present effort is still worthwhile. On the other hand, except for its thorough documentation of the consistent political support given by subculture members to their political parties, it has

some difficulty developing empirical links between the theoretical constructs and the substance of recent Dutch and Austrian politics. However, to varying degrees, this is a problem with many of the works in this subfield and should not detract from this study's theoretical contribution.

LEE E. DUTTER

University of Illinois, Chicago

Hegemony and Culture: Politics and Religious Change among the Yoruba. By David D. Laitin (Chicago: University of Chicago Press, 1986. xiii, 252p. \$30.00, cloth; \$13.95, paper).

Some students of contemporary African politics and society have long been impressed by the persistence of ethnicity in shaping political views and allegiances. It seems that for many African societies, neither class nor ideology nor religion has displaced ethnicity as a primary focus of political identity. Intrigued by the relationship between religion and politics, the author, professor of political science at the University of California, San Diego, designed a study predicting that in certain circumstances religious identity among the Yoruba of Ile-Ife, Nigeria, would override ethnicity in shaping political views and behavior. To this end he compared the political views of Muslims and Anglicans in the town, but he discovered that there was no significant difference between the two groups. Indeed he shows in his study that there was no significant political difference that could be attributed to religion between Muslims and Christians among any Yoruba, including the Ibadan Yoruba of Adelabu's time.

Instead of abandoning his study, Laitin uses his negative results to take social science to task. What kind of social science is it, he seems to ask, that would have us believe that a religious cleavage among the Yoruba, or anyone, should have political consequences? And if no such consequences manifested themselves, perhaps there is something amiss with the state of contemporary social science.

Laitin divides contemporary social science into two traditions, one following Weber and the social-systems approach and the other following Bentham and what he calls "rational-

choice" paradigm. Both approaches he finds deficient in explaining the relation or absence of relations between religion and politics in his case study.

The social-system approach, best exemplified in the works of Clifford Geertz, suggests that societies tend to homeostatic equilibrium and implies that when a social system like the Yoruba's is penetrated by a religious cleavage, certain effects must appear in other subsystems, most notably in their politics. Similarly, from the perspective of the rational-choice model, best exemplified in the works of Abner Cohen, a religious cleavage should have generated opportunities for political entrepreneurs to exploit in the context of Yoruba and Nigerian politics. From the point of view of both approaches, that no significant political differences between Muslims and Christians appeared was anomalous. This leads Laitin to suggest that both models may be deficient and that a third way is in order.

Following the works of Antonio Gramsci, the Italian Marxist theoretician, Laitin suggests that a third approach, which he calls *hegemony* (p. 19), might better explain the relationship between religion and politics. The crux of his proposal is that the social system does not work automatically to produce linkages among subsystems or equilibria but that on the contrary, these exist only when they have been forged by elites controlling the state and political power. The reasons for the origin and persistence of ethnic identity among the Yoruba and their rejection of other points of reference, suggests the author, lie in the colonial past. It was British imperial rule that helped to preserve and foster ethnic identity, and it was this rule that passed the problem of ethnicity on to the modern Nigerian state. Once such identities became forged, they came to be viewed as fundamental both by the rulers and the ruled, while other foci were perceived as illegitimate or lacking credibility. This explains why religion was not a factor in Yoruba politics: it was superseded by ethnicity.

A brief review does justice neither to Laitin's theoretical arguments nor to many of his credible suggestions. It should be pointed out, however, that a negative result questioning the Weberian and the Benthamite traditions does not provide sufficient grounds to establish a new model based on the works of Gramsci. To that end, some positive results need to be

reported. It would seem that Laitin's next task is to analyze some representative cases where, in fact, elites did forge links between religion and politics.

ROBERT MELSON

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Working Class Politics in Crisis: Essays on Labour and the State. By Leo Panitch (New York: Schocken Books, 1986. xi, 243p. \$19.95).

British Labour is an integrative party, that is, one that implicitly seeks, as it has done ever since its formation, to harmonize differences and conflicts in British working-class politics in order to align the greatest number of voters behind its parliamentary-based version of social democracy. Rather than take its orientation from the class tensions in society, it has constructed its ideology and organization in a way that hampers its scope and, indeed, hampers the desire for radical change in Britain. This is the charge Panitch develops on the basis of historical deduction and as a case study in the practical problems of the struggle to revitalize a British Marxist tradition.

Nine essays spread over 15 years offer sedimentary layers for the political archaeologist, welcome as soundings in the intellectual history of the Left in Britain. Inevitably, some have become dated. The debate about corporatism has recently become jaded for lack of empirical studies, but here are useful pointers on how it might be renewed. Some read well as assessments of what was at stake in the Labour party as it emerged from its 1960s political brokerage or as the struggle for leadership developed down to its denouement in 1981. Others, however, read more as reviews of the literature or as revisitations of hallowed themes like workers' control.

The lead essay (1985) bears astringent comparison with R. H. Tawney's essay, "British Socialism Today" (in *The Radical Tradition* [London: George Allen and Unwin Ltd., 1964]) in its analysis of how Labour responded to the mid-1970s economic crisis and to 1980s market populism in the hands of the "new liberal" Right. But where Tawney showed how far socialism had come, Panitch asks whether

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socialism is still possible and, if so, under what banner. It can, he argues, be recovered only if the party rediscovers its necessary working-class orientation. Otherwise the project will fail, as it did in 1976-79, not only in Britain but in Sweden and Germany and, indeed, with the failure of *autogestion*, in France.

Structures and the ideologies they embody are to blame, especially where they rest on essentially corporatist foundations, or on fallible assumptions about changes in the class composition of advanced societies, as did the Euro-Communists' dreams of building on supposedly transformed class alignments, or the Greens' hopes to do the same on cross-class issues. Hence, if Gaitskillism is not to reemerge under a future Labour government, Panitch claims (in a perceptive but tendentious account of events since 1979) that a program must emerge in tune with the working class's experience of increased deprivation at the hands of an ever more desperate capitalism.

The elements of faith rather than experience in this emerge clearly in the discussion of whether trades unions remain a central problem or are now to be seen as part of the proposed solution. The prescription appears to be a reissue of the 1970s "alternative strategy" with industrial democracy—as recommended in the Bullock Report—providing the necessary shift in the industrial balance of power. As befits an economy in deep structural decay, Panitch is cautious about the more ameliorative aspects of "market socialism," the current Fabian nostrum, as a diversion from the main programmatic task.

All the essays address in one way or another the central question of the forces and methods needed to transform the modern state. Yet there is a want of detail about, or even sensitivity to, what the state actually is as distinct from the political parties that compete to control it. The sort of corporatism that enmeshed the Labour government in the late 1960s and mid 1970s is unlikely to be revived in the same form, yet the renaissance market populism, which has, to a considerable extent, undermined the central institutions of capital and labor, is not here related to changes that have already taken place in the state since the first of these pieces were written. Consequently, just as divisions inside the trade-union movement are underplayed or papered over with talk of endemic crisis, so the potential for new con-

nections between them and the changing state is not explored.

It is hard to see, in current patterns of trade-union behavior, anything but a confused response to deep changes in the pattern of employment, work, and class affinity; hard to see that Panitch's argument rests on any dynamic other than the leadership of a Left vanguard somehow reborn from the ruins of 1981, supported by a popular-front range of allies. The desired strategy may indeed fit with this line of historical analysis, but the fact that other interpretations of recent history do not lead to the same end suggests that very substantial problems remain.

The first concerns the mismatch between balance-of-power struggles in the Labour party throughout the period in question and actual control of the machinery of government—one essential lesson from the failure of the Left's alternative strategy. It is hard to see, on a basis of what has happened, how any program of the economic severity implied here could be implemented, given the relative autonomy of the state and the predictable resistances in civil society. Whatever else one may conclude from the story of wages policy since the mid 1960s, the outcome has not been beneficial to the lower paid but to the well organized and powerful. These historical realities have surely had a long effect in shaping the Labour party's ideology. The second problem lies in the nature of a challenge rather different from that of revitalizing democratic socialism in Britain. Unless new currents in the behavior of trades unions like the electricians and plumbers and the engineering workers are to be taken as a complete aberration, market populism in other hands than the present administration's may turn out to have a far longer life span than is envisaged here. Whether Neil Kinnock's Labour party has yet faced this conservative challenge remains unclear.

ROBERT KEITH MIDDLEMAS

University of Sussex

The Future of Social Democracy: Problems and Prospects of Social Democratic Parties in Western Europe. Edited by William Paterson and Alastair Thomas (New York: Oxford University Press, 1986. xiv, 308p. \$39.95, cloth; \$19.95, paper).

This symposium is a most welcome addition to a growing literature comparing European parties of like hue, including Zig Layton-Henry's *Conservative Politics in Western Europe*, Roger Morgan and Stefano Silvestri's *Moderates and Conservatives in Western Europe*, Karl-Werner Brand's *Neue soziale Bewegungen in Westeuropa und den USA*, Emil Kirchner's forthcoming book on liberal parties, and the authors' own *Social Democratic Parties in Western Europe* of a decade ago. It compares favorably with all but Europe because it is more tightly focused on the special problems of Social Democratic parties in the last decade and their immediate prospects. Nevertheless, the coverage is far from uniform and most of the fine country articles are more concerned with idiosyncratic political difficulties and electoral vicissitudes than they are with the common problems of applying the Social Democratic wisdom of Anthony Crosland's *The Future of Socialism* (1956) amidst the social changes and economic crises of the seventies and eighties. The perplexities of passing from "the wave of the future" to "the future that does not work," as the editors have put it with unintended double entendre (p. 7), are explored expertly in chapters on the re-emergence of mass unemployment (Douglas Webber), Scandinavian social democracy (Thomas), and the reluctant adoption of European integration by Social Democratic parties (Kevin Featherstone)—and touched upon in all the other chapters. There is the familiar scenario of successful social movements whose attractive agenda of social remedies is widely adopted, even by their enemies, at the precise moment that the changing social and economic problems have begun to outrun these remedies. Economic growth as a precondition of full employment has slowed down or come to a grinding halt. The controls of party government in general, and of Keynesian demand management in particular are working less and less well. Social Democratic political decision makers are paralyzed by doubts about the appropriateness of their goals, divided within

their parties, and stymied by never-ending fiscal crises. A military Keynesianism of warfare-welfare states is hardly the answer to the breakdown of the Social Democratic consensus, however attractive it may seem to conservative opinion today, when even working-class voters have lost faith in the ability of Social Democratic governments to provide more jobs.

The editors and contributors are moving over familiar ground when they describe the diverging paths attempted by different Social Democratic governments to escape the predicaments of this last decade. Frustrated by their inability to cope with the post-oil shock economies and profoundly changed partisan clienteles, they have either turned right or left or simply ignored the obsolescence of their standard remedial approaches. Some tried to follow more than one of these paths, at least sequentially, or were faced with fratricidal disension among Left, Right, and centrist factions of their parties. Turning to the right (James Callaghan, Helmut Schmidt) demanded discipline and austerity, emphasizing corporatist relations and incomes policy, and it often aggravated internal divisions and the problems of solidarity between the trade unions and the party. Staying the course with the same ineffective instruments of policy invited a disastrous demonstration of the governments' incompetence in the face of changing circumstances. Turning left (Mitterrand's *projet socialiste*, Labour after 1979, or the Danish party after 1982) might please the party activists but was likely to be resoundingly rejected by the electorate and might even lead to a secessionist development, as in Great Britain. Faced with these agonizing choices, the great Social Democratic parties of Western Europe have also begun to revise their ideological basis, beginning with the 1959 Bad Godesberg Program of the West German Social Democratic Party which stood at the starting point of this Social Democratic era. For the Right, this has meant challenging centralization and bureaucracy and experimenting with market economics. Codetermination, *autogestion*, and pension-fund schemes all have helped to legitimize social democracy in the eyes of all but the most left-wing skeptics, while on the other hand pacifist and anti-nuclear issues have tended to undermine the authority of the right-wing and centrist leader-

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ship. Environmental and life-style concerns among a far more educated and less docile membership, feminism, and increasingly independent and volatile trade unions further contributed to the softening of the ideological consensus behind social democracy.

This book plausibly explains the diversity of positions and policies of Social Democratic parties and regimes. It is less forthcoming in showing the long-time erosion of the intense sense of solidarity and social cohesion that used to hold the old labor movements together long before 1945. The disillusionment of the war and of the "end of ideology," in a manner of speaking, were only ratified by programs like the one of Bad Godesberg. The concept of the "catch-all party" left of center took on an ever more heterogeneous and fissiparous reality with the social changes of the sixties and seventies. The progressive breaking down of the old *Arbeiterhochburgen* noted from election to election, and similar signs of the disintegration of the social bases of the Right has spelled increasing dealignment and volatility in most of the party systems under discussion. Except in cases like Austria or Sweden where party-union solidarity is still high or where Right-Left polarization helps to keep the Left united, the future of social democracy in Western Europe may be as bleak as the future of its oldtime social and economic remedies, bleaker perhaps than this book would lead us to expect.

PETER H. MERKL

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Saudi Arabia: The Ceaseless Quest for Security. By Nadav Safran (Cambridge: Harvard University Press, 1985. viii, 524p. \$25.00).

Saudi Arabia's ceaseless quest for security, as Nadav Safran emphasizes, is explained by certain geopolitical compulsions. Safran has a clear perception of the geographical or strategic factors but does not have as clear an appreciation of the political factors. The Soviet Union's political and military presence in surrounding areas of Ethiopia, Syria, and South Yemen, their incursion into Afghanistan, which is so close to the Straits of Hormuz, and the emergence of a hostile Islamic revolutionary regime in Iran have

obviously made the Saudi rulers extremely apprehensive about their security. Thus, the Saudis have offered a quid pro quo relationship to the United States, in which they are prepared to provide an uninterrupted flow of oil and limited Saudi cooperation with some of the strategic objectives of U.S. policy in the Middle East in return for U.S. military assistance involving the sale of some of the most sophisticated U.S. armor and technical assistance.

What dismays Safran is that given the mutuality of these interests between the United States and the Saudis, why have the Saudis followed such a zigzag course in their policies, "first tilting toward the United States then leaning toward neutralism; calling on American AWACs aircraft and personnel shortly after the outbreak of the Gulf war and then using the first opportunity to put some distance between themselves and the United States; making some attempts to isolate the Syrians and, at the first signs of failure, seeking to appease them again" (p. 431).

To a perceptive and shrewd observer of Middle East politics like Safran, it should be apparent that the political costs for the Saudis of too close an alignment with the United States, which is so committed towards supporting the fundamental interests of Israel, are prohibitive. Safran is aware of these factors but does not emphasize them, with the result that his conclusion is that U.S. long-term interests lie in a disengagement of its vital interests "from the policy and fate of the Kingdom" (p. 460). Kissinger, whose political judgement was also influenced by historical insights, displayed a greater understanding of the Saudi policy when he wrote, "The ambiguity of Saudi policy is imposed by events, not by preference" (*Years of Upheaval*, p. 659).

The book should be commended for the painstaking research that is displayed, but there are hardly any theoretical leaps flowing from such a vast array of historical and political data. Safran seems to think that the earlier Saudi client-patron relationship with the United States has been transformed into no more than a complex relationship of interdependence. But the data that he provides of growing U.S. military presence both in the form of weapons and military personnel indicates that the relationship is one of military and political dependence. Partly as a result of

this military dependence, the Saudis have been pressured into becoming a part of the strategic consensus that the United States has tried to promote among moderate Arab and Muslim states under its leadership. As a result of this consensus, the United States has tried on the one hand to moderate Israeli hostility towards Arab states like Saudi Arabia and Jordan and on the other to ensure that Saudi commitments to Arab nationalism and Islamic solidarity do not lead to any military action against Israel. It is when one considers the subtle means of influence that the United States has acquired over a country like Saudi Arabia through their military assistance that one begins to discern the full implications of military dependency.

Safran has also not explored fully the Islamic dimension's potential for security. Saudi Arabia is the home of Islam's holiest places. Therefore, its security in the eyes of the Muslims throughout the world is not the security of its ruling family but the security of its holy places, and for this, Muslim regimes would cooperate fully. Some of the Pakistani officers stationed in Saudi Arabia told me that they would fight for Saudi Arabia just as fervently as they would for their own country. However, there is no question that this scholarly work is replete with rich historical and empirical data that may generate several hypotheses and theories for students of Middle East politics.

KHALID BIN SAYEED

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The Scope of State Power in China. Edited by Stuart R. Schram (New York: St. Martin's Press, 1985. xxiv, 381p. \$35.00).

This anthology of essays by 12 eminent scholars of Chinese studies stems from two conferences sponsored by the European Science Foundation at Royaumont, near Paris, in 1980 and at Schloss Reisenburg, near Ulm, in 1981. Papers presented at a third conference at the Bellagio Study and Conference Center in 1983 under the auspices of the same organization are forthcoming in a second volume. *The Scope of State Power in China*, based on the first two conferences, examines the organization and development of various state institutions, the function of the state in shaping

moral values, and the economic role of the state. Although the essays share the common theme of the state's role in Chinese society, Stuart Schram informs us that the book does not purport to offer "a comprehensive survey of the problem of the state in China, from the Qin dynasty to the present day" (p. x). This volume does, however, emphasize the importance of studying China's past to understand the continuity of traditional and contemporary China or, in some cases, to assess the existence of a linkage between the past and the present. Karl Büniger's foreword, "The Chinese State between Yesterday and Tomorrow," outlines the significance of comparative studies in time (the past and present of China) and space (traditional China and Europe). Comparison with Europe, Büniger contends, should enhance scholars' understanding of the state in traditional China and make them less critical of Chinese institutions which, in some cases, preceded developments in European politics. Jacques Gernet reinforces the utility of the comparative approach in his introductory overview of the essays.

The reader who has grown weary of conference proceedings offering a glimpse of a topic in book form should not prejudge *The Scope of State Power in China*. It presents 10 carefully researched articles in two parts—"Patterns of Rule" and "The Economic Role of the State." All of the articles, with the exception of those by Schram, Billeter, and von Senger emphasize imperial China with comparative references to the Communist era. The three exceptions have a reverse focus. In the first part, Léon Vandermeersch outlines the origins of legal provisions in imperial China and examines the role of the emperor as the ruling authority. Today, the Chinese Communist party has replaced the emperor. State organs elaborate laws, but these organs, Vandermeersch contends, are subordinate to the Party. Tilemann Grimm, in his analysis of despotism in the Ming Dynasty, maintains that a state requires more than institutional structures and territorial boundaries. It is "governed by fixed legal norms, which bind those who control it and those who depend on it alike" (p. 49). He questions whether late imperial China fits this definition of a state. Marianne Bastid finds that financial institutions not only were an essential part of the organization and role of central state power in the late Qing dynasty

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but also have affected the "evolution of Chinese political institutions until the present day" (p. 79).

Schram's foremost contribution in his essay on state decentralization is a thorough presentation on the changing conception of *yi yuan hua* (integration) of leadership in the Communist era. He concludes that although changes may be forthcoming, the Party still prevails over the state, and the lower levels remain subordinate to the center. Jean-François Billeter studies the status system in the Maoist era and its declining significance in the Dengist period. He is skeptical of the amount of change the Party will allow. Harro von Senger reveals that Party norms dominate in the relationship between Party and state and finds no "parallel" in Chinese history.

The articles in the second part focus on the economic role of the state. Based on Yunmeng texts written on bamboo strips in the Qin dynasty, Anthony Hulswé finds that the Qin state influenced the economy via agriculture, trade, industry, and taxation. Michael Loewe asserts that economic decisions of the state in the Western Han dynasty established precedents for "later governments" (p. 267). Paolo Santangelo's essay discusses the fiscal and political nature of textile factories of Suzhou in the Ming and Qing dynasties. In his analysis of the state and hydraulics in imperial China, Pierre-Etienne Will examines variations in central, regional, and local intervention in the hydraulic infrastructure.

Each essay presents an interesting dimension of the expansive "scope" of the state of China. This positive aspect of the book, however, simultaneously represents a shortcoming. With the wide variation on the focus of the state, the volume needs a concluding chapter to weave the authors' conceptions of *state* and summarize the changing role of the state. Perhaps such a chapter is forthcoming in the second volume.

Another omission from the book is a discussion of state power in the nationalist period following dynastic rule. This addition would permit an examination of party/state relations under the Guomindang party and fill a void in the comparisons across time.

Notwithstanding the shortcomings, *The Scope of State Power in China* provides a wealth of information based on primary sources. The book is particularly useful for

scholars of comparative politics studying the role of the state in a one-party system. Moreover, it is necessary reading for specialists in Chinese studies, who will particularly appreciate the authors' use of pinyin romanization and Chinese characters for special terms and bibliographic citations.

CHERYL L. BROWN

University of North Carolina, Charlotte

Industry, the State, and Public Policy in Mexico. By Dale Story (Austin: University of Texas Press, 1986. xii, 275p. \$30.00).

In *Industry, the State, and Public Policy in Mexico*, Dale Story seeks to assess the role of industry in the formulation and implementation of public policy in Mexico and, more broadly, to assess the place of business in Mexico's authoritarian political system.

Story draws on economic data concerning production, investment, and public-sector expenditures to assess the relative contributions of the state and private-domestic and foreign capital to the pattern of industrial growth in Mexico during this century. He reports on a mail survey he conducted with Mexican and Venezuelan industrialists in 1980 to ascertain the political role and ideology of Mexican industrialists and their organized interest groups. He also provides case studies of recent governmental decisions, an extended discussion of Mexico's decision not to join the General Agreement on Tariffs and Trade (GATT) in 1980 and briefer accounts of the debate over petroleum policy in the late 1970s, the formulation of comprehensive development plans in 1979-80, and the nationalization of the banks in 1982. This weaving together of economic, attitudinal, and decision case-study materials and the effort to establish a comparative context are a sophisticated approach to an important topic.

Story's argument has two main thrusts: first, that private domestic entrepreneurs have played a significant role in national development and second, that "the Mexican industrial sector has considerable independent influence on many policy decisions" (p. 10). Story sees these conclusions as disproving widely accepted "truths" about Mexico. With both,

unfortunately, he is sparring against straw men.

The first is asserted "contrary to many conclusions of a weak national industrial sector" (p. 75), but he cites no one in this indictment. The book marshalls a great deal of useful information bearing on the roles of private domestic entrepreneurs, foreign capital, and the state in Mexico's economic development, but because Story insists on seeing them simply as competitive "rivals" (p. 32), he misses the opportunity to deepen our understanding of how complexly they relate to one another—sometimes in conflict, sometimes in cooperation.

His second argument is aimed at "the common interpretation of Mexico as an authoritarian regime in which industry, along with other political actors, is dominated by the state" (p. 195). Here, Story does have a target: an influential article by Susan Kaufman Purcell ("Decision-Making in an Authoritarian Regime," *World Politics* 26 [1973]: 28-54), but he fails to mention that even she had contended that "the business interests, or 'private sector,' undoubtedly are the most independent of all the [interest] groups" (p. 33). Moreover, the book seems innocent of even the suggestion that an authoritarian state might act to further the interests of capital. A root difficulty here is the book's lack of any conception of the state that situates it in relation to social classes. He sees the state only as a free-floating collection of roles and incumbents. In a peculiar reversal of contemporary theorizing about the state, he speaks recurrently of the autonomy of business interests from the state rather than of any relative autonomy of the state from dominant social classes.

There is a puzzle about Mexico that needs further unravelling. The business community has been formally excluded from the regime of the Partido Revolucionario Institucional (PRI), and yet it has been the primary beneficiary of economic growth in Mexico over the decades since the revolution. It helps only a little to insist that the business community has substantial independence. We need to know how private Mexican capital has exerted power or influence over the state, we need to understand the limits to this, and we need to consider the interests that the state itself may have in promoting private capital formation and the strategies by which it seeks to do so—not all of

which may be immediately to the liking of the business community. Story's case studies, particularly the one on the GATT decision, provide insight into all of these questions, even though they are not the focus of his discussions.

There is an impressive amount of useful information and data analysis in this book, but due to the way Story has framed its central questions, *Industry, the State, and Public Policy in Mexico* slides by the central issues it seeks to address.

DOUGLAS C. BENNETT

Temple University

Ballots and Trade Union Democracy. By Roger Undy and Roderick Martin (Oxford: Basil Blackwell, 1984. 240p. \$22.50).

How, whether, and for what purposes unions conduct ballots of their members became a political football in Britain in the late 1960s and has remained that ever since. Legislation covering union ballots was passed under the Conservative government in 1980, 1982, and 1984. Entirely apart from legislation, in some unions changes in balloting have been introduced by reformers of the Right or the Left.

Undy and Martin first outline the arguments made by the Conservatives for the promotion of the postal ballot in union elections and for collective bargaining decisions. They then devote most of their book to a description of actual practices in as many as 103 unions affiliated with the Trades Union Congress and to the actual effects of methods of balloting (or not balloting at all) through the presentation of 13 case studies. They attempt to answer such questions as, How much does the postal ballot increase participation in decision making, for example over voting at union meetings? Are officials elected by postal ballot less left-wing, and less militant? Does the postal ballot result in a greater acceptance of managements' offers in collective bargaining? Are postal ballots more likely to represent the views of the general membership? The Conservative party would of course argue the affirmative to these questions, believing that the postal ballot would clip the wings of militant, left-wing minorities. The authors point out that the

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Conservatives have also used the postal-ballot issue as an entering wedge for legislation limiting industrial action (sympathy strikes, picketing) and union political levies (for the Labour party).

Discussion of union democracy often takes place without an appreciation of the practical problems that distinguish voting in very large, dispersed organizations from that in governmental elections. Undy and Martin show that what might be thought to be the relatively simple matters of casting, collecting, and counting votes are complicated by such problems as vast, dispersed memberships, and membership turnover or geographic mobility (which in effect disenfranchise large numbers of members whose addresses are not available). Where collective bargaining is concerned, the constituencies may cover many unions and many large corporations (as in bargaining with a national employers' association), and postal ballots would obviously be time-consuming, and provide less opportunity for weighing alternatives than a conference of representatives.

From their case studies, the authors conclude that there is no evidence of strong or general effects of methods of balloting (or of not balloting at all) on union political processes. Postal ballots, in particular, have no general effect on the political complexion or industrial militancy of those elected. A change to postal ballots has indeed often resulted in a change in leadership, sometimes favoring right-wingers and sometimes left-wingers. In some cases the Left rather than the Right has favored postal ballots and—more often than the Right—has favored election rather than

appointment and periodic elections rather than permanent posts. Nor does the extent of factional organization seem to be related to the particular method of balloting. On the other hand, "unions with factional organization are potentially more open to political change from the introduction of postal ballots" (p. 116), and "the introduction of postal ballots is likely to reduce the effectiveness of any factions based on a small number of geographically concentrated groups of activists, as exist in some unions using branch ballots" (p. 114). Participation in elections through postal ballots is often disappointingly low, usually far lower than place-of-work balloting. There seem to be few arguments against place-of-work balloting where it is feasible and where the integrity of the process can be assured.

The authors close with a broad-ranging discussion of union democracy, going far beyond the limited focus of most of this book. While I disagree with some of their judgments, this is an important contribution to the literature on union democracy. Some of the authors' assumptions, for example, that "trade union leaders are drawn from the moral activists" (p. 188), may be less warranted for the United States; but then, of course, they are writing about Britain. Most of the practical, unavoidable problems of union democracy inherent in the logistics of balloting in very large organizations should be considered by social scientists. *Ballots and Trade Union Democracy* can provide a worthwhile, if sobering, experience in this respect.

J. DAVID EDELSTEIN

Syracuse University

INTERNATIONAL RELATIONS

The Foreign Policy of New States. By Peter Calvert (New York: St. Martin's Press, 1986. viii, 216p. \$29.00).

Despite the growing importance of the Third World, systematic treatments of the foreign policy of poor nations are rare and the determinants of their policy choices remain elusive.

While the title of this thin volume suggests an attempt to fill the void, its approach will make it more appealing to the beginning student than to more advanced analysts.

Chapter 1 ("The Illusion of Foreign Policy") emphasizes the limitations of generalization in foreign-policy studies. Its too brief (three pages) exposition and critique of Allison's

three models, Rosenau's pretheory, and Brecher's framework concludes with the dishearteningly vacuous lesson that "The student of foreign policy must therefore stand back from the process of policy-making and seek to understand it in his own terms" (p. 10). He then undermines the focus of the book by including among the "new states" not only countries decolonized since World War II, but also the European and Middle Eastern states created at the end of World War I and the century-old nations of Latin America. It is no wonder that he finds it difficult to generalize across a set defined less by a common element of newness—or even weakness or size—than as a residual category that excludes only the superpowers and the core European powers of the nineteenth century.

Calvert pulls from this a dubious and unsupported assertion that "foreign policy is dominated almost exclusively by internal considerations" (p. 22) and a methodological commitment "first to understand the nature of the politics of the new states" (p. 20), which together provide direction, but not focus, to Chapters 3-6.

He first digresses in Chapter 2 ("The Rules of the Game") to rehearse the classical concepts of state, sovereignty, and legitimacy, and to introduce nonstate actors, diplomatic norms, conceptions of international law, and the role of treaties and alliances. Those encountering these ideas for the first time will find the discussion clear and agreeably leavened with both historical and contemporary examples.

Chapter 3 ("The Domestic Environment") opens the comparative analysis with a superficial treatment of domestic factors including governmental structures, the armed forces, religion, public opinion, and economic structures. Surprisingly, there is no mention of many factors that make poor states distinctive, such as the penetration of external actors or the character of the state and its roots in social and economic structures.

Chapter 4 ("How Foreign Policy Is Made") is more attentive to differences between "new" states and older powers, especially in the shortcomings in personnel, resources, records, and organization common to the foreign service of new states. The resulting policy is held to be reactive, lacking in continuity, and influenced by "non-material factors such as pride, honour or dignity." Calvert concludes that the

rational-actor model's view that foreign policy "should be designed in order to ensure the best interests of the state concerned . . . is in no way an adequate explanation" (p. 95). He does not proffer an alternative framework, however, though he emphasizes the contribution of the organizational-process model and acknowledges through examples the influence of the head of state and the military. Still, this is as close as the volume comes to generalization or to a theoretical statement.

Chapter 5 ("Capabilities") is loosely organized around a list of components of power: population size, territorial extent, strategic location, communication systems, and economic resources. The concepts themselves are elementary, but the maddeningly irrelevant digressions that dominate the text produce occasionally engaging nuggets like the observation that "every year more people are kicked to death by donkeys than are killed in air crashes" (p. 108). Chapter 6 ("Limitations") discusses the implications of military weakness, social cleavages, and the legacy of decolonization in a similar way.

The concluding chapter ("Playing the Game") maintains the barrage of examples and historical briefs, including the anecdote crediting a 1963 Ecuadorian coup to the actions of the president who, at the airport to greet a foreign dignitary, "was overcome by alcohol and fell flat on his face on the tarmac" (p. 155).

In sum, this volume is a readable, loosely argued, fact-filled introduction, which may appeal to the newcomer to foreign-policy studies. It offers little to those with a theoretical bent, however, and contains no systematic statement on the foreign policy of new states.

BRUCE E. MOON

Northwestern University

Towards a Normative Theory of International Relations. Mervyn Frost (Cambridge: Cambridge University Press, 1986. ix, 241p. \$37.50).

Frost's principal purpose is to show that normative theory in international relations is not merely possible but necessary to an adequate understanding of the unavoidably moral issues of international politics. The first two chapters

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are devoted to criticism of positivist social science, radically skeptical value noncognitivism, and realism, which he argues are responsible for the dearth of normative international theory.

These chapters are quite weak. The critique of positivism amounts to little more than a good but familiar summary of the *Verstehen* approach and a very brief, allusive, and wholly inadequate account of "critical theory." The alleged "positivist bias" in the study of international relations is never demonstrated; in fact, Frost quotes not a single passage from any writer, major or minor. The sources he cites for the "scientific approach" to international relations are *all* more than a dozen years old and average more than 20 years, leading one to wonder whether he is beating a dead horse. Even more serious questions are raised by the inclusion, among those with a positivist bias against normative theory, of Stanley Hoffman, whose *Duties Beyond Borders* is an important work in normative international theory. The discussions of moral skepticism and realism in Chapter 2, however, although not especially original, are competent and may be useful to some readers.

The real interest and value of the book lie in Frost's ambitious and interesting efforts to do normative international theory in the remaining four chapters. He argues, persuasively, that international relations is a settled, rule-governed social practice, with its own, state-centered "domain of discourse" (Chap. 3) and a "settled body of norms" (p. 120) that in their simplest summary hold that "the preservation of a system of sovereign states is the primary good" (p. 128). Any moral theory of the contemporary practice of international relations, therefore, must provide an answer to the question, "What is the best justification for the preservation of a system of sovereign states?" (p. 128).

Perhaps the best section of the book (pp. 128-56) is devoted to a strong attack on familiar answers to this question, resting on considerations of order, utility, and rights. Here Frost gives close attention to the work of important recent writers, especially Bull and Walzer. Although I do not find all of his arguments convincing, they are on target, powerful, and well worth careful scrutiny.

The most original aspect of the book is Frost's largely Hegelian theory of the state and

the society of states. This "constitutive theory of individuality" (Chap. 5) holds that individuals simultaneously constitute both themselves, as autonomous, rights-bearing moral persons and their social institutions by their participation in mutually valuing relationships in the family, civil society, and, most importantly, and most perfectly, the state. International relations and the norms of the state system are morally justified because they protect the state and the constitutive moral relations that the state alone makes fully possible.

This theory is certainly provocative, but Frost's presentation is almost fatally brief and sketchy. For example, he devotes a mere two pages (183-84) to claiming—not even seriously trying to show—that his is a good background theory for the settled norms of international relations he earlier identified (pp. 128-28). In contrast, his discussion of the family runs three pages (168-71). The one sustained application (Chap. 6) does not even deal with any of the settled norms but rather considers the notoriously difficult and relatively peripheral issue of unconventional violence. At best, Frost gives us an interesting but largely undeveloped idea.

Space precludes any serious attempt at evaluating the theory, but let me at least list two problems, one general and one specific. First, Frost simply assumes that there must be a single moral theory that justifies the settled contemporary norms of international relations. It is quite possible, however, that this set of norms represents an uneasy compromise, in which case his criticisms of Walzer in particular lose much of their force. Or many of the norms may be politically or prudentially, rather than morally, justified, in which case his entire project is called into question. Second, for the specifics of the theory, it seems on its face incompatible with the standard understanding of human rights as rights one has simply because one is a human being. For example, the International Human Rights Covenants state that human rights "derive from the inherent dignity of the human person," not from constitutive social relations. The difficulty of other theories in accounting for the norm of protecting human rights is used by Frost as a major argument against them, so this would seem to be a very serious problem.

In what increasingly seems to be a world of uniformly positive reviews in U.S. political

science journals, I hope that these criticisms will not be taken wrongly. Whatever its problems, *Towards a Normative Theory of International Relations* is a book from which I learned some important things about even familiar material and a book that forced me to think seriously and critically about important issues. I recommend it to anyone interested in the problems and possibilities of normative theory in international relations.

JACK DONNELLY

University of North Carolina, Chapel Hill

The Logic of Surprise in International Conflict. By Alex Roberto Hybel (Lexington, MA: Lexington Books, 1986. viii, 179p. \$22.00).

This is a finely crafted study of surprise attack. The author contends, correctly, that most seminal works on the subject, such as Roberta Wohlstetter's classic on the Japanese attack on Pearl Harbor, have focused on the victim. He proposes a framework to analyze surprise with emphasis on the aggressor by examining four case studies: the Nazi invasion of the Soviet Union in June, 1941; the Japanese attack on Pearl Harbor six months later; the Cuban missile crisis of 1962; and the Egyptian-Syrian attack on Israel in 1973.

The author attempts to answer three questions about these four cases: first, Why did the aggressor opt for surprise? second, Why did he choose one strategy of surprise over another? and lastly, To what extent did his actions contribute to the achievement of surprise?

On the first question, the author suggests that an aggressor's resort to surprise is a function of the value of the interests at stake. For Yamamoto, for example, it was crucial to cripple the United States at Pearl Harbor in order to increase Japan's prospects for success in Southeast Asia. Anwar Sadat in 1973 hoped to revitalize Egypt's prestige in the world by launching a successful attack on Israel. Hitler believed that a successful Operation Barbarossa would save Germany from a two-front war; and finally, Khrushchev was convinced that the installation of Soviet missiles in Cuba would tip the strategic balance in the Soviet Union's favor.

As a corollary to this central theme, the

author postulates that an aggressor is likely to resort to surprise if his most valued interests are threatened by an adversary perceived as stronger than himself and if the attacker's freedom of choice is not severely curtailed by domestic or international pressures. Japan's attack on Pearl Harbor is again cited as a case in point.

In his efforts to find answers to his second and third questions, the author develops 12 further propositions, each of which assumes one basic characteristic: the aggressor's effectiveness in manipulating his victim's sense of vulnerability. Yamamoto, for example, manipulated Washington's anxiety by successfully concealing his specific target even though the U.S. leaders expected Japan to attack somewhere. Khrushchev in 1962 took numerous precautions to disguise the deployment of Soviet missiles. Egypt and Syria in 1973 manipulated Israel so successfully that Eliahu Zeira, her director of military intelligence, actually predicted that Israel would not be attacked. Hence, in the author's view, the shortest and most reliable path to surprise by a potential aggressor is to do what his intended victim is convinced he will not do.

An enormous amount of thought has gone into this valuable book. The author does not engage in facile generalizations and guards against them with dozens of nuances and shadings. The quality of his empirical research is impeccable. Yet his conclusions seem to substantiate what commonsense already tells us. And, some of the deeper questions about surprise attack seem to elude him.

Why, for example, did all the aggressors considered by the author expect a quick and easy victory and why did ultimate victory elude them in the end? Hitler and Yamamoto, for example, both came to an ignominious end in 1945, Khrushchev had to withdraw his missiles from Cuba in 1962, and the Yom Kippur War of 1973 ended in neither victory nor defeat.

I would suggest that the author's conceptualization of surprise attack may be incomplete. An aggressor's irrationality may well be a critical factor, indeed at times so crucial that psychology may be more helpful in analysis than strategic thought. Operation Barbarossa, one of the author's case studies, may provide a clue. "Since I struggled through to this decision" (to invade the Soviet Union),

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Hitler wrote in a revealing letter to Mussolini on June 21, 1941, a few hours before German troops poured into Russia, "I again feel spiritually free. . . . The partnership with the Soviet Union . . . seemed to me to be a break with my whole origin. I am happy now to be relieved of these mental agonies" (*The Rise and Fall of the Third Reich* [Greenwich, CT: Fawcett, 1960], 1114-15). In other words, a bottomless and irrational hatred was apparently responsible for Hitler's utterly self-destructive decision to invade. And Stalin, that least trusting and most cunning and devious of men, had apparently placed his trust in the rationality of Hitler, the least rational of men.

The author is to be commended for focusing our attention on the aggressor rather than the victim. This, in and of itself, is a valuable addition to the literature on surprise attack. Yet, the deeper questions—Why the aggressor's expectation of quick victory? and Why his ultimate frustration or defeat?—need to be asked and answered.

JOHN G. STOESSINGER

Trinity University

The Politics of International Debt. Edited by Miles Kahler (Ithaca: Cornell University Press, 1986. 272p. \$9.95, paper).

The authors of these eight essays felt that the politics of international debt had not been discussed enough. Their attempt to rectify this omission resulted in this interesting volume. Miles Kahler sets the tone in the introductory article, which characterizes recent efforts at resolving the debt problem as "cooperation without reform." Kahler also provides a closing article evaluating methods of reforming the system. Both articles are well done, though sometimes overstated. In particular, his assertion (p. 245) that the arguments of other authors are pervaded by the notion that the burden of adjustment should fall on developing countries is patently untrue of some of the articles he cites.

Albert Fishlow provides a historical perspective in his discussion of the debt problems of the nineteenth and early twentieth centuries, distilling important works that have been neglected in recent years. He then draws out

the similarities and differences in the current debt problems. Other researchers have also attempted this task, but not so thoroughly as Fishlow.

Next come two essays on bank behavior. Philip Wellons provides some of the political and cost factors that distinguish banks based in different countries. His main point, however, might best be rephrased as follows: there are no banks whose home country has no influence on their activity; however, there are many banks operating across borders. That is but a modest contribution, which should be well known to those interested in the politics of finance. Benjamin Cohen argues that bank loans have often constrained U.S. policy toward Eastern Europe and the developing countries. For example, he feels that the banks reinforced U.S. influence in the early days of Latin America's problems but were a stumbling block at later stages when they were less willing to continue lending. The argument is quite interesting, but at times comes perilously close to the simplistic notion that foreign policy is much easier when someone else picks up the tab.

Stephen Haggard offers a useful summary of lessons from past International Monetary Fund programs. He points out that authoritarian governments do not have an inherent advantage over democratic governments in successfully implementing stabilization policies. Robert Kaufman, using Argentina, Brazil, and Mexico as models, shows how weak governments can sometimes transcend their limitations when truly creative initiatives are needed. These two articles aptly display the subtle political differences needed for a full understanding of the debt problem.

Charles Lipson provides an excellent summary of the role of international institutions in solving the debt problem. He carefully sets out his thesis that the role of public institutions in handling the debt crisis "is a response to coordination failures among private creditors and is limited by the extent of those failures" (p. 240) but does not go so far as to argue that some new mechanism centered on private institutions is needed.

All the essays in this collection are interesting, and should prove valuable to political scientists and economists alike. While most were previously published during 1985 in the journal, *International Organization*, this book

provides a very handy source for those who have not yet read them.

RICHARD P. MATTIONE

Morgan Guaranty Trust Company

Bureaucracy at War: U.S. Performance in the Vietnam Conflict. By Robert W. Komer (Boulder: Westview, 1986. xviii, 174p. \$17.50).

As an encyclopedic analysis of many of the military issues raised in the course of the Vietnam War, Komer's book is a must. He focuses on an endless number of technical, tactical, strategic, military and nonmilitary questions concerning the U.S. prosecution of the war. But nowhere is there any broader questioning of the presence of the United States in Southeast Asia. Komer does not deceive in this regard, however. In William Colby's foreord, the thrust is already clear: the Achilles heel of "so many fine policies . . . [is] their execution" (p. xiii).

"Fine" policies, in other words, to cite Colby again, were marred only by inadequate execution. In the first few paragraphs of his first chapter, Komer forecloses any serious discussion of U.S. policy—"Whatever the wisdom of U.S. intervention on the side of South Vietnam. . ." (p. 1)—and reasserts the same argument in Chapter 2: "It is important to look beyond why we intervened to the way we went about it" (p. 9). The author then proceeds to focus, chapter by chapter, on the long list of specific technical, logistic, and mostly military issues noted above. The implicit implication is that if only all of these had been handled better or differently, the war could have been "won."

Komer's book thus details the gap between policy and actual performance, taking his cue from Maxwell Taylor's observation, "One of the facts of life about Vietnam was that it was never difficult to decide what should be done but it was almost impossible to get it done." Komer reviews the inadequacies of the South Vietnamese government, the military mistakes made by the United States, and bureaucratic and institutional inertia. He then presents an extensive list of general as well as very specific policy recommendations. He notes that one constraint for the United States was the

"unfamiliar" conflict environment that it found itself in, and the incremental nature of the United States' response to every escalatory move by the opponent. He holds the South Vietnamese government very much to blame for the outcome of the war, even though, as he notes, many U.S. citizens intimately involved in Vietnam knew of the graft and corruption rampant throughout the South Vietnamese government. He also criticizes the U.S. armed forces for devoting so much energy to the technological side of the conflict, when it might have been better "to have devoted a more significant portion of our research to the nature of the conflict and the enemy, his patterns of operation, and better counterinsurgency techniques" (p. 51). Of course, the "rotation" pattern of 6-12 months for command slots also comes under fire, for it was, as Komer notes, "our only war," hence the only opportunity for military officers to get combat experience and concomitant medals. And of course the type of classic example of bureaucratic decision making—probably true in so many cases in the Vietnam situation as elsewhere—is aptly illustrated by Komer: "The Navy and Air Force preferred to use expensive F-4 Phantom jets in Vietnam rather than propellor-driven A-1Es because this way they got more Phantoms for their inventory" (p. 73).

Komer concludes the way he began: regardless of the original reasons for the United States getting involved, "the underlying conclusion . . . is the difficulty encountered by conventional government institutions . . . in responding optimally to such atypical problems as they encountered in the Vietnam war" (p. 159). This difficulty, moreover, goes a long way toward explaining the disparity between the effort mounted and the ambiguous results. Komer presents a long list of "lessons" that might have been learned from the Vietnam War, like "atypical problems demand specially tailored solutions" (p. 166), and "We must select flexible and imaginative conflict managers at all levels" (p. 168). But many of these are either hopelessly vague and general homilies or are so point-specific that they may not apply to any other situation. But more important, especially given current United States' policy in Central America, they are "lessons" that policymakers should have been aware of before the Vietnam involvement.

In other words, totally to bypass the

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"original reasons" for our involvement, as Komer does, and to focus instead on the endless series of specific "mistakes"—technical, military, tactical, strategic—is to identify all the dead trees but miss the forest. It is naive to act as if the "original reasons" for involvement are unrelated to or do not affect the ultimate waging of a particular conflict. Politics, ideology, motives and goals—depending on what they are, how rational and realistic they are, and how much popular support they can garner and then sustain—may serve either to constrain or to free decision makers in their prosecution of a conflict. They are directly related to the technical aspects of a war. Komer, as noted already, certainly does not try to mislead on this, and he does an excellent job of focusing on the specifics. But that can also tend to be misleading, for it implies that technical jiggling can change an outcome that may have been doomed because of the original reasons—or lack thereof—for involvement.

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Verification: How Much Is Enough? By Allan Krass (Lexington, MA: Lexington Books, 1985. x, 271p. \$14.95, paper).

Originally published for the Stockholm International Peace Research Institute, *Verification: How Much Is Enough?* is by far the most comprehensive book on the subject of verifying arms-control agreements. It is text that should be studied by arms-control practitioners and scholars alike. Most significant is its recognition and careful analysis of the political dimension of verification.

As a technical term in the vocabulary of arms control, *verification* refers to the process of assessing compliance with international treaty obligations. The process consists of a number of activities including monitoring (i.e., data gathering), information processing, data analysis, and identification of an observed event as a violation or not. A decision then must be made on an appropriate response.

Most prior writing on verification has focused on the monitoring stage of this process, and one can now find in the open literature a surprising amount of technical information

on U.S. surveillance and reconnaissance capabilities. A very useful summary of this literature is provided by Krass in a lengthy chapter, "The Technology of Verification." What sets the Krass book apart from other studies in the field, however, is the author's care in treating verification as "a complex and intimate interaction between technology and politics" (p. 12).

Both critics and supporters of prior arms-control accords acknowledge that formidable verification difficulties confront arms control today. Some of these difficulties are technical in nature and can be attributed to the fact that innovations in weapons to be monitored often outpace improvements in monitoring systems. Increased modernization and mobility of weapon systems, dual (conventional/nuclear) warhead capabilities, and greater compatibility of delivery systems with multiple launch platforms are among the technological developments that strain national verification capabilities. As Krass points out, technical advances in verification may mitigate these verification problems but are unlikely to resolve them. The benefits to be derived from more intrusive forms of verification (such as on-site inspection), too, are generally exaggerated.

The most complex and difficult-to-resolve problems of verification, Krass believes, are political and psychological in nature. As such, "they are more dependent for their solution on the creation of an appropriate atmosphere within which the compliance process can function" (p. 252). A major contribution of the Krass book is its attention to the environment in which U.S.-Soviet arms-control and verification deliberations take place. "At the root of most of the problems faced by verification," Krass notes, are deeply contradictory attempts by two powerful states to negotiate arms-control agreements even as they work diligently to maintain or enhance the credibility of their military threats against each other" (p. 252).

The picture Krass paints of the future of verification is not a rosy one. Where verification was once seen as a means to build public support for arms control, it now all too often serves as a political instrument for its obstruction. Nevertheless, by detailing the differences, as well as the points of convergence in U.S. and Soviet perspectives on verification, Krass

suggests areas where verification and arms control may yet prosper.

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Marxism and International Relations. By Vendulka Kubalkova and Albert Cruickshank (Oxford: Clarendon Press, 1985. 281p. \$19.95).

This book addresses an important question that has not so far received an adequate answer: is there a Marxist theory of international relations? The question is important for practical as well as theoretical reasons because of the impact Marxism has had upon world politics in the twentieth century. Marxists as political leaders have been willy-nilly practitioners of international relations, however imperfect their theoretical formulations. There is a problem for theory because the classical Marxian literature did not directly address the nature of the state and interstate relations. Twentieth-century Marxist literature has made considerable efforts to fill the gap as regards the state, but it has so far done little to deal with interstate relations. Imperialism and *dependencia* have had little bearing on superpower confrontation.

The authors begin by acknowledging three main intellectual traditions in international relations: the Hobbesian, focusing entirely upon the state; the Kantian, concerned ultimately with the universal community of mankind and with the state only as a mediating element; and the Grotian, which recognizes states but sees them as restricted by law and institutions. The authors see the Marxist tradition as closest to the Kantian, insofar as Marx envisaged the ultimate withering away of the state and the attainment of a nonalienated human community. The Kantian tradition is, however, basically ahistorical, concerned with timeless natural right. Much international relations theorizing in the Hobbesian tradition is also ahistorical insofar as it attempts to make statements about interstate behavior across time and space. Marxism is, by contrast, a theory of history that sees institutions, behavior, and theories as changing products of history. There could, accordingly, be no Marxist theory of international relations but

only a sociology of international relations that takes explicit account of the historical specificity of forms of state and structures of interstate relations.

So far so good. The authors have defined the problem. There are several ways it might be confronted. One would be to write a Marxist interpretation of international relations. This they do not attempt. Rather they adopt two other approaches. One is to consider as Marxist international relations the international behavior of states that have proclaimed themselves to be Marxist, or rather the doctrines of such states, not their actual behavior. The other is to give an account of the views of Marxist "professors," that is, theoreticians not responsible for the conduct of foreign affairs.

The account of Marxist state doctrines is the more interesting and useful part. What it does is underline the theoretical flimsiness of "actually existing" Marxism in this sphere. Class, which for Marx was the central category, becomes a metaphor emptied of its original substance. Initially, with Bukharin, the class struggle is projected onto the world scale, with different groups of states becoming class champions. This position, which became incorporated into Soviet thinking, still retains some semblance of a relationship between Marxian class definitions and the state system. The semblance is totally lost when the proletarian/bourgeois cleavage becomes simply a poor/rich cleavage and a virtually nonexistent proletariat is replaced by the peasantry or the more amorphous "wretched of the earth," and when even these vaguely class-related notions become replaced by that of the "proletarian nation." Marxism at this point has become nothing more than a slogan. The study of state doctrines for a clue to a Marxist theory of international relations, though interesting, has here reached a dead end.

The account of the "professors'" views is the more disappointing part of the book. It is little more than a series of summaries of the variety of Marxist or near-Marxist positions having some relevancy to international relations theory. Amongst these, Gramsci is given some prominence as one who "does appear to have widened, and smoothed the path (if not opened several possible alternative avenues) by which International Relations may 'arrive' in Marxism" (p. 204). But the authors fall short

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of doing justice to Gramsci and leave the reader with a sense of uncertainty (reflected in the tentative style of the above quotation) as to the answer they give to the question they confronted at the outset.

This reviewer hopes they will regard their present book as but a preliminary effort to be followed by a more analytical treatment of the problems, in both theory and practice, of bringing a Marxist perspective to bear upon international relations. In this effort, Gramsci would be a good starting point.

This book will be useful to students, given the paucity of material available. The bibliography both includes a wide range of tangential references and shows how few are the works that give a Marxist view of international relations.

ROBERT W. COX

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The Uncertain Crusade: Jimmy Carter and the Dilemmas of Human Rights Policy. By Joshua Muravchik (Lanham, MD: Hamilton, 1986. xxii, 247p. \$18.95).

A reader might reasonably expect to find a certain ideological slant in a book written by a former aide to Senators Henry Jackson and Daniel Patrick Moynihan (Joshua Muravchik), introduced by Jeane Kirkpatrick, and assisted in publication by Midge Decter. For the most part, this book on human rights and U.S. foreign policy under Jimmy Carter fits the expectation.

Muravchik starts with a superficial and incomplete overview of the Carter record, then focuses on four themes: whether the issue of human rights can transcend the East-West ideological struggle or must be part of it; whether the United States should base its human-rights policy on international law or on the U.S. tradition; whether U.S. human-rights policy can be consistent; and whether that policy should rely on punitive steps for implementation.

The best of these treatments pertains to the question of consistency. The chapter in question is nonideological, empirical, informative, and persuasive up to a point. It argues that the Carter human-rights policy was inconsistent because there was no central conception

behind the policy. Thus when the human-rights policy conflicted with other interests, the conflict was resolved in an erratic way. As Muravchik writes, "In sum, of the five reasons [sic] for inconsistency that are most often cited in discussions of human rights policy, the one whose effect was by far the most powerful on the Carter administration was the conflict between human rights and its other foreign policy goals" (p. 148).

This conclusion, however, does not fit very well with other parts of the book. In particular, it does not justify the emphasis placed on the personnel Carter appointed to the new human-rights bureau. The author argues that Carter erred when he appointed personnel from the McGovern wing of the party to the exclusion of the Jackson wing in staffing that bureau. It was not the McGovernite nature of personnel that made for problems, however, but rather the failure of any human-rights team to win policy debates systematically or within a set conceptual framework. Moreover, a number of decisions, especially on human rights and economic assistance, were made by Deputy Secretary of State Warren Christopher in a coordinating committee given his name. Muravchik's analysis sheds little light on the decisions reached by Christopher, and it certainly cannot be sustained that the lawyerly Christopher was a McGovernite.

Muravchik argues that a policy must strive for consistency to maintain its moral integrity. Although this argument has merit, this work fails to address competing arguments that also have merit, directly. David Owen, former British foreign secretary, has argued from practical experience that any foreign policy will necessarily be inconsistent on human rights (*Human Rights* [Topsfield, MA: Jonathan Cape, 1978]). And Stanley Hoffmann has argued that beyond a minimum consistency, there should be inconsistency tailored to particular situations (*Duties beyond Borders* [Syracuse, NY: Syracuse University Press, 1981]).

Beyond the issue of consistency, Muravchik is largely predictable, ideological, and unpersuasive to this reviewer. As for human rights and political systems, like his mentors in the Senate, he understands human rights basically as part of the East-West ideological struggle. Human rights means Western democracy and is thus a useful weapon in calling attention to

deficiencies in Communist countries. He is critical of the Carter administration for not doing that consistently and vigorously.

Interestingly, he acknowledges that it would be reasonable to prefer Communist Yugoslavia to Idi Amin's Uganda, but he dismisses such comparisons (and does not broaden his analysis to compare Hungary with Guatemala, China with Chile, etc.). Instead, Muravchik prefers comparisons based on "level of carnage" (p. 60). Because Stalin and Pol Pot killed many, this is taken as proof that communism is the worst violator of human rights. Certain Soviet-model political systems surely have a poor record on human rights, but the Argentine junta hardly fared better. Moreover, Stalin and Pol Pot can be differentiated from Alexander Dubcek, Robert Mugabe, and other Marxist leaders in a number of ways pertinent to human rights. Still further, there are a number of ways to evaluate human rights apart from the number of political deaths.

Muravchik, like former Ambassador Kirkpatrick, is obsessed with U.S. power in the world, seeing Carter's human-rights policy as a means of weakening the United States in its struggle with Soviet-led communism—especially since Carter's policy focused heavily on anti-Communist dictators in Latin America. This concern sometimes distorts analysis in this book. Muravchik writes of members of Congress interested in human rights: "the main impetus behind the growth of punitive measures was the desire not to advance human rights but to diminish American influence" (pp. 225–26). This will come as a great surprise to many mainstream members of both parties in both houses who have insisted on U.S. attention to human rights precisely to exercise power in the world, albeit in as humane a way as possible. Likewise, the author distorts the human-rights lobbying network in Washington, focusing on a few lobbies in order to repeat the claim that they were primarily interested in reducing U.S. power. When one examines the full range of lobbying in behalf of human rights, including action by the likes of Amnesty International and Americas Watch, one cannot seriously maintain the argument that the groups were primarily or really interested in a diminution of U.S. power.

Related to his argument about political systems is Muravchik's argument about international law. Unlike Senator Moynihan, who

has written powerfully in support of international law (*Loyalties* [New York: Harcourt, Brace, Jovanovich, 1984]), the author is contemptuous of the international law on human rights, regarding it as the product of hypocrisy, unenforceable, and entailing objectionable socioeconomic rights. The most charitable thing that can be said of this section of the book is that the author is uninformed on basic information and unpersuasive in interpretation. He shows no awareness of how once non-binding international norms can become binding over time, as illustrated for example, by U.S. federal courts (*Pena v. Filartiga*). His argument that there is no body of binding human-rights law overlooks the fact that about half of the member states of the United Nations have adhered to the Covenants on Civil and Political, and Economic, Social, and Cultural Rights. It strains credulity when he suggests that the Carter administration endorsed socioeconomic rights in order to court favor with the Third World and to satisfy the State Department's "clientitis" (p. 92). His attacks on socioeconomic human rights never mention that all the other industrialized democracies have found them compatible with their liberalism.

Having dispensed with international law on human rights, the author is left with a crusade in defense of U.S. tradition as a basis for U.S. human-rights policy. This becomes most clear toward the end of the book. "If we Americans cherish our human rights, and if we share with America's Founders the belief that these rights are 'unalienable' or that they are goods with which people have been 'endowed by their Creator,' then it is certainly morally permissible, probably even morally obligatory, that we do what we can within reason to help other people to secure theirs" (p. 222). A little later this becomes "America's mission" (p. 235). A reader may be forgiven for wondering at times whether the book is a justification for the Spanish-American War or Woodrow Wilson's mythical crusade for democracy, rather than an interpretation of Carter's human-rights policy.

Finally, the chapter on punitive measures is thin. The author does not like them, and, as noted above, he is critical of Congress and human-rights lobbies for pushing them. At another point, however, the author notes that Carter's threatened sanctions against the mili-

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tary of the Dominican Republic kept the soldiers in their barracks and preserved democracy there (p. 198). He does not analyze the impact of congressional pressures on the Guatemalan junta, which were eventually successful when joined with other pressures from public and private agencies. He entirely omits attention to the importance of the political symbolism of economic sanctions in places like South Africa. As David Baldwin has noted (*Economic Statecraft* [Princeton: Princeton University Press, 1985]), economic sanctions can be politically useful even if they do not totally control a situation. United States disassociation from a repressive regime may be neither isolationism nor a diminution of U.S. power, but rather a useful policy tool. All of this is not to deny the author's point that there may be a place for positive approaches to promoting human rights.

In sum, *The Uncertain Crusade* will be popular reading among Reaganite ideologues. (One of them recommended the book to me during a recent interview.) They will like Muravchik's assertions that "U.S. influence in . . . countries is generally a force for liberalization" (p. 69), however much that may contradict the facts in Nicaragua, the Philippines, Guatemala, Liberia, and elsewhere. They will like his view that Carter had a "commitment to a policy of national self-effacement" (p. 70). They will like his unvarnished nationalism, as well as his acceptance of the Kirkpatrick doctrine in favor of authoritarians rather than totalitarians. They may not like the more balanced segments, where he analyzes the problem of consistency (because the second Reagan administration appears to be just as inconsistent as Carter), or where he observes that Carter policy was not single-mindedly anti-Shah, or where he notes that the Somozistas are probably better than the Somozistas in terms of human rights. These balanced parts will be of use to scholars. Otherwise, one can only say that he has put forward his theses clearly. That is helpful as one formulates a reaction to his mostly unpersuasive interpretations.

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Sword and Shield: The Soviet Intelligence and Security Apparatus. By Jeffrey t. Richelson (Cambridge, MA: Ballinger, 1986. xiv, 279p. \$39.95, cloth; \$16.95, paper).

Jeffrey Richelson has gathered information about Soviet intelligence from numerous sources into a compact yet comprehensive and well-organized volume, clearly written and crammed with details. Subjects covered include human and technical collection, counterintelligence and internal security, covert action, theft of Western technology, Soviet use of other Communist intelligence agencies, and the organizational structure of both the Committee of State Security's Main Radio Administration (KGB/GRU) and the national-security establishment they serve. It will be useful for reference, for review, and in courses needing a single volume treatment of Soviet intelligence.

For those familiar with the subject, however, there is nothing new. The book draws entirely on published accounts by others—journalists, scholars, defectors, the CIA, and so on. It is an exercise in open-source collection. Any doubts one has about the accuracy and reliability of these sources on such a murky subject must necessarily apply to this book.

Missing is any analysis of the information collected. For instance, the reason for the Soviet preoccupation with intelligence is not addressed. Is it mainly a carryover of traditional Russian insecurity or a peculiarly Soviet phenomenon? Perhaps it reflects the Soviet system's conception of politics as subterranean, that is, that everything of importance happens well beneath the surface. Or maybe the gargantuan scale of Soviet intelligence reflects a bureaucratic penchant for growth within a congenial system and political culture, the explanation so often issued for other overblown aspects of Soviet officialdom.

Weaknesses or limitations of Soviet intelligence are also not assessed. One wonders if Soviet efforts aren't damaged by their scale and ubiquity, casting suspicion on almost any Russian abroad. Russian use of Cuban and East European intelligence is marred by the compromise of Soviet operations through defectors from, and Western penetration of, those agencies. The Soviet emphasis on Humint magnifies defection, double-agent,

and bad-publicity problems and seems odd in view of improvements in Soviet technical collection capabilities and the richness of open sources in the West.

Richelson is excellent on how much can be learned from those open sources but says little about their limitations—the staggering volume of information, much contradictory, much worthless. And because Soviet exposure to such information is so controlled, more “learning” from open sources must be done by intelligence agencies. This is a burden for which their organizational character and expertise are not particularly appropriate, immersed as they are (like intelligence agencies everywhere) in bureaucratic complexities, compartmentalization, and institutionalized paranoia. “Open” sources may be rather opaque to closed agencies in a closed society.

The concluding chapter does raise an important matter of an analytical sort, but only very briefly. Richelson suggests Soviet intelligence need not cause grave concern. It doesn’t permit a Soviet strategic surprise attack, doesn’t truly manipulate Western peace groups, can’t decisively deceive Western intelligence, can’t make a crucial difference by stealing our technology. Therefore we must not overreact in ways that compromise our liberties and values. This is a welcome conclusion, but it is not an analysis that the details in the book would lead us to adopt.

The paucity of analysis is not mentioned here as a reproach. The book does what Richelson obviously set out to do, namely compile information. But it is clear that we have far to go in nailing down the significance and meaning the information he has amassed.

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Technology Transfer and East-West Relations.

Edited by Mark E. Schaffer (New York: St. Martin’s, 1985. 273p. \$39.95).

The product of a 1983 London School of Economics Conference, this book is one of several recent edited volumes on the economics and politics of East-West trade prompted by the dispute between the United States and its allies over the construction of the Urengoi natural-gas pipeline from the Soviet

Union to Western Europe. Many of its arguments are similar to those found in volumes like Gordon Smith’s *The Politics of East-West Trade* (Boulder, CO: Westview, 1984) and Bruce Parrott’s *Trade, Technology, and Soviet-American Relations* (Bloomington: Indiana University Press, 1985). What distinguishes this collection is its uniquely European perspective and its uniformly critical evaluation of U.S. policies.

The first series of questions the authors pose concerns the impact of Western technology on the Soviet economy. Stanislaw Gomulka and Alastair McAuley agree that socialism inhibits innovation. The real deficiency of the Soviet economy does not lie in basic research, but in the systemic impediments to the diffusion of new technology. The impact of Western technology is limited, according to McAuley, because of its “failure to provoke a creative response” (p. 47) in an innovation-averse environment. Julian Cooper criticizes “Western technology fetishism,” downplays the significance of Western imports in Soviet economic growth and highlights the net resource-demanding effect of Western imports. Each ruble of imported technology costs the Soviet Union an additional two–three rubles in capital investment. David Holloway shows how, despite its technological backwardness in the military sector, the Soviet Union designs around its own shortcomings to achieve good operational performance. The Western technology of most interest to the Soviet military is microelectronics. Holloway, like the others, concludes that despite the attraction of Western technology for the Soviet system, one should not overestimate its importance for Soviet economic development because of the USSR’s strong indigenous research and development base. The Reagan administration incorrectly believes that a rigorous strategic embargo can bring the Soviet economy to its knees.

Stephen Woolcock addresses the question of how to frame a realistic, effective Western East-West technology-transfer policy. He lucidly details the problems but, like others who have tackled these issues, finds the answers elusive. U.S. and European approaches toward East-West economic relations are fundamentally incompatible, largely because of basic political differences over how to deal with the Soviets. Moreover, the United

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States, with less at stake economically but with primary responsibility for Western security, has stressed the strategic danger of East-West economic ties that strengthen the Soviet military-industrial complex. The Europeans, with more at stake economically and less responsibility for Western security, have emphasized the mutual commercial advantages of East-West trade. There is, therefore, an inherent tension between the U.S. leadership role in policy-making on strategic issues and European desires for equality in determining commercial policy. The Reagan administration's imposition of extraterritorial pipeline sanctions, which affected the United States' allies more than the Soviet Union, was particularly destructive for allied cooperation.

Hugh Macdonald, in the liveliest and most controversial chapter of the book, attacks the policies of those in the Reagan administration "whose claims to scientific rigor are in fact rooted in deformed theology" (p. 208). In his criticism of the strategic embargo, MacDonald makes a number of questionable assertions. For instance, he states that the size of the Soviet defense effort is directly related to that of the West. How would he explain the fact that in the 1970s, at the height of detente, while the United States cut back on defense spending, the Soviets forged ahead with an unprecedented military buildup? He also claims that since the Cuban missile crisis, "everything points to an enduring Soviet sense of—circumspection" (p. 226). Are we to interpret Soviet activities in Angola and Ethiopia and the invasion of Afghanistan as examples of "circumspection"? Although MacDonald's attack on U.S. technology-transfer policy contains some provocative and valid insights, it is flawed by his refusal to concede that the Soviet Union does present a security challenge, if not a threat, to the West.

There is a consensus in this book that the United States, which has exaggerated the impact of Western technology on the Soviet Union, must modify its strategic embargo if it is ever to achieve viable cooperation with its allies. On the other hand, one might also argue (although no one in this book does) that the Europeans must be more receptive to U.S. security concerns if the West is ever to arrive at a coherent and workable policy.

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Dominant Powers and Subordinate States:

The United States in Latin America and the Soviet Union in Eastern Europe. Edited by Jan F. Triska (Durham, NC: Duke University Press, 1986. vi, 506p. \$57.50, cloth; \$16.95, paper).

Here is a wide ranging and thoughtful volume skillfully edited by Jan Triska of Stanford University. Many of the contributors are international relations generalists, but there are a few specialists on both Latin America and Eastern Europe, including the USSR. In general, the authors seek neither to defend nor to attack dominance but to analyze its origins, instrumentalities, costs, and impacts coolly.

The book's most important contributions are contained in its conclusions, concisely summarized by the editor at the end of the volume. The first is that "domination, as practiced in the past, is no longer in the national interest of dominant powers." Certainly, that is true for the United States, especially when national interest is conceived as embracing the entire society. Yet that does not seem to be the view of large sectors of the U.S. public who are still enamored of the United States being "Number One" as if that had some kind of intrinsic value of its own without reference to purposes, implications, and consequences.

The U.S. public has usually been unwilling to look U.S. hegemony squarely in the face and certainly has been unwilling to discuss it in the same breath as Soviet hegemony in Eastern Europe. The Triska volume should help bring some reality into the public dialogue on this subject. U.S. citizens should not be surprised that Washington has dominated other countries. All great powers have done so. Hegemony, or domination, is more the result of disparities in power, that is, of the structure of relationships, than the personalities or policies of particular governments. Recognition of the history and nature of hegemony makes it neither desirable nor right but it does help various actors cope with it more effectively.

With reservations I agree with the book's conclusion that domination is no longer in the national interest of the Soviet Union. Soviet domination of Eastern Europe, for example, has been costly, and the Eastern Europeans' bitterness and hate of Soviet interference will some day come home to roost. But Soviet domination of the area appears essential to the security of the ruling Soviet elite and their con-

trol over their restive populations. My conversations with Soviet officials make it plain that, whatever their personal preferences, Soviet control of Eastern Europe is a sine qua non of the present regime.

The second conclusion is that "the dominant powers will eventually have to yield, however grudgingly, to the subordinate states." Certainly, that process appears to be underway in the Caribbean Basin where the United States has so far decided that armed attack by its regular forces would be too high a price for military control of Cuba and Nicaragua and possibly that military occupation of these two countries would not be viable. The question arises whether a compromise with these two governments might suit U.S. interests more than the existing situation.

Prospects for greater autonomy in Eastern Europe do not seem promising in the near term. While the USSR might like to reduce tensions and its economic burdens, the taste of greater independence might whet appetites beyond Soviet control. As Triska makes clear, greater autonomy is more likely to be wrong at great effort from the USSR rather than granted as an act of grace.

Part 1 deals with historical comparisons in chapters by Jeffrey L. Hughes, Robert Wesson, and James R. Kurth. Part 2 covers concepts and theories with chapters by David B. Abernethy, Paul Keal, Gabriel A. Almond, and Jeffrey L. Hughes. Parts 3 and 4 treat the domination powers (David D. Finley, Terry Karl, Richard R. Fagen, Condoleezza Rice, and Jiri Valenta) and subordinate states (Paul M. Johnson, Robert A. Packenham, and Jeffrey L. Hughes). Part 5 discusses spheres of influence with chapters by Paul Marer and Kazimierz Z. Poznanski, Richard Ned Lebow, and Michael I. Handel.

Dominant Powers and Subordinate States is the most measured, comprehensive, and authoritative book on this subject and is likely to remain so for a long time.

COLE BLASIER

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Revolution and Foreign Policy in Nicaragua.

By Mary B. Vanderlaan (Boulder, CO: Westview, 1986. xiii, 404p. \$29.95).

This volume is a useful addition to the large and ever growing body of literature on Sandinista Nicaragua. Some aspects of Nicaragua's foreign policy and international relations—its strained and deteriorating relationship with the United States, the Contadora effort, Nicaragua's growing relationship (diplomatic, military hardware and training, and development assistance) with Cuba, the USSR, and Soviet allies in Eastern Europe—have been dealt with extensively in the media, in the speeches of public officials, and in various scholarly books, as well as books aimed at a wider, more popular audience. The Vanderlaan book looks at Nicaragua's foreign policy and international relations in their totality, exploring aspects that have not been touched on by other works.

At the outset, the author makes the important point—albeit not original with her, yet still of importance—that "the Sandinista revolution was, among other things, a reaction to the history of United States-Nicaraguan relations" (p. 3). That point is not adequately comprehended in Washington. If it had been, the United States almost certainly would have taken a different tack toward Nicaragua.

The book is divided into four parts. Part 1 offers a survey of Sandinista Nicaragua's foreign policy and international objectives. It makes clear that Nicaragua's foreign-policy behavior and objectives—expanded diplomatic contacts, greater independence, diversification of relationships of dependency—is very much in line with what most Latin American countries are doing or trying to do. If the reviewer reads this and other parts of the book correctly, the Sandinista ideal would have been to avoid the substantial reliance on Cuba, the USSR, and Eastern Europe that is increasingly a Nicaraguan reality. What the Sandinistas apparently desired was a much more diversified complex of international relationships.

Part 2 explores the domestic constraints on the foreign policy of post-Somoza Nicaragua. It describes the country's new political structures and the internal policies of the new regime. Detailed attention is given to the economic constraints facing the Sandinistas and

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the internal opposition to the government and the internal opposition's external allies.

Part 3 is concerned with the international constraints. These are mainly U.S.-imposed: the general U.S. hostility to the current Nicaraguan government, the mining of Nicaraguan harbors, the backing of the "Contras" and their activities, and pressures on U.S. allies to withhold aid of both an economic and a military sort to Nicaragua. This part also treats the Latin American and Western European reaction to the Sandinistas. What emerges from these chapters, as well as earlier ones, is that U.S. hostility and U.S.-backed "Contra" action has hampered the Nicaraguan socioeconomic reforms. Indeed, the impact of that action has been far more socioeconomic than military.

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The book is the product of much research and interviewing in Nicaragua as well as a thorough survey of the secondary literature. It is definitely sympathetic to Sandinista Nicaragua. But the book is not an apologia, nor is it so biased as to lack credibility. The author sees flaws and problems as well as more positive factors.

Vanderlaan has produced a thoughtful, well-researched, and well-written book.

JAMES D. COCHRANE

Tulane University

From Marshall Plan to Debt Crisis: Foreign Aid and Development Choices in the World Economy. By Robert E. Wood (Berkeley: University of California Press, 1986. xviii, 400p. \$32.50, cloth; \$11.95, paper).

The scope of this book includes not only foreign aid—as that topic is narrowly construed, encompassing bilateral government

assistance—but also private loans to third world countries, and multilateral assistance from the World Bank and International Monetary Fund. Its survey of these topics is impressive in detail and thoroughness. It carefully narrates the evolution of financial relations between the industrialized and nonindustrialized nations beginning with the early post-World War II period of the Marshall Plan and continuing up through the debt crises of the early 1980s. If you are looking for a chronology of these topics, this is the book for you. If, on the other hand, you are looking for central themes, points, or conclusions about the profoundly important topics of aid, debt, and other international financial policies, you will have to look elsewhere.

No author should have to suffer reproach for failing to accomplish something he had no intention of doing. In the case of *From Marshall Plan to Debt Crisis*, however, the author explicitly has more in mind than pure narrative. It is, in fact, the enormity of his ambition that cripples his achievement. In the introduction, he states, "I have sought to identify the overall way that access to concessional external financing is structured and to link that structure to the issue of development options and choices in the Third World" (p. xiv). Hunting for something as illusory as "the overall way" that concessional financing takes place is bound to result in a description of "the way" so general and amorphous as to be nothing more than a thin conceptual veneer over a base of factual narrative.

The author attempts to present a coherent structure of the immensely complicated subject by drawing upon the body of concepts called "regime analysis" and "world systems" (p. xiv). Using these concepts results in unnecessarily awkward side trips. For example, one characteristic of "regimes" is that they have "norms." The foreign-aid regime does indeed, have norms, such as "regional withholding" (p. 116). The author's point, as we eventually discover, is that the U.S. government resisted giving much foreign aid to Latin America in the 1950s because, the government argued, Latin America had access to adequate capital through private foreign investment. Classifying this policy as a regime norm leads to neither analysis nor evaluation. It is just an excessively complicated way of describing U.S. policy.

Although very interesting and suggestive, Jon Elster's contribution is equally vulnerable to criticism. Most of the charges he lays against social-choice theory do not stick, as Sen effectively shows. Elster accuses social-choice theory of confusing "the kind of behavior that is appropriate in the market place and that which is appropriate in the forum" (p. 111). Sen's concluding judgment seems right: "Elster's criticism of social choice theory turns out to be inappropriate not because he is wrong in counting certain constraints as unjustified, but because he wrongly attributes these constraints to social choice theory" (p. 236).

Robert Goodin, in his essay, "Laundering Preferences," argues that social-choice theory can exclude certain preferences without having recourse to nonutility information. Sen and others have criticized "welfarism," the view that social states are to be evaluated solely in terms of utility information, arguing that reference to nonutility information is necessary. Goodin persuasively argues that perverse and meddlesome preferences can often be excluded on the basis of various "want-regarding" considerations.

John Roemer is critical of welfarism and sketches "an historical materialist alternative." He proposes ranking social states in terms of "socially unnecessary exploitation": a distribution with less socially unnecessary exploitation is better than one with more. The notion of *exploitation* is characterized in terms of the core of a game: a coalition of agents is exploited, at a given allocation, if that allocation is not in the core, that is, if the members of that coalition could better their positions by withdrawing from the economy. Different concepts of *exploitation* are defined in terms of the withdrawal rules of different games. *Feudal exploitation* is defined by the game permitting a coalition to withdraw taking with it only its endowment; *capitalist exploitation* by the game permitting a coalition's withdrawal with its per capita share of the alienable assets of society (e.g., the means of production); and *socialist exploitation* by the game permitting a coalition's withdrawal with its per capita share of society's inalienable assets (e.g., skills) as well. This conception of *exploitation* may not fully capture intuitive and Marxist notions; it nonetheless is the basis for richer notions of *efficiency* and provides an interesting alterna-

tive to existing social-choice theory. Sen claims that the theory should be able to make room for such alternatives (p. 223), although some of the requisite changes (e.g., dropping the condition of independence of irrelevant alternatives) would radically transform current theory.

Two essays, by the philosophers Allan Gibbard and Donald Davidson, discuss issues having to do with interpersonal comparisons of utility and interests. While the use of social-choice theory for the study of choice institutions (e.g., voting mechanisms) characteristically does not make use of such information, other uses (e.g., making "social-welfare judgments") do. Insofar as the question of the nature of interpersonal judgments is foundational for these uses of social-choice theory, inclusion of these papers in this collection is warranted.

Gibbard argues in favor of a concept of the good of a person or of "the intrinsic reward of life" to serve as the standard for utilitarian social-welfare judgments. He argues that alternative candidates for standards of welfare (e.g., ideally informed or self-interested preferences) are inadequate, and he consequently favors the concept of *intrinsic reward*. It is this concept, Gibbard stresses, and not that of interpersonal comparison, that is most problematic for normative social-choice theory. Without it, notions of *prudence* and even of *rationality* become problematic. Without endorsing all of Gibbard's arguments, his essay can be recommended especially to those who remain skeptical of the very possibility of interpersonal judgments.

For radical skeptics, as well as for those interested in the *basis* of interpersonal judgments, Davidson's essay, "Judging Interpersonal Interests," is required reading. Davidson argues that the very process by which we attribute beliefs, desires, preferences, and the like to others makes interpersonal judgments. Thus the basis for such judgments is to be found in these attributions. Beliefs and desires explain intentional behavior. In interpreting the beliefs and other propositional attitudes of another, one assigns propositions to those attitudes (e.g., "A believes or desires that *p*"). In so doing we make use, and must make use, of propositions available to *us*. Comparisons are unavoidable in the very process of attribution. (Further, these comparisons are not, Davidson

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Elsewhere, the author discusses the role of the U.S. government's Overseas Private Investment Corporation (OPIC), touching upon the question of just how much additional foreign investment is produced because of the availability of OPIC's political risk insurance. He refers to an apparently definitive piece of evidence: "One study of investment plans turned down by OPIC found that 97 percent of their projected value was not subsequently invested" (p. 165). Curiously, this study has no identification: no footnote, no source, no time frame, nothing. Given the author's meticulous footnoting elsewhere and given the fact that the "additionality" of OPIC's insurance remains a hotly debated policy question, the

lack of a source here is frustrating, to say the least.

Similarly, at the very end of the book, Wood engages in dalliance with relevance once again. He speaks of the dynamic mutual interaction between the constraints of the international financial system (or "regime" if you will) on the one hand, and the developments of democracy or authoritarianism in the Third World (p. 326). This is, without doubt, a profoundly important question, surely worth more than a paragraph.

MICHAEL K. O'LEARY

Syracuse University

POLITICAL ECONOMY

Fighting Poverty: What Works and What Doesn't. Edited by Sheldon H. Danziger and Daniel H. Weinberg (Cambridge: Harvard University Press, 1986. 419p. \$27.50).

Taking their cue from Ronald Reagan's fascination with the *Reader's Digest* account of a welfare recipient living well in a New York hotel, a number of studies purport to reinforce this view of what is happening in the current war on poverty. Although the flawed designs of Charles Murray's *Losing Ground* or David Stockman's Office of Management and Budget report on poverty increases are transparent, there has been no important body of research seriously challenging the Reagan ideology until now.

Virtually all the studies included in *Fighting Poverty* represent profound insights into the basic issues of poverty policy—the status of welfare programs and their effects (Gary Burtless); impacts of antipoverty strategies on the poor and the nonpoor (Sheldon Danziger, Robert H. Haveman, and Robert D. Plotnick); welfare effectiveness and disincentives (David T. Ellwood and Lawrence Summers); health care (Paul Starr); job training (Laurie J. Bassi and Orley Ashenfelter); education (Nathan Glazer); how inflation, recession, and other features of the economy affect the poor

(Rebecca M. Blank and Alan S. Blinder); family structure (Mary Jo Bane; William Julius Wilson and Kathryn M. Neckerman); legal rights (Michael R. Sosin); racial discrimination (Charles V. Hamilton and Dona C. Hamilton); and public opinion on poverty issues (Hugh Heclo). These studies also benefit from commentaries by Christopher Jenks, Edward M. Gramlich, and Daniel H. Weinberg. The reader will note that many of these contributors have well-established reputations as being among the most knowledgeable scholars of the topics addressed. Several of the less-known scholars have made stunning contributions to this volume.

Each contribution offers a review and critique of important research bearing on the various topics over the last decade. These bibliographic essays and the massive bibliography alone are worth the price of the book. Yet, in addition, many of the authors present original analyses of data and reach original conclusions that cast considerable doubt on the growing conventional wisdom of poverty policy. The research is carefully crafted and highly persuasive. It is precisely these findings that need wider dissemination and attention of the mass media if U.S. policymakers are to reach a more realistic appraisal of the true parameters of poverty in the United States.

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A common theme of the work is viewing the past 10 years as a period of social experimentation in what works and what doesn't in treating the poverty problem. These experiments have been carried out largely at the expense of the poor, and the lessons purchased at such a price should not be lost:

Although total outlays for the poor have continued to mount, all of the recent rise is attributable to inflation and rising poverty. . . . None of the rise has been due to more generous government provision for the low-income population. (Burtless, 18).

The first lesson is that the income support strategy of the past two decades has worked. Providing cash and in-kind transfers has reduced the extent of both poverty and income disparities across age and racial groups. . . . Rather modest efficiency losses have resulted. (Danziger, Haveman, and Plotnick, 74).

Decent health care for the poor is not a fiscal impossibility, nor a political impossibility, unless we become utterly resigned to a kind of national incompetence in public policy. (Starr, 132).

There is very little justification for low AFDC benefits or for the enormous variations across states. (Ellwood and Summers, 97)

Programs providing intensive (and expensive) investment in each participant, such as the Job Corps and Supported Work Demonstration, have, at least for some groups of the disadvantaged, more than paid for themselves from a society-wide point of view. (Bassi and Ashenfelter, 148).

We know enough . . . to suggest that greater resources would be most usefully spent in pre-schools and elementary schools . . . that more poor minority youth will be able to get through high school. (Glazer, 172).

Despite many denunciations of inflation as the cruelest tax, there is little doubt that unemployment, not inflation, actually bears most heavily on the poor. (Blank and Blinder, 183)

To sum up, this research indicates that welfare receipt or benefit levels have no effect on the incidence of out-of-wedlock births. (Wilson and Neckerman, 251)

Despite Americans' endorsement of free enterprise and many antigovernmental attitudes with respect to the economy, one of the most enduring findings is the continual public support for government guarantees of a job for all those who want to work. (Heclo, 331)

As a summary of the best research on pov-

erty, this is an extremely important work. In addition to its profound contributions to thinking about poverty policy, it establishes an agenda for future research. It also offers a retrospective analysis of what has and has not worked in the past from a more manageable perspective toward the War on Poverty. The conclusion is that it was more successful than we have been led to believe. Students of poverty policy must be intensely grateful to the Institute for Research on Poverty for producing this book.

ROBERT B. ALBRITTON

Northern Illinois University

Foundations of Social Choice Theory. Edited by Jon Elster and Aanund Hylland (Cambridge: Cambridge University Press, 1986. viii, 250p. \$32.50).

The editors of *Foundations of Social Choice Theory* worry that "formal theorizing in the social sciences is today in some danger of becoming baroque" (p. 1). Their hope is that this collection of essays on various topics in social-choice theory will contribute to shifting discussion towards foundational issues.

Two of the contributors, Brian Barry and Jon Elster, are especially critical of social-choice theory and find much of it misguided and mistaken. Barry's essay focuses on Amartya Sen's well-known "liberal paradox" and argues that there is no conflict between liberalism properly understood and Pareto efficiency, and that social-choice theory neglects both the rights and the duties of actors. These claims, as well as others, are criticized by Aanund Hylland and Sen in their essays. Barry's contribution suffers from a surfeit of points and claims, some of which are mistaken if intended as criticisms of the literature (e.g., a literal reading of 'x is socially preferred to y'). This is unfortunate, as some of the issues about liberty and rights that he raises are important. (The matter of the *liberal* nature of Sen's "paradox" deserves further discussion. Sen notes that the problem is at least in part one of instability [p. 227], which raises the question as to whether the paradox is a special case of a more general instability problem having nothing specifically to do, as Barry claims, with liberalism.)

Although very interesting and suggestive, Jon Elster's contribution is equally vulnerable to criticism. Most of the charges he lays against social-choice theory do not stick, as Sen effectively shows. Elster accuses social-choice theory of confusing "the kind of behavior that is appropriate in the market place and that which is appropriate in the forum" (p. 111). Sen's concluding judgment seems right: "Elster's criticism of social choice theory turns out to be inappropriate not because he is wrong in counting certain constraints as unjustified, but because he wrongly attributes these constraints to social choice theory" (p. 236).

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argues, "normative.") The argument of Davidson's essay is subtle and makes use of ideas familiar to philosophers amongst whom his work is well known.

The collection does succeed in raising foundational issues about social-choice theory, as well as of much formal theory generally. Unavoidably some of the essays are flawed, and some foundational issues are neglected. The volume as a whole could have been better, I believe, had the editors written a lengthy introduction characterizing social-choice theory in general terms, relating it systematically to other types of formal theory (e.g., welfare economics, game theory). Such a characterization is difficult, to say the least, and the surveys and analyses provided by Hylland and Sen in their contributions are very helpful to this end. Nonetheless, something more general and systematic than the editors' brief and somewhat idiosyncratic introduction would have been useful, at the very least, to establish a common framework for the contributors (some of whom attended the conference in Ustaoset, Norway where the volume originated). With such an introduction, the volume would be useful to a wider audience; as it is, it is accessible primarily to readers with considerable familiarity with social-choice theory.

CHRISTOPHER W. MORRIS

Bowling Green State University

Values at Risk. Edited by Douglas Maclean (Totowa, NJ: Rowman & Allanheld, 1986. 168p. \$28.50, cloth; \$15.95, paper).

This is a collection of eight papers on the large problem of how public policy should deal with risks, chiefly risks to life. A major issue in six of the papers, written by philosophers and economists, is the calculations of costs and benefits in deciding upon life-saving expenditures and regulations. This review will focus upon these six papers.

Several of the writers, in their evaluation of cost-benefit analysis, refer to a classic problem in social-contract theory: supposing that people were to reason objectively, as though they were ignorant of their parochial interests, how would they wish government to deal with risks? Economists Leonard and Zeckhauser

respond that such individuals would consent to policy based upon the cost-benefit principle because this principle aims to maximize net gains for the group as a whole. Yet they uneasily concede that the public, through the political process, reveals distrust of the use of cost-benefit analysis concerning risks to life. They do not explain this other than to imply irrationality in politics.

As if in reply to economists—there is no direct interchange—the philosophers Maclean, Baier, and Gibbard dispute this maximizing principle. These philosophers suggest why our society, for good reason, rejects a core idea in cost-benefit calculations, which is to allocate limited life-saving resources so as to maximize the number of lives saved. They observe that policymakers finance expensive rescue missions to save identified individuals even when this implies sacrificing a larger number of unidentified lives that could be saved at less expense in other ways. Such rescue policies, the philosophers argue, are rational in that they publicly affirm the value we place on particular human lives irrespective of costs, a sort of open-ended entitlement to be rescued.

But placing greater value on identified lives in this way leads us to unanswered questions concerning both fairness and efficiency. To what extent is the special sense of obligation to persons we can identify objectively grounded in principles of fairness? Suppose we were to deal with the budget constraint by maximizing life saving but only within particular categories that are considered morally different, such as identified and unidentified persons. By what criteria do we define the various categories and then assign priorities among them? Is, for example, self-care such a criterion? Is it fair to spend a lot to rescue persons who have acted recklessly while sacrificing a larger number of unidentified lives because funds run short for preventative pollution control? And is it efficient to do so considering the possible disincentives to taking precautions? Then too, what if technology continues to expand opportunities to save identified lives at increasing total cost not just through rescues but through organ transplants, and so on? How far is society to carry this particular affirmation of the value of life at the increasing sacrifice of competing values? (cf. Nicholas Rescher, *Risk* [Washington, DC: University Press of America, 1983], Chap. 14).

Leonard and Zeckhauser would cut through these problems by appropriately measuring, in order to compare and aggregate, the benefits of all life-saving programs in terms of willingness to pay. Their test of justifiable life-saving follows the model of market exchange: estimate whether those who would benefit from the proposed protection would be willing to compensate fully those who would bear the costs, though for reasons of practicality, compensation would not actually be required. Drawing on a theory in welfare economics, Leonard and Zeckhauser try to dispose of the difficult problem of distributive equity. They would have government use taxes and expenditures, as though through a separate distribution branch of the federal budget, to achieve the "appropriate" distribution of income before the benefits of protection are measured.

In the field of social ethics, especially since the appearance of John Rawls's work, *A Theory of Justice*, a literature has flourished on the problem of distributive equity and the Kantian view of the intrinsic rights of the person as against group interests. A number of philosophers have attacked the utilitarian principle of maximizing the good, irrespective of distributive effects, which constitutes the foundation concept in cost-benefit calculations. However, as indicated by the questions raised above about affirming the value of life through rescues, the philosophers in the book under review have chosen not to pursue the problem of distributive equity regarding risks. For example, although the second paper by Maclean nicely clarifies how single measures of risk for the group conceal different ways in which the risks may be distributed, none of the papers pursues the trade-off between maximizing and equalizing protection. The interested reader can find analysis of this policy problem in the economics literature (see Carl Shoup, *Public Finance*, p. 117).

Nor is it the purpose of this set of papers to develop a theory of how governmental institutions, particularly legislatures and regulatory agencies, actually do treat risks. Such an endeavor would need to consider the forces that shape policy-making, and also agency enforcement practices, regarding environmental and other sorts of risks. Yet, the writers' ethical programs are of course influenced by their perceptions of what our institutions permit us to accomplish in the particular cases

they have in mind. It is therefore fair to note the conflicting factual assumptions about policy-making, perhaps of special interest to the readers of this journal, that tend to divide the advocates and critics of cost-benefit analysis in the book.

On the one hand, Leonard and Zeckhauser tell us that the political process is "unpredictable" (that is, they lack a theory to predict it?) and "unfettered" in its tendencies to neglect the general welfare unless subject to the constraining and rationalizing effect of cost-benefit analysis. Yet if the forces that move policymakers do not favor the general welfare, why should policymakers wish to adopt a technique that, as the advocates of cost-benefit analysis assume, would promote it? On the other hand, Baier, who seems to represent the other philosophers in her more trustful view of the political process, fears giving "the rationalizers a free hand," as though any hand in government can ever be made free of political constraint. Baier would revise our moral code to apply the full force of the criminal law to polluters who knowingly poison us, but she does not pause to consider whether a view of the social welfare different from hers may constrain policymakers from restricting pollution to the extent she urges. We are left with an old question: What part of the argument over social ethics is due to disagreement in belief as to what the controlling values of the public that operate through our political institutions are?

The social scientist who is interested in the issue of conflicting values in regulatory policy, especially about life saving, will find this book, despite some unevenness in quality, worth reading. For the selective reader, this reviewer would recommend especially the discussion of the distribution of risks by Maclean, Hacking's treatment of risks that cannot be quantified, and Gibbard's wrestling over whether risks to life should be quantified. The case for a cost-benefit approach to risk policy articulated by Leonard and Zeckhauser might well be read first by those who wish to review the concept of social-welfare maximization the philosophers are questioning.

DONALD C. CELL

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1987 Book Reviews: Political Economy

Averting Catastrophe. By Joseph G. Morone and Edward J. Woodhouse (Berkeley: University of California Press, 1986. 224p. \$16.95).

In an effort to go beyond the detailed case studies that have constituted most of the literature of risk policy, many recent writers have tried to explore the managerial side of risk regulation. *Averting Catastrophe* falls clearly into this management school. Its five case studies emphasize strategies for controlling risks and form the basis for the rather novel conclusion that these strategies are quite successful.

The five cases include toxic chemicals, nuclear power, recombinant DNA, threats to the ozone layer, and the greenhouse threat. Each of these has received book-length treatment in its own right, and the first three are perhaps unbearably familiar to readers of the science-policy and risk-policy literatures. Morone and Woodhouse have written no more than 20 pages about each. Nevertheless, their clear and generally fair discussions are probably the most economical descriptions of the cases available. The response to nuclear power, for example, is divided into two phases: first, prevention plus containment and second, after new scientific evidence suggested the inefficacy of containment, prevention alone. The chapter on threats to the ozone layer goes well beyond the traditional discussions of fluorocarbons to include both the SST and other chemical threats, including fertilizers. The utility of these short studies is increased by the way in which the authors elicit more general regulatory strategies from the specifics of each case.

The final chapters integrate these managerial findings into a system for averting catastrophe: (1) protect against the possible hazard, biasing early judgment against it; (2) reduce uncertainty by prioritized monitoring of experience and testing of risks; and (3) revise original precautions in light of information gained through experience with the risk.

Although neither novel nor unrecognized, the managerial strategies have seldom before been used to provide a uniform approach for assessing a wide variety of cases nor so explicitly brought forward as evidence for the overall success of risk-management policy in the United States. Unfortunately, the conclu-

sions about the potential uses of these strategies are not as convincing as the earlier parts of the book.

According to the authors, *analysis* is an activity that consists of seeking more and more (scientific) facts in an attempt to resolve policy conflicts, while *strategy* is a procedural approach that allows policymakers to take action without actually resolving the conflict. Morone and Woodhouse want analysis to be subordinated to strategy. Yet their own case studies are clearly in the analytic camp—choice of regulatory strategy is explicitly shown to be dependent upon the level of scientific or technical knowledge in each case.

Furthermore, the level of generality of the "strategies" proposed (for example, "attack egregious risks") means both that analysis is necessary for strategy to be implemented and that the strategies are very open to political maneuvering. At the outset of the book, however, the authors argue that political matters are superficial epiphenomena irrelevant to their task of highlighting deeper, systematic managerial responses.

Finally, there is little attempt to delineate the characteristics of problems that would allow appropriate control strategies to be selected. An argument that properly exercised strategies explain the success of U.S. risk management to date (itself a conclusion that readers may well not accept) surely would be strengthened by reference to the literature relating risk characteristics to control mechanisms.

It is fashionable in the risk literature to contrast rational scientific analysis with irrational political decision making. Morone and Woodhouse are social scientists. They believe that there are rational managerial procedures, or strategies, that can mediate between the two ends of this false continuum. Their book is well suited for classroom use because of the clarity and brevity of the case studies and the emphasis on these managerial strategies, but it will only provoke, not resolve, discussions about the complex interrelationships between science, management, and policy.

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Is Prevention Better Than Cure? By Louise B. Russell (Washington: Brookings Institution, 1986. x, 134p. \$26.95, cloth; \$9.95, paper).

Increased interest in cost containment has focused attention on assessing the costs and benefits of various medical interventions. Preventive measures are seen by many as potentially cost-saving alternatives to traditional medicine. This book combines a discussion of prevention with an introduction to one method of program evaluation, cost-effectiveness analysis (CEA). The principles of CEA are explicated through discussions of increasingly complex prevention issues. The preventive measures examined are smallpox and measles vaccinations, hypertension screening, and increased exercise.

Many preventive measures do not reduce medical expenditures. By their nature, prevention programs yield future benefits which are often uncertain and difficult to quantify. So, while prevention may be preferred to cure by some individuals, cure may be cheaper in terms of medical expenditures. The author argues that advocates of prevention must demonstrate that the overall benefits of prevention outweigh the costs. The use of CEA to evaluate prevention programs would permit more rational decisions about resource allocations.

The description of CEA as a method of identifying, assessing, and summarizing the costs and benefits of any particular program is lucid and concise. This work complements other books in the area (Mishan's *Cost-Benefit Analysis*; Warner and Luce's *Cost-Benefit and Cost-Effectiveness Analysis in Health Care: Principles, Practice, and Potential*) by providing a nontechnical introduction. Mathematical notation is kept to a minimum, and the use of increasingly complex examples works beautifully. The book is aimed at consumers of CEAs rather than practitioners. Instructors of health economics, public health, health administration, and health-policy courses will find this a useful supplementary text.

Inconsistent treatment of measurement issues is the principle shortcoming of the book. The author argues strongly for two measures and against a third, yet all share similar characteristics. First, quality-adjusted life years (QALYs) are advocated as potentially

objective outcome measures. One QALY represents a year of healthy life, and years of less-than-perfect health receive values between zero and one. Clearly, the derivation of QALYs is subjective and involves the aggregation of preferences. Second, the cost-effectiveness ratio is advocated over the balance-sheet method of presentation because of its simplicity. However, the value of the ratio varies with the assumptions of the CEA, so that this measure is somewhat subjective. In contrast, the use of income as a measure of the opportunity cost of time is discouraged. After discussing the difficulties involved in valuing time, the author recommends simply listing the hours of time involved. This third recommendation is at odds with the previous two, and it would seem at least as difficult to define a QALY as to find the opportunity cost of someone's time.

The clear exposition and historical detail of the book more than outweigh this criticism. For example, the discussion of the development and dissemination of the smallpox vaccination is fascinating. The description of the different types of preventive programs is interesting, and an appendix provides information on federally funded prevention programs. Overall, the book is a welcome addition to this area.

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The Motorization of American Cities. By David St. Clair (New York: Praeger, 1986. xi, 192p. \$32.95).

St. Clair's is the latest of several recent volumes (Barrett, *The Automobile and Urban Transit*; Foster, *From Streetcars to Superhighway* [Philadelphia, PA: Temple University Press, 1983 and 1981 respectively]; and Yago, *The Decline of Transit* [Cambridge: Cambridge University Press, 1984]) on the dramatic shift from mass transit to automobiles in U.S. cities during the first half of this century. St. Clair's thesis is that the automobile industry, and General Motors in particular, played an important role in hastening the demise of mass transit by first conspiring in the 1930s to finance the conversion of streetcar lines into inferior motor-bus lines, and then lobbying in

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the 1940s and 1950s for federal funding to build urban expressways that eventually resulted in the interstate highway system.

The streetcar conspiracy thesis has been around for a long time, fueled by the Department of Justice's successful antitrust prosecution in the 1940s of GM in the National City Lines case. General Motors established National City Lines to buy up ailing streetcar lines and finance their conversion to GM motor buses. The Department of Justice argued that in so doing GM was attempting to restrain competition among bus vehicle suppliers. St. Clair and another recent author (Yago) contend that GM had a more ambitious goal in mind: to increase auto sales in urban areas by saddling mass-transit companies with motor buses when streetcars or trolley coaches (electric buses) would have been more efficient and profitable.

The critical evidence St. Clair marshalls to support his "conversion for destruction" hypothesis is flawed and unconvincing. St. Clair recognizes that we are unlikely to ever know for sure what GM's executives had in mind when they created National City Lines, so he concentrates on trying to prove that modern streetcars or trolley buses were more cost-efficient and profitable than the motor buses GM was promoting. Despite the fact that St. Clair is an economist by training, however, his cost analysis relies heavily on national average statistics for streetcar, trolley-coach, and bus operations and thus fails to consider the importance of traffic volumes or densities on a route fully in determining the relative costs of different modes. Streetcars and trolley coaches require heavy investments in track and catenary so their average cost per passenger declines if these investments can be spread over large passenger volumes. Motor buses operating on public streets are subject to fewer scale or volume economies. Motor buses became more and more cost competitive during the 1930s, in part because transit patronage was dropping on routes (in response to increasing auto use). Since motor buses replaced streetcars on the lowest density lines first, it is extremely misleading for St. Clair to compare national average costs and profits for streetcars, which were increasingly concentrated on the remaining "cream" of the high-density routes, with those for buses, which were serving the most troubled low-density lines. St.

Clair's failure to recognize the importance of the streetcar's scale economies is all the more striking since it was widely acknowledged by the industry and other observers.

The discussions of both streetcar conversions and interstate highway lobbying also suffer from too few efforts to put the potential importance of the auto industry's actions in perspective. St. Clair does acknowledge at several points that many other forces contributed to the motorization of cities, such as rapid improvements in bus and auto technology in the 1930s and 1930s, the spread of paved roads, the development of low-density suburbs (where auto use was more convenient) and the growth in per capita incomes that made suburban living and auto ownership more affordable. Other than the chapter on cost analysis described above and another chapter outlining how shortsighted local government officials contributed to transit's decline, however, St. Clair spends the bulk of his book chronicling the history of GM's involvement with National City Lines and its lobbying efforts. Although he is careful never to say that the auto industry's actions were a key contributor to motorization, St. Clair's tone and the short shrift he gives alternative explanations strongly imply that the auto industry is the major culprit.

Disentangling the relative importance of the auto industry's streetcar conversions and lobbying from other forces would obviously be difficult. St. Clair might have made some attempts, however, by comparing the fortunes of transit companies that converted to motor buses early (under GM's encouragement) with comparable companies that were late or by examining whether urban auto ownership rates increased substantially after interstate projects were completed. Even if GM was trying to sabotage mass transit instead of monopolize bus supplier markets (which I doubt), a strong argument can be made that the other forces contributing to transit's troubles were so powerful that streetcar conversions hastened the industry's decline by only a few years at most. Similarly, the auto industry was not the only politically important force lobbying for the interstate system, and in any event most of the growth in per capita auto ownership rates occurred first in the 1920s and then again in the 1950s, before most of the interstate system was opened.

St. Clair's book is particularly disappointing when compared to other recently published volumes on the same topics. Paul Barrett and Mark Foster have both recently published books that give a richer and more balanced account of the forces that contributed to the growth of urban automobile use in the United States. Similarly, Mark Rose's recent book, *Interstate: Express Highway Politics, 1941-1956* (Lawrence, KA: Regents Press of Kansas, 1979), and Gary Schwartz's article, "Urban Freeways and the Interstate System" (*Southern California Law Review* 49(1976): 406-513), offer more insight into the variety of political forces that lead to the creation of an interstate highway system. St. Clair presents his views strongly and provocatively, but his book should be supplemented with one of the other volumes if the reader is to get a balanced perspective.

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Errata

Samuel Merrill III and Jack Nagel, "The Effect of Approval Balloting on Strategic Voting under Alternative Decision Rules" (June 1987, 509-524): On page 511, column 1, line 6 of the last paragraph should read "when one votes) as sincere if and only."

Beer, Samuel H. *Britain against Itself: The Political Contradictions of Collectivism*; King, Anthony, ed. *The British Prime Minister*; Krieger, Joel. *Reagan, Thatcher, and the Politics of Decline*; Studlar, Donley T., and Jerold L. Waltman, eds. *Dilemmas of Change in British Politics*. Reviewed by Gary Marx. (March 1987, 245-253): The reviewer's name should be spelled "Marks," not "Marx."

ARTICLES

INTIMIDATION AND THE SYMBOLIC USES OF TERROR IN THE USSR

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Theories of regime-society relations in Communist states stress the central role of coercion in maintaining political control. Based on a survey of Soviet emigrants, we examine whether Soviet citizens are deterred from nonconformity by the punitive actions of the KGB (individual deterrence), a perception of the KGB's coercive potential (general deterrence), or mistrust of other people. We find that few respondents were directly coerced by the KGB (and those who were had engaged in the most serious kinds of nonconformity); that those who had punitive contacts with the KGB in the past were not deterred from subsequent nonconformity; that the KGB's competent image was a general deterrent; and that trust in other people facilitated both nonconformist and compliant political activism. Those who came of political age under Khrushchev and Brezhnev were more likely to be involved in both kinds of activism than those who came of age under Stalin.

The feature of the Soviet political system that is most often said to distinguish it from industrial democracies is the use of state terror—physical coercion or the threat of it—to assure compliance with the regime and to remake the nature of man. In his characterization of the Soviet system under Stalin, Merle Fainsod wrote, "Terror is the linchpin of modern totalitarianism" (1961, 384). In their classic book, *Totalitarian Dictatorship and Autocracy*, Carl Friedrich and Zbigniew Brzezinski (1965) described a terrorist police as one of six key features of the "totalitarian syndrome." Totalitarian states are said to atomize society so that people become isolated and mistrustful of one another and hence unable to concert their efforts in organized political activity (cf. Kornhauser 1959). They fear the *stukach* (stool pigeon) and the possibility that even an

unguarded or innocent remark could be used against them.

The actual use of state-sponsored terror as an instrument of control has unquestionably decreased, however, since Stalin's time. The reduction is reflected not only in the decline in the use of physical force for political ends but also in reduced autonomy and authority of the secret police agency itself. Even though in the late 1970s the status of the KGB (Komitet gosudarstvennoi bezopasnosti, "Committee on State Security") was upgraded in visible ways and the number of dissidents arrested increased (Knight 1980; Reddaway 1983), overt mass terror is much less common now than in Stalin's time.

Whether a Soviet system with limited overt use of terror should be classified as something other than totalitarian has exercised the minds of many scholars.

Although few question that the Soviet state has reduced its overt use of force for political ends since Stalin's day, most recognize that the main instruments of political control remain in place. For example, Bialer (1986) has argued that there are three main reasons why the Soviet system survived the abolition of terror: the inhibiting effect of the memory of the Great Terror among the generations brought up under Stalin; the maintenance of a political police that uses more measured and predictable, but nonetheless effective, measures of control; and the use of additional means of nurturing mass support, such as material incentives.

This suggests that it is the style of coercion that has changed in the Soviet Union, not the essential commitment to use it to enforce political orthodoxy. The argument is not simply that the Communist party has cultivated other means of generating mass compliance, such as material incentives, but that a mature coercive regime does not need to engage in constant harassment of every person who strays from the orthodox line. It can tolerate unorthodox thoughts and a certain amount of unorthodox behavior, while resorting to coercion only when an unorthodox activity intensifies or challenges the authority of the regime.

More important, an efficient system of controls can rely on a perception of its coercive potential to inhibit deviation from prescribed political norms. As Jeremy Azrael has observed, in the post-Stalin era more of the activities of the secret police are "designed to have a 'prophylactic' instead of a 'punitive' effect" (1962, 10). From this perspective, it is important that the state be perceived as having the capability and will to uncover and to act against dissident or unorthodox activity, whether or not it actually does so in every instance.¹

An equally important instrument of collective coercion against the individual according to the totalitarian model is the

individual's lack of trust in others, a sense that risks lurk not only in the overt activities of the agencies of coercion but also in one's most ordinary contacts with co-workers, bosses, friends, and even relatives. It is this set of perceptions—of the coercive potential of the political police and of other people—that we call the *intimidation factor*.

We use information provided by former Soviet citizens to study both perceptions of the KGB and interpersonal trust and to test propositions about the relation between these perceptions and political behavior. Did the experience of direct intimidation by the KGB deter the individual from engaging in subsequent unorthodox behavior? What is the KGB's image? Is the KGB held in higher regard than other Soviet institutions? How prevalent is social atomization? Were those who were less trusting of others likely to take fewer chances and to engage less in behavior that the regime considers unorthodox?

We focus on the KGB as the primary agency of coercion in the USSR. Other institutions, of course, also play a role in intimidation. We study the image of the KGB because it represents the most severe coercive agency. Those who are not intimidated by the secret police are unlikely to be intimidated by other, less coercive institutions.

The Soviet Interview Project General Survey

The data for this study come from the Soviet Interview Project (SIP) general survey of 2,793 former Soviet citizens. Most of the questions on the survey centered on the respondents' "last period of normal life" (LNP) in the USSR—the five years prior to the major disruption associated with their decision to emigrate. For most survey respondents, this was when Leonid Brezhnev was general secretary of

the Communist party and Yuri Andropov was the head of the KGB.² The respondents arrived in the United States between 1 January 1979 and 30 April 1982 and were between the ages of 21 and 70 on the date of their arrival. They were interviewed between March 1983 and January 1984.³

Can the results of the SIP general survey yield valid insights about mass politics in the USSR? We would not suggest that either the overall distributions or summary statistics from a survey of Soviet emigrants be generalized directly to the Soviet population, especially because the respondents come overwhelmingly from large cities and are (by definition) emigrants and because about 85% of them are Jewish by some definition.

Our concern, however, is with the pattern of relations among variables, such as the relation between people's image of the KGB and their compliance with the Soviet model of a politically mobilized citizen (cf. Zimmerman 1987). And our ability to generalize is limited to the Soviet adult population from large Soviet cities, the "referent Soviet population" (Anderson and Silver 1987a). Previous research using SIP data confirms that such findings do reflect Soviet reality (Bahry 1987). For instance, the connections between socioeconomic characteristics and political activity essentially match those reported in Soviet surveys. So, too, does the pattern of early recruitment into the Komsomol for different generations and social groups. Friedgut's (1979) study based on Soviet emigrants supports a similar conclusion.

In addition, previous research shows that most of the responses to questions on political attitudes and behavior are not sensitive to whether the respondents were Jews or to whether they had especially traumatic experiences associated with emigration. Thus, although one can never prove that all the patterns in the responses

reflect the attitudes and experiences of the referent Soviet population, the results are not sensitive to some of the obvious candidate sources of bias.⁴

Theoretical Background

Conceptual Insights from the Theory of Deterrence

The literature on deterrence, particularly as it has been applied to understanding law enforcement, provides a useful conceptual framework for understanding the effects one should expect from government coercion, though we do not wish to press the analogy between ordinary crime control and the activities of political police.⁵

When agencies of the state resort to force against people, their usual immediate objective is to suppress the activities themselves. By arresting or punishing people, they try also to affect their future behavior or that of others. First, they can *incapacitate* people—remove them from society—by detaining, imprisoning, exiling, or killing them. Second, they can seek *individual deterrence*, by retaliating against nonconformists and making them hesitant in the future to repeat the same or other undesirable actions or by inducing them to behave in more appropriate ways in future. Third, they can try to foster *general deterrence*, by making other people hesitant to become involved in undesirable activities. General deterrence depends on shaping peoples' perceptions of what could happen to them if they were to overstep the boundaries of acceptable behavior.

Individual Deterrence. The shift in the KGB's role in Soviet society from the 1930s to the present involves a shift from relying heavily on incapacitation to relying more on individual and general deterrence. The literature on law enforcement suggests that the individual is deterred

from straying because of both the certainty and the severity of punishment. In general, however, high certainty of punishment is a more effective deterrent than high severity of punishment. The logic, supported by some empirical evidence, is that increasing the penalty is not likely to deter deviant behavior unless people think they are likely to be caught (see Piliavin, et al. 1986).

By this same logic, the KGB does not need special tribunals and a vast archipelago of labor camps to be an effective political police. But it does need to make people believe that if they do step out of line, retribution will be nearly certain. This logic applies to both individual and general deterrence.

General Deterrence. For general deterrence to work, the agencies of coercion must appear to be effective and the punishment meted out must be visible, publicized. Or the memory of past activities of the agencies must be strong. A perception that some activity might be risky is only a deterrent if reprisal is very likely.

Evidence among SIP general-survey respondents supports this distinction. Often, people who were aware of the risks of engaging in unconventional behavior nonetheless did it. For example, 30% of the respondents reported that they read *samizdat* or *tamizdat* (unauthorized materials in the USSR);⁶ of those who did not read it, less than 1% refrained from doing so explicitly because it was too risky. On the other hand, of the 30% who did read *samizdat*, 84% thought it was risky to do so, and 40% of the latter classified it as very risky.

In fact, however, only 1 of the 280 respondents who claimed to have read *samizdat* during their LNP reported being punished for doing so. Thus, this form of unconventional behavior was not deterred by the perception of risk alone. Those who did not engage in it seldom volunteered its riskiness as a reason; they

were much more likely to mention that *samizdat* was not available or that they were not interested in it. At the same time, those who did engage in the behavior did so despite perceiving the potential hazards.

The relation between the perception of risk and type of *samizdat* that people read is instructive. Those who read literature about human rights and the regime's political abuses regarded the reading of *samizdat-tamizdat* as much riskier than those who read nonpolitical materials, even though punishment was rare in both cases. Ironically, politically oriented materials may send an unintended message that reinforces the image of the potency and ruthlessness of coercive agencies. Materials that chronicle state abuses remind readers of the state's power and the heavy costs to be paid by those who fail to comply. And articles and books critical of the vast majority of compliant citizens tacitly underscore the social isolation of political dissidents (Jahoda and Cook 1954).

Random Terror. Scholars writing on terror in the USSR have emphasized the importance of random terror as a method of totalitarian control. According to this idea, it is not the predictability and certainty of punishment that deters people from engaging in unconventional behavior, but instead its unpredictability and uncertainty. Brzezinski puts it thus: "It is this selective, unfathomable character of the purge which gives it its effectiveness" (1956, 28). Alexander Dallin and George Breslauer note that "it is this element of arbitrariness—or, from the vantage point of the citizen, the unpredictability—in the use of terror that is [terror's] distinguishing mark" (1970, 4).

We think the appearance of randomness or arbitrariness with respect to the *targets* of terror is most effective at given stages in the development of a regime, in what Dallin and Breslauer (1970) refer to

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as the *takeover* and *mobilization* stages. Random terror is especially relevant to a regime that is committed to undermining the established social order. As Barrington Moore has observed, "Capricious or arbitrary use of terror undermines the essential basis of social organization" (1954, 170).

At a later stage of development of the regime, however, not only are other mechanisms of achieving compliance available, but utter arbitrariness in the application of coercion punishes people who are supportive of the system. Furthermore, it is not efficient for the police either to try to achieve 100% compliance or to punish every transgressor of official norms.

An analogous argument has been presented in the context of the strategy of two-person games. Assuming that agencies of coercion must be selective in the application of coercion, it is better to be unpredictable than not.⁷ In this way, it is more difficult for those who wish to violate the established norms to work the system to their own advantage, like knowing when the cop on the beat makes his rounds. Under uncertainty, the prospective wrongdoer will be deterred from acting because the risk of punishment becomes incalculable.

The usual objective of the person who threatens coercion is to modify the behavior of the other person in a specific way, not to terrorize him into total inaction. For this reason, enforcement should not be random or unpredictable with respect to the types of behavior that are sanctioned. That is, which individuals will be caught and punished may not be highly predictable; but what kinds of activities will be punished should be more certain.

Following this line of argument, one would not expect to find certain and severe punishment of all transgressors of official Soviet political norms. Nor would the lack of universal and harsh enforcement of official norms of behavior be a

sign of a lack of capability or will of the political elite. But one would expect actions that are more threatening to the regime to be more consistently punished than those that are not. And leaders of unsanctioned activities should be more likely to be punished than those who are only participants.

Social Atomization. A related component of deterrence that has been especially emphasized in theories of totalitarian control is the deliberate undermining of social bonds among individuals. Society itself thereby becomes an instrument of coercion: the memory of mass terror, the elimination of autonomous intermediary groups between state and individual, and the continued reliance on informers breed an atmosphere of social intimidation that undermines any collective activity not officially sanctioned by the state. Moore outlined the process well:

The regime deliberately seeks to sow suspicion among the population, which to a marked extent results in the breakup of friendship groupings, in the work situation and elsewhere, and the isolation of the individual. . . . Terror ultimately destroys the network of stable expectations concerning what other people will do that lie at the core of any set of organized human relationships. Dissent may thus be deterred not so much by the direct threat of punishment by the KGB (although it looms in the background) but by a generalized fear or mistrust of others, a lack of faith in people (1954, 158, 164).

Thus, based on the scholarly literature on deterrence and on terror in totalitarian systems, there is reason to ask both when and how coercion comes into play, and whether the actual or expected application of negative sanctions works as either an individual or a general deterrent. How likely is it that people will in fact be punished? And does punishment or the anticipation of punishment actually deter people from political nonconformity?

Results

Is Coercion Random?

Previous research based on SIP general-survey data has shown that only a small proportion of the respondents engaged in overt political nonconformity.⁸ By and large, they were quite conventional, judging by both the standards established by the Soviet government and the patterns of political participation found in other developed countries.

Because of the small number of respondents to the survey who engaged in specific unconventional political activities, we must be cautious about interpreting the patterns of regime response to each kind of noncompliance. In addition, we do not have information about the frequency with which individuals engaged in each of the behaviors. Usually, we know *whether* they engaged in different types of behavior in their LNP, but not how often.

We have better evidence about the level of involvement of the individual in various kinds of unconventional activity, that is, whether the respondents were leaders in the activity or were only participants in it. And for those activities, we can assess whether involvement led to punishment by the authorities.

As shown in the top panel of Table 1, those who were leaders of unconventional activities were consistently more likely to be punished for their activity than those who were merely participants. Table 2 shows that, of all nonconformist leaders, 50% were punished.⁹ The probability that they were punished increased with the variety of activities that they led. In contrast, of all those who were participants (but not leaders) in the same kinds of activities, only 7% were punished, though the proportion penalized also increased with the number of different types of activity in which people participated. Reprisals were especially likely for those who engaged in more serious,

public threats to the regime, for example, strikes and overt protests.

From this evidence, one would conclude that punitive action by Soviet authorities is very predictable.¹⁰ The question remains, however, whether those who were *not* involved in overt unconventional behavior during the LNP were also targets of the KGB. If the use of coercion were random with respect to the kinds of activities in which people engaged, the secret police would have intervened directly in the lives of many people who did not knowingly engage in any overt unconventional behavior.

Only 2% ($N = 58$) of the SIP general-survey respondents reported such contact with the KGB during their LNP.¹¹ One-fourth of this small group were asked by the KGB to help it conduct *its own* business: the KGB solicited the respondents as witnesses in an investigation or asked them to provide information about other people. Other encounters with the KGB related to the *respondent's* activities: 24% concerned contact with foreigners; 17%, illegal economic activities; 12%, other restrictions on the respondents' freedom of speech or movement (e.g., the respondents' reading of *samizdat*, complaining to the government, "saying the wrong thing," visiting a place where they were forbidden to go); 5%, ethnic reasons or engaging in religious observances; and 17%, unidentified reasons.

Direct contact with the KGB was therefore minimal for those who refrained from unconventional activities in the LNP.¹² Not surprisingly, the KGB was far more likely to intervene in the lives of those who publicly threatened the established political order. For example, out of approximately 20% of all respondents who reported not voting during their LNP, the only ones punished for it were individuals who also took part in overt unconventional activity.

A similar pattern holds with respect to

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regime responses to individual demands and complaints (see Table 1). The vast majority of citizen-initiated contacts with Party and state agencies, mostly concerned with pocketbook issues and public services, evoked either a positive response

or no response from the agency contacted. Only a handful led to reprisals, and these were for contacts that were more overtly political, such as complaints about the state itself or its handling of citizen rights.

**Table 1. Punishment by Authorities for Political Activities in the LNP,
by Type of Activity**

Activities	Percentage Punished ^a	Base Number of Cases
Unconventional political activities		
Unofficial study group		
Participant	4.2	166
Leader	18.8	16
Distribution of <i>samizdat</i>		
Participant	8.8	102
Leader	23.1	13
Overt protest		
Participant	23.1	26
Leader	100.0	5
Strike		
Participant	13.0	23
Leader	55.6	18
Other self-defined activity ^b		
Participant	16.7	6
Leader	64.3	14
Other unconventional activities		
Nonvoting		
Sometimes	1.6	506
Always	1.7	292
Attending religious services	1.1	888
Read <i>samizdat-tamizdat</i> ^c	.4	262
Citizen-initiated contact with authorities^d		
Contacted government or party official		
Material reason	2.4	405
Administrative, civil matter	2.8	69
Protest	28.0	25
Contacted media		
Material reason	5.1	59
Administrative, civil matter	.0	8
Protest	27.8	18

^a*Punishment* is any official action taken against the respondent for the particular activity the respondent reports.

^b"Other" unconventional activities are defined by respondent. The most frequently mentioned were teaching/study of Hebrew/Yiddish; involvement with dissident movement; and illegal economic activity.

^cThe question about reading of *samizdat* or *tamizdat* was asked only of a random one-third of the respondents.

^dThese items included citizen-initiated contacts of all types, and are recorded by the authors into the categories listed. Many such contacts evoked positive responses, and many evoked no response.

Table 2. Punishment by Authorities, by Intensity of Involvement in Politically Nonconformist Activities

Number of Activities	Participant		Leader	
	Percentage Punished ^a	Base Number of Cases	Percentage Punished ^a	Base Number of Cases
1	5.7	176	44.7	38
2	16.4	55	50.0	8
3	38.9	18	100.0	3
4	20.0	5	100.0	1
5	50.0	2	—	0
1 or more	7.2	256	50.0	50

Note: *Political nonconformity* includes participation during the last normal period in an unofficial study group, distribution of *samizdat*, overt protest, strike at work, or any other unsanctioned political activity as defined by the respondent.

^aIn order for a person to be *punished* for any of the activities defined here, he or she must have been either a participant or a leader. Hence, no individual in this tabulation can have engaged in 0 unsanctioned activities.

Individual Deterrence

Since most questions in the SIP general survey focused on events and attitudes during the last five years of a respondent's life in the USSR before it was disrupted by plans to emigrate, it is difficult to determine whether people who were punished for unconventional behavior were deterred from doing it again. Fortunately, we do have some critical additional information: whether respondents had been contacted by the KGB earlier in their lives (before the beginning of the LNP), and whether they had a prior history of family arrest (whether someone in their family was arrested before the respondent ever got into trouble for some activity during the LNP). We would expect that if individual deterrence comes into play, those with early contact or family arrest should be less involved in nonconformity.

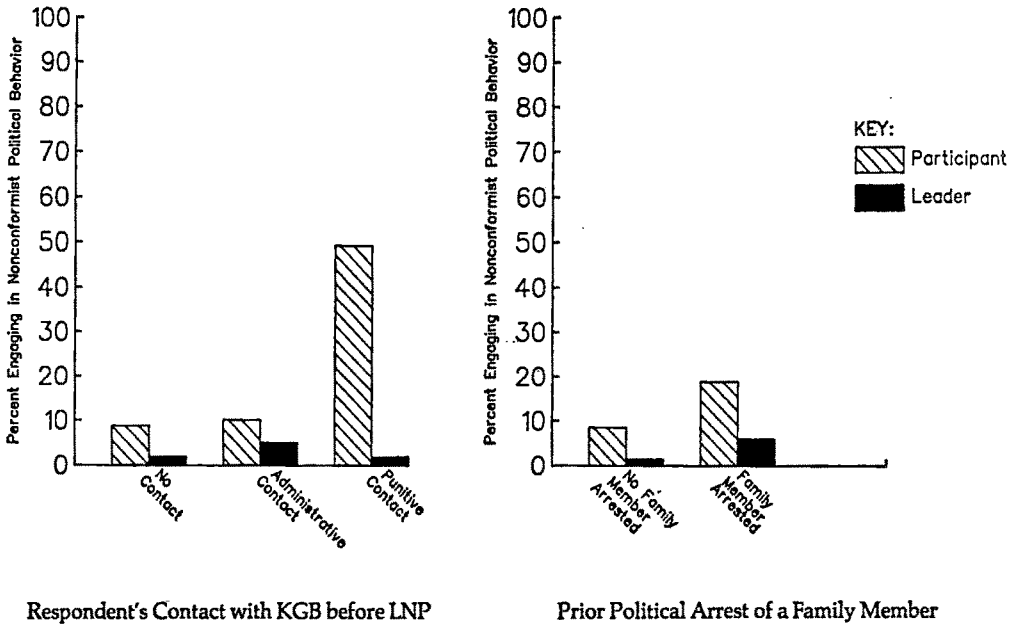
The data in Figure 1 reveal that such earlier experiences did not necessarily deter further involvement. Individuals penalized earlier by the KGB were almost four times as likely as other respondents to engage in unorthodox political activity during the LNP, although they were not more likely to be leaders.¹³ And those

who came from families that experienced a political arrest turned out to be twice as likely as others to engage in unconventional activity during the LNP. Evidence on the number of family members arrested provides additional support for this conclusion: the higher the number, the more likely the respondents themselves were to engage in unorthodox political activity (the data are not shown here).

Two explanations for this pattern seem plausible. First, whatever it is that impels people to engage in unorthodox political activity does not necessarily disappear once the person has been punished by the regime. Calling unorthodox political actors to account may be punitive but not rehabilitative or corrective.¹⁴ Second, once individuals are branded as troublemakers, their opportunities for political redemption may be limited. They face continuing extra scrutiny by persons in authority at work, by neighbors, and by social organizations. They may be isolated from and ostracized by others who do not want to be judged guilty by association. At the same time, they may receive social support from others who are similarly stigmatized by the authorities as political deviants, and perhaps

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Figure 1. Relation between Early KGB Contact and Political Nonconformity



from Western news media, intellectuals, or politicians.

This does not mean that being punished makes someone more likely than before to engage in unconventional political activity. But the balance of incentives and disincentives to be compliant or nonconformist shifts once the person is identified by the authorities as a nonconformist.

Perceptions of the KGB: Competence, Honesty, and Other Dimensions

The analysis to this point shows that individual deterrence does not necessarily work. For the majority of SIP respondents, however, who did not have direct contact with the KGB, the effect of the KGB on political behavior may depend more on the KGB's image of competence. As Moore has written, knowledge of how many people the regime has arrested is not the only factor shaping compliance

and nonconformity: "Particularly in the case of such a phenomenon as terror, popular beliefs about its impact are likely to be more influential in determining actual conduct than the objective facts themselves" (1954, 156).

Accordingly, we now examine two dimensions of the intimidation factor that may serve as a general deterrent on unorthodox behavior: perceptions of the competence of the KGB and of the trustworthiness of other people. A KGB that is perceived as competent is likely to be more of a general deterrent than one perceived as incompetent. People who do not trust one another are also more likely to be deterred from unorthodox behavior than people who have greater faith in other people. In the final section of the analysis, we will test for the effects of these perceptions on the incidence of unorthodox behavior.

Competence and Honesty. Judging from

answers to questions about the competence of leaders of eight key Soviet institutions, the KGB is viewed as highly capable: 57% of all respondents said that almost all or most of the KGB's leaders were competent (*kompetentnyi*) or did their jobs well (see Figure 2).¹⁵ The KGB ranked third in competence among the eight institutions. The highest ratings were given to the leaders of the Academy

of Sciences and of the military; 66% of all respondents stated that almost all or most of the leaders of these institutions were competent.

Industrial-enterprise managers were rated fourth, the Politburo fifth. The leaders of local political institutions—the *militsiia* (local police), the local party, and the local government council—were rated much lower. Thus, the responses to

Figure 2. Perceptions of Competency of Leaders in Major Soviet Institutions

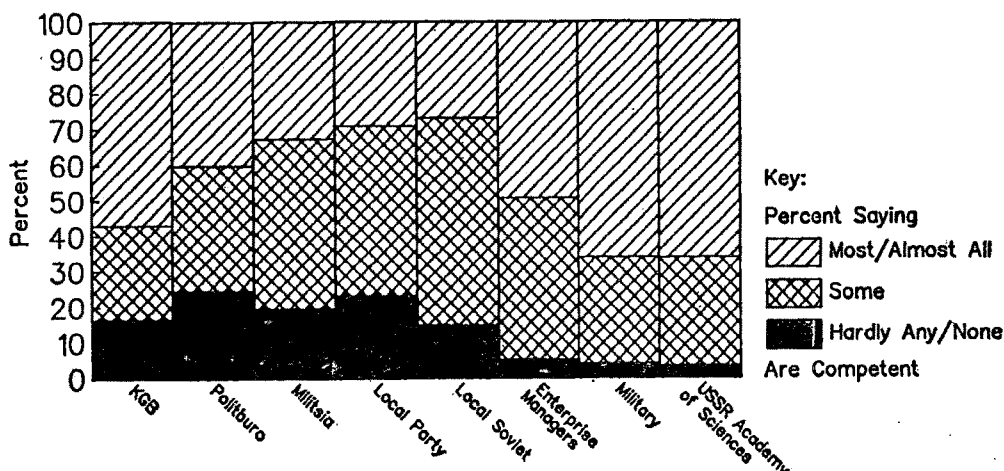
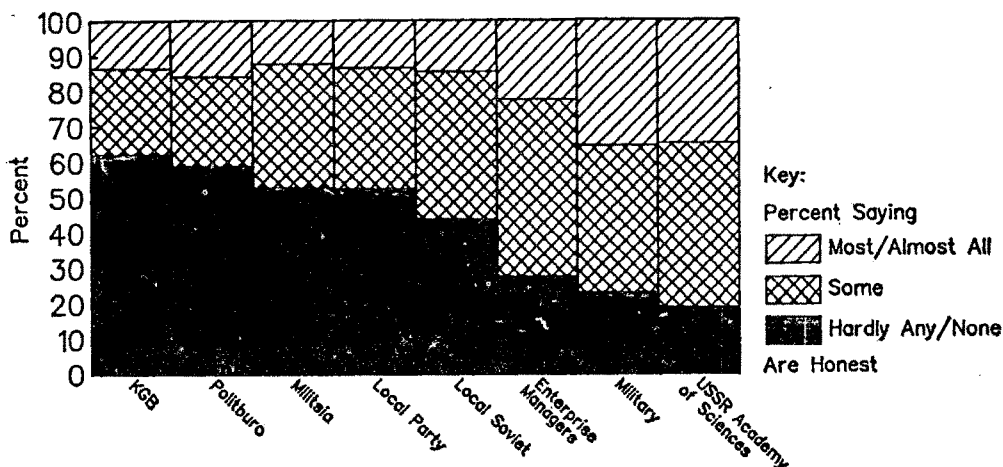


Figure 3. Perceptions of Honesty of Leaders in Major Soviet Institutions



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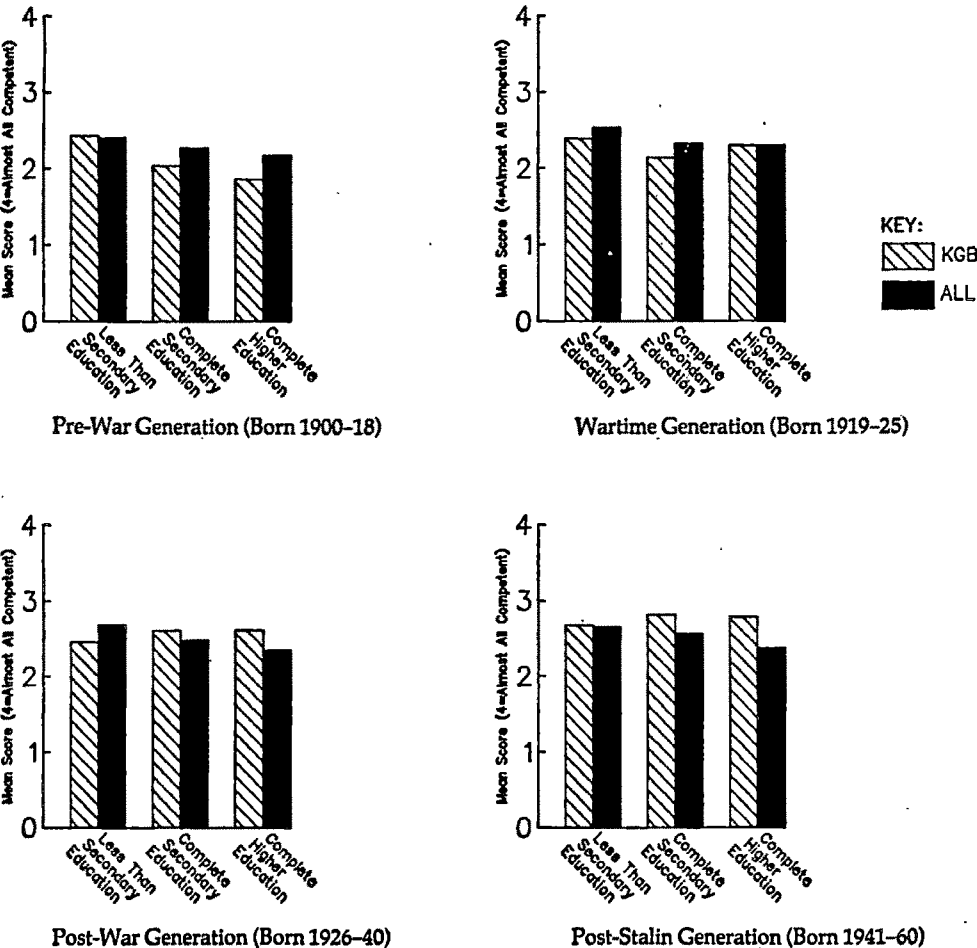
the questions on competence vary considerably, and within the range of variation, the KGB was rated quite highly.

It is possible that the answers to the questions about the competence of Soviet officials reflect a more general evaluation rather than an evaluation of competence per se. To determine whether this was so, we examined responses to questions about how honest or honorable (*chestnyi*) the leaders of the eight institutions were. If the responses to the questions about competence and honesty were substantially

different, then we could be more confident that the responses to the questions about competence are judgments of competence rather than of more general qualities of the institutional leaders.

When the questions related to honesty, the leaders of major Soviet institutions were judged much more negatively (compare Figures 2 and 3). The gap between the average competence and honesty ratings for each institution implies that the responses to the two sets of items do not derive from a single underlying

Figure 4. Perceived Competence of Leaders of the KGB and All Major Institutions, by Level of Education within Generations



dimension of evaluation.¹⁶ The leaders of the Soviet military and the Academy of Sciences were again the two most highly rated. Enterprise managers were third. Depending on what part of the distribution one focuses on, the leaders of the KGB rank either sixth or eighth—last—on the *honesty* dimension.

Thus, leaders of major Soviet institutions have a split image. They are viewed as generally capable, but not very honest. Even for the two agencies for which a large majority of the officials were judged to be competent, barely a third of the respondents judged them to be honest. The largest relative gaps between their image of competence and their image of honesty appeared for the Politburo, the KGB, and the *militsiia*. The smallest relative gaps appeared for leaders of the Academy of Sciences, the military, and industrial enterprises.

It is important to emphasize that neither dimension of evaluation should be interpreted as a measure of affect for the leaders of the institutions, particularly of the KGB. One does not admit that one actually likes a hangman when one acknowledges the efficiency with which he manipulates the rope.

In the remainder of the analysis, we examine factors related to the image of the competence of institutional leaders. Our main concern is to study perceptions of the capability or competence of the KGB, and to determine whether these perceptions are related to political non-conformity.

Determinants of Perceptions of Competence. To understand the factors that affect the evaluation of the secret police in particular, it is important to understand first what affects evaluations of authorities more generally. The basic reason for this is that respondents who evaluated the leaders of one institution as competent were also likely to evaluate leaders of the other seven institutions as competent.

Table A-3 (see Appendix) shows the correlations between the evaluations of the eight institutions, with the responses to the questions on competence transformed into numeric scores by assigning values to the response categories, ranging from zero (lowest evaluation) to four (highest evaluation).¹⁷ The correlations range from moderately to highly positive.

Initially, one might hypothesize that both the level of education and political generation will affect individual perceptions of leaders of major Soviet institutions. Previous research (Silver 1987) has shown that increases in education have tended to undermine support for two Soviet "regime norms," state control of the economy and collective or social control at the expense of individual rights.¹⁸ Also, younger respondents were less likely to support the norms than older ones.

The responses to the questions on competence of the authorities bear out only part of the pattern. As shown in Figure 4, the perceived competence of institutional leaders declines as the respondent's education increases. This is true for all eight institutions (see Table A-1), and within every generation. But generational effects move in the opposite direction. Younger cohorts rate the competence of Soviet leaders more highly than do older ones, even after adjusting for educational differences (see Figure 5 and Appendix Table A-2).

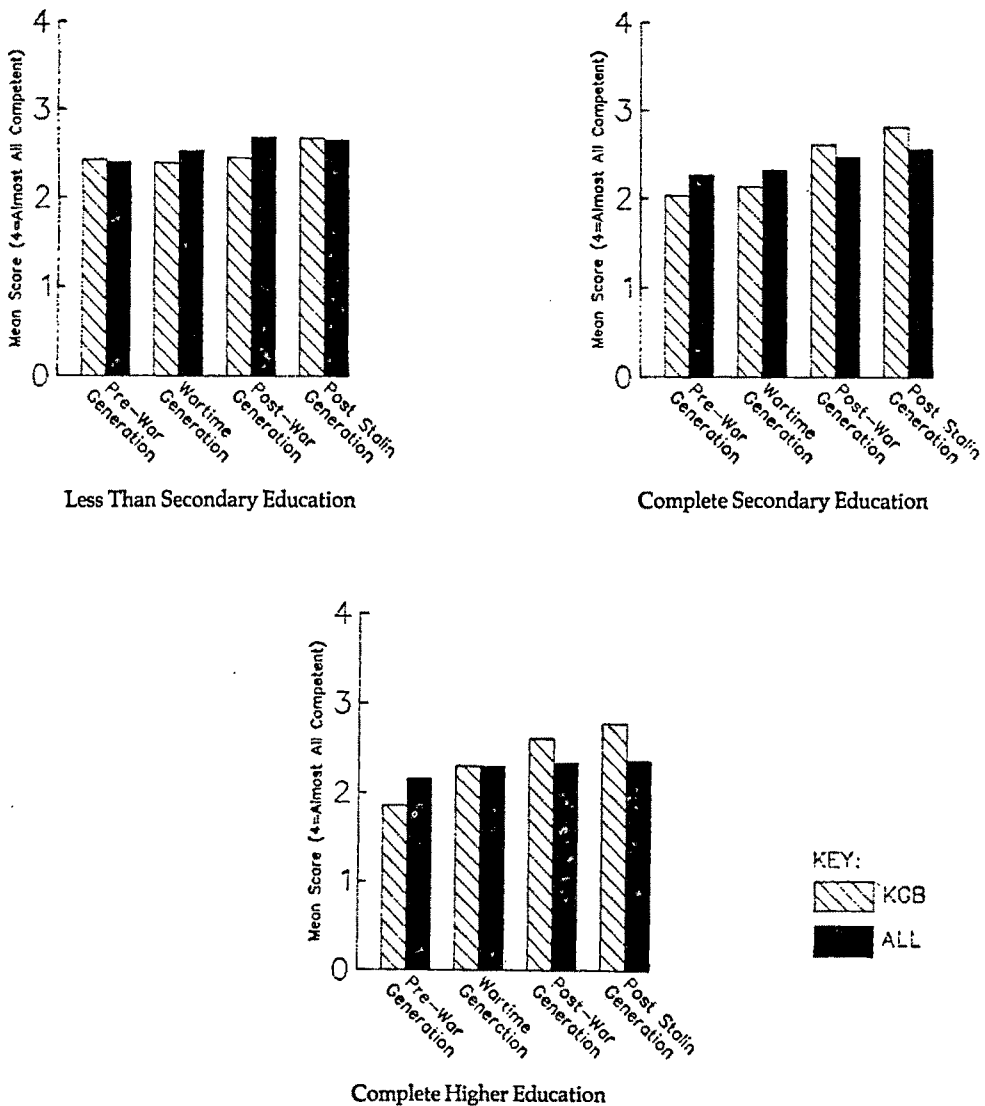
In light of these findings, it is important to examine whether other factors known to be related to support for regime norms are also related to the evaluation of the competence of Soviet institutional leaders. Based on earlier research, one might expect people who were more satisfied with their material status (housing, job, standard of living, provision of medical care) to evaluate the abilities of Soviet officials more highly (Silver 1987). Those more supportive of regime norms (established institutional practices) should also be more likely to evaluate the leaders of

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the institutions positively. On the other hand, one might expect persons who were more interested in political affairs to have a more negative evaluation of Soviet leaders. Finally, people who have experienced direct punitive contact with the KGB prior to their LNP ought to hold a more negative view of the authorities.

The results of a multiple regression analysis assessing the effects of all of these factors are shown in Table 3. The age categories are those used by Bahry (1987), and are based on Hough's (1980) classification of Soviet political generations. As expected, people's satisfaction with their material welfare is positively related to

Figure 5. Perceived Competence of Leaders of the KGB and All Major Institutions, by Generation within Levels of Education



their evaluation of the competence of the leaders of key Soviet institutions. In addition, the stronger people's support for regime norms, the more positive their image of institutional leaders, over and above the effects due to their satisfaction with material conditions. As shown in column 2, this relation holds up even after adjusting for the effects of the other factors.

Neither interest in politics, the number of privileges the respondent enjoyed, nor whether the respondent held a Party *nomenklatura* post¹⁹ showed a statistically significant relation to the evaluation of the institutional leaders.

Consistent with the data in Figure 4, persons with higher education held Soviet authorities in lower esteem than those with less education, even after other factors are taken into account. Consistent with the results in Figure 5, respondents in the postwar generation (born 1926–40) and the post-Stalin generation (born 1941–60) held a more positive image of the competence of Soviet authorities than did earlier generations.

The comparatively positive assessment of the competence of the authorities by the younger generations and the comparatively negative assessment by the older generations thus persist even after a large number of additional respondent characteristics are taken into account. It seems likely that this pattern of evaluations is a generational rather than an aging phenomenon. That is, it reflects the historical experiences of differing cohorts, not a typical pattern of change in political beliefs through the life cycle. One indication is that the cohort differences are especially sharp in the evaluation of the competence and honesty of the most politically coercive agency, the KGB. The prewar generation (born before 1919, reaching age 18 before 1937), whose formative political experiences occurred during the Great Terror, has a particularly

negative evaluation of the leaders of the KGB.

Table 3 also shows whether, after adjusting for other factors, people's direct experiences of being punished by the KGB affect their evaluations of the authorities. Respondents who were in families where another member was arrested for political reasons had a lower evaluation of the competence of officials. But those who themselves had previous punitive contact with the KGB (prior to the LNP) were not affected by this in their evaluation of the authorities (after adjusting for the effects of other factors).

In the latter case, it may be that most of the effect of people's experience with the KGB is captured by the measure of their subjective satisfaction. This is supported by additional analysis (not shown). Individuals who had been sanctioned by the KGB were much less satisfied with their material status in the LNP than those who did not have those experiences. Hence, via that mechanism, they had a lower estimation of the competence of the authorities in general.²⁰

How do subjective satisfaction and social background affect evaluations of the KGB in particular? To answer this question, one must take into account that over half (56%) of the evaluation of the competence of leaders of the KGB can be accounted for by people's evaluations of the competence of leaders of the other seven institutions.²¹ This means that if ratings of the KGB's competence were used directly as dependent variables in the equations for Table 3, the factors affecting evaluations of the KGB would work in much the same way as the factors affecting the average evaluation of all institutions. But the similarity in the results would be an artifact of the respondents' evaluation of the competence of institutional leaders in general.

To obtain a measure of the evaluation of the KGB that is distinct from the eval-

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Table 3. Regression of Average Competency Ratings on Subjective Material Satisfaction, Support for Regime Norms, Measures of Status, and Experience of Direct Coercion

Independent Variable	Average Competency ^a	
	b (1)	b (2)
Subjective material satisfaction ^b		
Average satisfaction	199**	202**
Support of regime norms ^c		
State control	.032**	.078**
Collective control	.082**	.037**
Interest in politics ^d		
Very interested		-.079
Somewhat interested		-.065
Slightly interested		-.054
Status measures		
Number of material privileges ^e		-.055
On Party <i>nomenklatura</i> ^f		-.097
Highest education ^g		
Complete secondary		-.064
Complete higher		-.095*
Generations ^h		
Wartime		.077
Postwar		.195**
Post-Stalin		.317**
Punitive experience before LNP ⁱ		
Punished by KGB		.045
Family member arrested		-.087*
Constant	2.014**	1.951**
Adjusted R ²	.111	.129
Number of cases	2,167	2,207

^aA single asterisk (*) indicates that the coefficient is significant at $p = .050$ using a one-tailed test. A double asterisk (**) indicates coefficient significant at $p = .050$ using a two-tailed test.

^b*Subjective material satisfaction* is a summated scale based on responses to questions concerning the respondent's satisfaction with five material conditions of life during his last normal period of life (LNP) in the USSR: housing, job, standard of living, medical care, and availability of goods. The scale ranges between 2 (most satisfied) and -2 (least satisfied), with 0 as a midpoint. For further details, see Silver 1987.

^cScores on the *state-private control* measure and the *collective-individual* measure range from 7 (greatest state-collective control) to 1 (greatest private-individual control). Each measure is based on responses to three questions. For details, see Silver 1987.

^dDummy variables are used to represent the respondents' stated degree of interest in politics. The question was "During your last normal period [of life in the Soviet Union], how interested were you in politics and public affairs—were you *very interested*, *somewhat interested*, *only slightly interested*, or *not at all interested*?" The omitted category is those who said they were *not at all interested*.

^eThis is a count of the number of privileges the respondent claimed to have received during the LNP, from a list of three: *access to a closed medical clinic*, *access to special shops*, and *use of an official car*. Scores range from 0 to 3.

^fDummy variable: 1 if the respondent held a job controlled by the Party *nomenklatura*, 0 if not.

^gDummy variables representing highest diploma or degree earned. The omitted category is *less than complete secondary* education.

^hDummy variables: *wartime*, born 1919–1925 (reached age 18 in 1937–1943); *postwar*, born 1926–1940 (reached age 18 in 1944–1958); *post-Stalin*, born 1941–1960 (reached age 18 in 1959–1978). The omitted category is persons born before 1919.

ⁱDummy variables: Did the respondent have an encounter with the KGB that led to some kind of negative sanction *before the beginning of the LNP*? Was any family member arrested for political reasons at some time prior to any arrest the respondent may have experienced, or prior to the LNP?

uation of the other agencies, we calculated a *KGB differential*: the difference between the respondents' actual (reported) evaluation of the competence of KGB leaders and their expected evaluation of KGB leaders given their evaluation of the competence of the leaders of the other seven institutions.²² Positive KGB differentials mean that the respondents rated the KGB as more competent than would be expected from their rating of other authorities; negative KGB differentials mean that the respondents rated the

KGB as less competent than would be expected.

Table 4 replicates the analysis in Table 3, but with the KGB differential for competence as the dependent variable. The question answered by this part of the analysis is, Do the explanatory variables have a distinctive effect on the evaluation of the competence of the KGB's leaders over and above their effect on the average evaluation of the leaders of all institutions? The answer depends on the variable considered.

Table 4. Regression of Differences between Actual and Expected Ratings of KGB Competence on Material Satisfaction, Support for Regime Norms, and Measures of Status and Experience of Direct Coercion

Independent Variable	Rating of Competence of KGB Minus Expected Rating ^a	
	b (1)	b (2)
Subjective material satisfaction		
Average satisfaction	-.057**	-.010
Support of regime norms		
State control	-.053**	-.036**
Collective control	-.031**	-.015
Interest in politics		
Very interested		.156**
Somewhat interested		-.031
Slightly interested		-.029
Status measures		
Number of material privileges		.025
On Party <i>nomenklatura</i>		-.078
Highest education		
Complete secondary		.025
Complete higher		.051
Generations		
Wartime		-.066
Postwar		.130*
Post-Stalin		.264**
Punitive experience before LNP		
Punished by KGB		.035
Family member arrested		.146**
Constant	.308**	-.095
Adjusted R ²	.024	.067
Number of cases	1,607	1,607

^aA single asterisk (*) indicates that the coefficient is significant at $p = .050$ using a one-tailed test. A double asterisk (**) indicates coefficient significant at $p = .050$ using a two-tailed test.

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One finding is that variation in people's subjective material satisfaction does not affect the size of the KGB differential. The KGB neither derives any distinctive benefit from increases in material satisfaction nor suffers any distinctive loss in reputation from decreases in people's material satisfaction.

In contrast, support for regime norms, particularly the norm of state control, is related differently to the evaluation of the KGB than it is to the evaluation of other institutional leaders. Persons more in favor of state control of the economy (state ownership of industry, state control of agriculture, state provision of medical care) give lower ratings to the KGB than to other institutional leaders, even after adjusting for the effects of age or generational differences.

Given that a high estimate of the competence of Soviet leaders is correlated with greater support for state control of the economy (see Table 3), the results in Table 4 can also be interpreted as showing that persons who have relatively positive evaluations of institutional leaders *other than the KGB* are more likely to favor state control. Additional statistical tests (not shown) sustain this interpretation: support for regime norms is directly related to the evaluation of the competence and honesty of institutional leaders as a whole (excluding the KGB) but inversely related to evaluations of the competence of the KGB in particular. Hence, the more competent or intimidating the KGB appears, the lower the support for regime norms.²³

Also noteworthy are the effects of early punitive experience with the KGB. Respondents who were punished for unorthodox political activity did not have a measurably higher or lower estimate of the KGB's competence than those who were not punished. But those who had family members arrested had higher estimates of the KGB's competence (net of their estimate of the competence of other

institutions) than those who did not.

These results do not present strong evidence that punitive contact with the KGB affects people's evaluation of the KGB. However, it should be recalled that there is a link between people's punitive experience and their subjective material satisfaction. Those who were punished for political reasons were less satisfied with their material welfare than those who were not, and lower material satisfaction is linked to more negative evaluations of the authorities in general.

Differences by generation also stand out in Table 4. Once again, the KGB receives a much higher rating on both competence and honesty by the postwar generation and the post-Stalin generation than by the prewar and wartime (born 1919-25) generations. It is especially notable that the generational differences in the evaluation of the competence of the KGB are so sharp even after we control for the effects of generalized attitudes towards institutional leaders and of several other factors. Not only are the younger generations likely to rate the authorities in general more highly than the older generations, but they are especially likely to rate the KGB more highly.

These results are consistent with an earlier finding of a lack of historical sense among younger respondents (Bahry 1987). A significant minority of the young, particularly the less educated, were likely to report that the KGB was more powerful in Brezhnev's time than in Stalin's.

Avoiding Trouble with the KGB. The SIP general survey also included questions designed to tap perceptions of the randomness of the KGB's actions and the pervasiveness of its network of informers. The questions are given in the notes to Table 5. The answers are instructive.

Almost half of all respondents stated that it was "easy or very easy" to avoid

**Table 5. How Easy Is It To Avoid Trouble with KGB?
How Easy Is It to Tell Who Is an Informer?
(by Generation within Levels of Education)**

Generation within Levels of Education	Respondents Who Said It Was Very Difficult or Somewhat Difficult	
	Percentage	Base Number of Cases
To avoid trouble with KGB		
Less than complete secondary		
Prewar generation	60.8	120
Wartime generation	54.4	68
Postwar generation	58.6	99
Post-Stalin generation	56.5	23
Complete secondary		
Prewar generation	64.8	108
Wartime generation	61.6	99
Postwar generation	49.8	247
Post-Stalin generation	52.6	470
Complete higher		
Prewar generation	66.0	53
Wartime generation	51.1	45
Postwar generation	43.4	318
Post-Stalin generation	34.7	398
To tell who is an informer		
Less than complete secondary		
Prewar generation	81.1	122
Wartime generation	77.6	67
Postwar generation	79.8	94
Post-Stalin generation	78.6	28
Complete secondary		
Prewar generation	92.2	115
Wartime generation	87.9	107
Postwar generation	83.1	267
Post-Stalin generation	85.8	471
Complete higher		
Prewar generation	88.3	60
Wartime generation	94.6	56
Postwar generation	85.6	361
Post-Stalin generation	85.5	421

Note: The two questions that were asked were: (1) "Some people say that it is easy for most Soviet citizens to avoid trouble with the KGB. Others say it is difficult to avoid trouble with the KGB. What did you think during your last normal period [of life in the Soviet Union]—was it *very easy* to avoid trouble with the KGB, *fairly easy*, *fairly difficult*, or *very difficult*?" (2) "Some people say that it is easy to tell who is a KGB informer or undercover KGB agent among one's coworkers, fellow students, or neighbors. Others say it is very difficult to know for sure. During your last normal period, did you think it was *very easy* to tell, *somewhat easy*, *somewhat difficult*, or *very difficult*?"

trouble with the secret police, hence that the actions of the KGB were predictable. But a much larger proportion—close to 80%—felt that it was somewhat difficult or very difficult to tell who might be a KGB informer among co-workers, fellow students, or neighbors.

The answers to these two questions taken together suggest yet another split image, between an organization with eyes and ears everywhere and one that is predictable and thus avoidable. The gap is especially pronounced for the younger generations and the highly educated. As Table 5 shows, virtually all the respondents agreed that it was difficult to know who was an informer or agent. Even so, the college-educated among the postwar and post-Stalin generations stand out as much more confident about the possibility of staying out of political trouble.

Further analysis (not shown) reveals that the respondents' sense of being able to calculate the odds, to avoid the secret police, is correlated with a more general sense of trust in other people. The lack of avoidance ability may thus be part of a more general syndrome of social atomization. On the other hand, the capabilities of the KGB to enforce social orthodoxy seem to be captured in the notion of the KGB's competence, and that is what we rely on as a measure of general deterrence in the analysis below.

Social Atomization

To understand how the intimidation factor might work to deter unorthodox political behavior, it is just as important to examine perceptions of other people as it is to examine perceptions of the KGB. To measure atomization, we rely on two items in the SIP questionnaire taken from Morris Rosenberg's (1956, 1957) widely used "misanthropy," or *faith-in-people*, scale. These questions were asked of a random one-third of the respondents. We combine the answers to the items to

derive our measure of *faith in people*, which ranges in value from zero (least faith in people) to three (most faith in people).²⁴

Although we hypothesize that those less trusting in other people will be less likely to become involved in unorthodox political activity, faith in people should be positively related to both unconventional *and* conventional political activism. This would be consistent with the findings in Almond and Verba's (1963) Five Nation Study, in which faith in people was important in accounting for cross-national differences in people's sense of political competence. Thus, trust of others may be an important political resource that facilitates participation in both unconventional and conventional political activity.

Table 6 shows that, within political generations, there is very little difference in faith in people across educational levels. In contrast, generational differences are very sharp. Within each educational level, those who came of political age before or during World War II had much less faith in people than those who came of political age after the war.

The low correlation between education and faith in people among SIP respondents contrasts with Almond and Verba's (1963) finding that trust in others increased with education in the five nations that they studied. The pattern among SIP respondents indicates that social atomization in the Soviet Union is not confined to persons of a certain social status. Instead, the level of interpersonal trust varies much more systematically by generation than by social status.

The evidence suggests for the older generation something of a "burnt-finger-tips" phenomenon, a wariness about letting down one's guard; while the younger generations, which came of political age in a period when the overt use of terror was much less common, were less wary of trusting others. Thus, the atomization and isolation of people from one another

Table 6. Faith in People, by Education and Generation

Education and Generation	Mean	Standard Deviation	Number of Cases
Level of Education within generations			
Prewar			
Less than complete secondary	.91	1.11	53
Complete secondary	.81	1.06	52
Complete higher	.82	.83	22
Wartime			
Less than complete secondary	.96	1.11	29
Complete secondary	1.00	.99	45
Complete higher	.83	1.13	18
Postwar			
Less than complete secondary	1.46	1.14	58
Complete secondary	1.18	1.12	107
Complete higher	1.24	1.08	133
Post-Stalin			
Less than complete secondary	1.50	1.17	15
Complete secondary	1.22	1.13	187
Complete higher	1.31	1.84	147
Generation within levels of education			
Less than complete secondary			
Prewar	.91	1.11	53
Wartime	.96	1.11	29
Postwar	1.46	1.14	58
Post-Stalin	1.50	1.17	15
Complete secondary			
Prewar	.81	1.06	52
Wartime	1.00	.99	45
Postwar	1.18	1.12	107
Post-Stalin	1.22	1.13	187
Complete higher			
Prewar	.82	.83	22
Wartime	.83	1.13	18
Postwar	1.24	1.08	133
Post-Stalin	1.31	1.84	147

Note: The two items used to construct the measure were (1) "Some people we've talked to say that most people can be trusted. Others say you can't be too careful in your dealings with people. How did you feel about that during your last normal period [of life in the Soviet Union]?" Answers coded: *Most people can be trusted*, *You can't be too careful*, and *It depends* (if volunteered). (2) "Speaking generally, would you have said during your last normal period that most people are more inclined to help others, or more inclined to look out for themselves?" Answers coded: *more inclined to help others*, *more inclined to look out for themselves*, and *it depends* (if volunteered).

Scores were derived by assigning a 3 to the answers *Most people can be trusted* and *more inclined to help others*, 1.5 to *It Depends*, and 0 to *You can't be too careful* and *more inclined to look out for themselves*, then averaging the values of each item. If only one item was answered with a valid value, only that item is used to compute the score.

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appears to have been more or less indelibly impressed on the older generations but not firmly impressed on the younger ones.

This interpretation is supported by other evidence in the SIP general survey. The respondents were asked whether during their last normal period of life in the USSR they could have criticized an official (*ofitsial'noe litso*): to *nobody, members of their family, friends, other relatives, coworkers or fellow students, or their boss or teacher*.²⁵ While few respondents of any generation said they could have criticized an official to their boss or teacher, among respondents in the prewar and wartime generations, 18% said they could not have talked to anyone about this issue, while only 9% of those in the postwar generation and 4% of those in the post-Stalin generation said they could not have talked to anyone about it.

Of those who felt that they could criticize an official to someone, 44% of those in the prewar and wartime generations would extend the circle of communication only as far as their immediate family; while 31% of those in the postwar generation and only 22% of those in the post-Stalin generation would limit the criticism to their immediate family. This generational pattern holds up even after adjusting for differences due to education.

The differences in the level of faith in people may explain in part why the younger generations are more inclined to become involved in unorthodox political activity than the older ones. Being able to count on the support of others or, at least, relative freedom from worry about their doing harm to you, is likely to facilitate greater political involvement.

The Intimidation Factor and Political Activism

Our aim in the last part of this study is to determine whether intimidation served as a general deterrent on nonconformist

behavior. We also explore the impact of intimidation on *compliant* political activism, to determine whether social atomization or fear of the authorities causes people to be more active in conventional participation and whether some of the sources of nonconformist activity also give impetus to compliant acts.

Measuring Activism: To measure the two dimensions of political activism, we constructed scales that capture both the level and intensity of each person's involvement.²⁶ For compliant activism we devised a summated scale based on the number of types of communal groups to which the respondent belonged in the LNP: housing, repair, or sanitary commissions; commissions at work or in local soviet or Party organizations; membership in the citizens' militia (*druzhina*) or comrades' courts; parents' commissions; the Komsomol; and other communal organizations.²⁷

This measure is weighted by the intensity of involvement in each type of group—whether the respondent was a leader, attended regularly, attended occasionally, or belonged but never attended.²⁸ About half the respondents engaged in some such activity; the average score for the respondents as a whole is 2.1 out of a possible 28, with individual scores ranging from 0 to 28 and a standard deviation of 2.96.

Our measure of nonconformist activism follows the same logic. It is constructed by totaling the number of different types of nonconformist activity in which the respondent engaged during the LNP, from unsanctioned study groups to distributing *samizdat*, engaging in protests, and strikes. It also includes a count of other unconventional acts as defined by the respondent; and it counts similar behaviors reported in response to questions about contacts with the KGB during the respondent's LNP. Ten percent of all respondents participated in some form of

nonconformist activity as defined here; and 2% more were leaders. Although actual individual scores range from 0 to 16, the average score for the respondents as a whole is very low—.41, with a standard deviation of 1.44.²⁹

Hypotheses. We conceptualize the impact of intimidation—fear of the KGB and of other people—on unconventional political behavior by regarding nonconformity as a result of three types of variables: (1) those that *motivate* the expression of political disaffection, (2) those that *facilitate* its public manifestation, and (3) those that *inhibit* its manifestation. This approach is similar to that proposed by Gurr (1968) for explaining civil strife. Gurr's model posits that there are sources of dissatisfaction (relative deprivation) that motivate people to engage in civil strife, but whether or not this engagement occurs is mediated by the factors of state coercion, legitimacy, institutionalization, and facilitation.

As indicators of the motivation for expressing disaffection, we include the respondents' satisfaction with their material quality of life during their LNP, and the respondents' support for established regime norms. Unconventional activists should be less satisfied with their material quality of life, established regime norms, or both.

Certain political resources should facilitate nonconformity. Education, for example, should provide the conceptual skills necessary to visualize alternatives and to manipulate the system (Moore 1954). Increasing education has been shown already to be related to decreasing support among SIP respondents for established institutional practices (Silver 1987). Interest in politics can also be viewed as a resource, one that prompts the individual to seek out more information and thus to weigh the regime's message against other (foreign and domestic) sources. Interest can thus help to reduce the ambiguity sur-

rounding public issues, and the less the ambiguity, the greater the likelihood of questioning the established order (Jahoda and Cook 1954).

Earlier research has shown that the generations that came of age after Stalin were both the ones most likely to engage in compliant activity and the ones most likely to engage in nonconformist activity (Bahry 1987). The young were also more mobilized in the conventional sense (Zimmerman 1987). These higher levels of both types of activism are a product of the renewal of mass organizations after Stalin and of changes in political atmosphere that prompted the growth of dissident movements after Khrushchev. We should expect to find similar generational differences in the level of political involvement in the analysis here.

A person's status may be important as well: those with more material privileges and a job on the Party's *nomenklatura* are likely to have connections and thus a degree of insulation from reprisals, at least for the more common brands of unconventional behavior. On the other hand, persons whose jobs are controlled by the Party's *nomenklatura* may also be more immediately subject to discipline for transgressions against official standards of behavior.

The intimidation factor should tend to inhibit overt unconventional activity. Individuals with a high regard for the capabilities of the secret police and those with low faith in other people should be less likely than others of the same age, education, and degree of disaffection to overstep the bounds of acceptable political behavior.³⁰

Nonconformist Activism. Column 1 in Table 7 shows the relation between nonconformist activism and the hypothesized motivational factors.³¹ As expected, people less supportive of regime norms (particularly state control of the economy) were more likely to be nonconformists.

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Table 7. Regression of Nonconformist and Compliant Activism on Subjective Satisfaction, Support for Regime Norms, Atomization, and Perception of Coercion

Independent Variable	Nonconformist Activism			Compliant Activism
	b (1)	b (2)	b (3)	b (4)
Subjective material satisfaction				
Average satisfaction	-.087**	-.095**	-.066**	-.026
Support of regime norms ^a				
State control	-.225**	-.233**	-.163**	.061
Collective control	-.008	-.009	.039	.154**
Atomization-coercion				
Faith in people ^b		.062**	.039**	.060*
KGB highly competent ^d		-.039	-.067*	-.011
Interest in politics				
Very interested			.214**	.221*
Somewhat interested			-.005	.112
Slightly interested			-.016	.063
Status measures				
Number of material privileges			.140**	.165**
On party <i>nomenklatura</i>			-.178**	.510**
Highest education				
Complete secondary			-.057	.043
Complete higher			.053	.204*
Generations				
Wartime			.044	.200
Postwar			.110*	.306**
Post-Stalin			.196**	.651**
Punitive experience before LNP				
Punished by KGB			.301**	-.436*
Family member arrested			.065	-.064
Constant	.479**	.439**	.157	-.119
Adjusted R ²	.075	.090	.179	.146
Number of cases	612	612	612	612

Note: Logarithms of the dependent variables are used. A single asterisk (*) indicates that the coefficient is significant at $p = .050$ using a one-tailed test. A double asterisk (**) indicates coefficient significant at $p = .050$ using a two-tailed test.

^aThe natural log logarithm of the values of state-private and collective-individual control was used in the equations for this table.

^bScores range from 3 (most faith in people) to 0 (least faith in people).

^dDummy variable: 1 if respondent said that *most* or *almost all* of the leaders of the KGB were competent, 0 if not.

Columns 2 and 3 add in the effects of atomization, coercion, and the other variables hypothesized to facilitate or inhibit the manifestation of nonconformity. As the atomization thesis would predict, the greater people's interpersonal trust (faith in people), the more likely they were to be nonconformist. Similarly, persons who perceived the KGB to be highly competent were less likely to engage in unorthodox activity. Thus, both components of the intimidation factor appear to operate in the expected direction: social atomization and perceptions of the KGB's competence serve to deter would-be nonconformists. At the same time, consistent with our earlier findings concerning *individual* deterrence, prior punishment by the KGB is associated with an increased likelihood that the respondents were nonconformist in their LNP.

Persons who were more interested in political affairs were more likely to be unconventional activists, even after adjusting for the effects of education, material satisfaction, and support for the regime. At the same time, interest in politics is also positively associated with *compliant* activism (see column 4). Thus, those who are interested in politics are likely to be motivated to some sort of action whatever their political orientation.

Having more material privileges is also positively related to nonconformity. One explanation for this may be that persons with access to privileges benefit from greater *protektsiia* (connections) with people in high places, and hence risk less by their involvement in nonconformist activities.³² Another explanation may be that the privileged among the SIP respondents more often were in the arts and culture and not only had more temptations and opportunities for engaging in unconventional activities but also were more attracted to them. A third explanation may be that having access to privileges is linked to greater incentives to become involved in political affairs, both

nonconformist and conformist. That there is a positive relation between access to privileges and participation in compliant activism (column 4) is consistent with this interpretation.

Persons who held important official positions that require Party approval were less likely to be involved in nonconformist activity and more likely to be compliant activists than those who did not hold such positions (compare columns 3 and 4). Thus, they may have been inhibited by Party control from engaging in unconventional behavior, whatever their personal sense of material satisfaction or level of support for regime norms.

As expected, persons in the postwar and post-Stalin generations were more involved in political nonconformity, even after we adjust for their more critical views of established institutional practices, their lower levels of satisfaction (higher aspirations) concerning material conditions, their greater sense of the capabilities of the leaders of the KGB, and their higher levels of interpersonal trust. In other words, the relation between generation and nonconformist activism holds up even after adjusting for the effects of motivational factors, sense of intimidation, and degree of faith in people.

One could, of course, maintain that young people are inclined toward nonconformity, and that progression through the life cycle fosters greater compliance as people develop a greater stake in society in connection with forming their own family and developing their careers. Evidence in the survey challenges the life-cycle interpretation, however. Nonconformity did not diminish evenly with age among the postwar and post-Stalin generations (see Bahry 1987). It is also important to keep in mind that the "younger generations" in the survey do not consist of "very young people." Respondents in the postwar generation had a median age of 41 at the midpoint of their LNP; and respondents in the post-Stalin generation

had a median age of 28 at the midpoint of their LNP.

The younger generations were also more likely than the older generations to be involved in *compliant* political organizations. Hence, the activism of the younger cohorts appears to reflect special formative experiences and commitments of those generations that cannot be readily reduced to a set of explicitly measured motivational and ideological factors.

Compliant Activism. The results in column 4 indicate that the motivation for compliant activism derives from something other than dissatisfaction with the material quality of life. Nor is it an expression of satisfaction with the established political order. That those with higher education, as well as those who hold Party *nomenklatura* positions, were more likely to be compliant activists implies that they were responding to the expectation for persons of their status to become involved in public organizations and *obshchestvennaia rabota* (work for society). Hence, such activism may be less a reflection of individual conviction and more a calculated conformity, a formal adherence to official norms of behavior intended to assure physical survival or to secure career advancement (for a similar interpretation, see Shtromas 1984).

Comparison of the determinants of nonconformist and compliant activism reveals that some factors both motivate and give a direction to political activism—toward conformity or nonconformity. These include degree of satisfaction with the material quality of life, level of support for regime norms, and whether a person holds a job that is under the control of the Party's *nomenklatura*. Other factors stimulate involvement in both types of activism: faith in people, interest in politics, access to privilege, and coming of political age in the postwar period.

Intimidation, however, works differently with respect to the two types of

political involvement. A perception of the KGB as competent inhibits nonconformity but does not breed greater compliant activism, while atomization appears to deter both types of political activism, nonconformist and compliant.

Conclusions and Implications

Our analysis confirms that general deterrence works, at least to a degree. What is striking is that individual deterrence does not. Perceptions of the regime's coercive capability tend to inhibit dissent *ex ante*; but once people do transgress the borders of acceptable political behavior, punishment (short of incapacitation) is not likely to keep them from further nonconformity. We think this indicates that the experience of being punished reduces an individual's possible gains from future compliance and induces a more permanent hostility to the regime.

Thus, there appears to be a trade-off between individual and general deterrence. By cutting off the return route to normalcy for those whom it punishes, the state assures a certain amount of recidivism and may create a class of people who are permanently alienated. At the same time, the unidirectional nature of this route heightens the effectiveness of *general* deterrence: the permanency of the stigma of being punished may be a key to deterring the otherwise disaffected from nonconformity in the first place.

Therefore, aside from the question of cost-effectiveness, singling out leaders for reprisals is logical from the regime's standpoint. By not trying to punish every transgressor of official norms, the state minimizes the number of people permanently alienated by being penalized. But punishing leaders provides a demonstration effect that enhances general deterrence.

Our data also reveal that the modernization of coercion has engendered a sense

of its predictability. Soviet citizens, especially the young and the highly educated, seem to believe they know the odds of reprisal if they violate official political norms. However, the differences in such perceptions suggest that the reduction in the use of mass terror since Stalin's day has had different impacts on different publics.

For witnesses of the Great Terror in the late 1930s, the message is still clear: *the KGB can still get you, and you shouldn't trust other people*. Those who survived the Stalin era see the secret police as less competent, perhaps because of the arbitrariness and sheer scope of police activity during that time. But among these generations, the more competent the KGB is perceived to be, the less nonconformity.

For the young, the Great Terror is an appropriated memory. The postwar and post-Stalin generations view the KGB as more professionally competent, but they are also more inclined to see the KGB's actions as predictable and avoidable. Without the experience of the Stalin era, they seem to feel better able to calculate the odds of reprisal. Equally important, these generations have grown up in an atmosphere of more stable social relations. Mistrust of others has diminished; these generations are less atomized.

Not all members of the postwar and post-Stalin generations see the world through the same eyes, however. The college-educated are much more sanguine about their ability to avoid political trouble, in part because they possess the conceptual skills and information needed to manipulate the system, and in part because the kinds of unconventional activities in which they are most likely to engage bring fewer reprisals. Few unsanctioned "intellectual" activities (e.g., unsanctioned study groups, reading and distributing *samizdat-tamizdat*) evoked any official penalty. But the types of nonconformity in which the *less* educated are likely to engage (e.g., strikes) are much

more likely to evoke a coercive response.

The regime's selectivity in dealing with unorthodox behavior thus carries class overtones and may help to explain why there are fewer blue-collar dissenters. In addition, the less educated are more satisfied with material conditions, more supportive of state controls over the economy and over the citizenry (Silver 1987), and more positive in their view of the leaders of Soviet institutions in general.

For Soviet leaders, there appears to be a trade-off between building up mass support for the established political order and building up the image of the KGB. The greater the sense of material satisfaction, then the more positive the evaluation of Soviet leaders as a whole, and the stronger the backing for the established norms of state control. But the greater the perceived capability of the KGB—that is, the more coercive the regime is believed to be—the weaker the popular support for the regime.

Our results thus suggest that if Soviet leaders were to downgrade the KGB, they could increase support for the regime. But a KGB with a less potent image would also be less able to deter nonconformity. Hence, a leadership that considers the maintenance of its own control over society to be a paramount goal is likely to continue to promote the KGB's image. As long as the KGB does not threaten the leadership itself, the leadership can accept the resulting reduction in popular support as a necessary cost.

In sum, the regime's ability to maintain social control relies on a correlation of forces: the manipulation of material and normative rewards and a mix of mistrust and intimidation. One critical variable is the perceived level of material welfare of the population. The greater the sense of material well-being, the greater the popular support for Soviet political orthodoxy and the more positive the evaluations of Soviet leaders. But even among those who have a low sense of material

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satisfaction and low support for the system, social atomization and perceptions of the regime's coercive potential still inhibit nonconformity.

Our data show that the balance is changing, however. The success of Soviet leaders in maintaining political peace depends on their striking a balance between individual and general deterrence, on a residuum of social atomization, and on a reserve of popular support that is stronger among the older genera-

tions and the less educated. But with the succession of generations, the social atomization bred by the Great Terror may be withering away, along with its inhibiting effect on unconventional behavior. Generational replacement and increasing educational attainments also appear to be weakening support for the old orthodoxies. Therefore, a regime bent on suppressing dissent and open opposition must rely on other means to achieve the same degree of control.

Appendix

Table A-1. Mean Rating of Leaders of Institutions for Competence and Honesty, by Level of Education within Generations

Level of Education Within Generations	Local Party	Local Soviet	Local Police	Managers of Industry	Academy of Sciences	Military	Politburo	KGB	All
COMPETENCE									
All respondents									
Less than secondary	2.29	2.40	2.30	2.65	2.91	2.94	2.36	2.45	2.54
Complete secondary	2.13	2.20	2.20	2.58	2.86	2.90	2.33	2.62	2.48
Complete higher	1.94	2.01	2.10	2.46	2.77	2.72	2.07	2.64	2.34
Prewar									
Less than secondary	2.17	2.22	2.09	2.55	2.81	2.74	2.18	2.43	2.40
Complete secondary	1.97	2.16	2.03	2.45	2.72	2.72	2.04	2.03	2.26
Complete higher	1.67	2.00	1.92	2.43	2.51	2.59	1.73	1.85	2.16
Wartime									
Less than secondary	2.12	2.40	2.44	2.63	2.77	2.98	2.17	2.39	2.53
Complete secondary	1.95	2.17	2.08	2.48	2.70	2.68	2.11	2.13	2.32
Complete higher	1.93	1.98	1.91	2.56	2.70	2.74	1.97	2.30	2.30
Postwar									
Less than secondary	2.48	2.50	2.42	2.73	3.07	3.09	2.60	2.45	2.68
Complete secondary	2.17	2.24	2.20	2.53	2.76	2.92	2.37	2.61	2.47
Complete higher	1.97	2.00	2.13	2.45	2.76	2.66	2.02	2.61	2.34
Post-Stalin									
Less than secondary	2.33	2.77	2.46	2.78	3.00	2.96	2.57	2.67	2.65
Complete secondary	2.18	2.20	2.25	2.65	2.96	2.96	2.39	2.81	2.56
Complete higher	1.94	2.02	2.11	2.46	2.82	2.78	2.15	2.78	2.37
HONESTY									
All respondents									
Less than secondary	1.76	1.88	1.69	2.02	2.34	2.53	1.76	1.49	1.99
Complete secondary	1.48	1.61	1.40	1.86	2.20	2.20	1.36	1.24	1.70
Complete higher	1.10	1.37	1.21	1.84	2.08	1.94	.93	.94	1.46
Prewar generation									
Less than secondary	1.59	1.85	1.63	1.99	2.21	2.34	1.58	1.34	1.89
Complete secondary	1.59	1.79	1.59	1.90	2.35	2.29	1.41	1.10	1.80
Complete higher	1.31	1.63	1.29	1.93	2.09	2.08	1.00	.85	1.57
Wartime									
Less than secondary	1.55	1.69	1.45	1.78	2.04	2.44	1.47	1.31	1.84
Complete secondary	1.51	1.70	1.48	1.92	2.11	2.20	1.52	1.30	1.73
Complete higher	1.21	1.35	1.22	1.80	2.17	2.15	1.20	1.02	1.54
Postwar									
Less than secondary	2.06	2.04	1.88	2.18	2.58	2.75	2.07	1.65	2.18
Complete secondary	1.43	1.55	1.30	1.84	2.13	2.19	1.26	1.13	1.63
Complete higher	1.06	1.30	1.17	1.78	2.00	1.90	.88	.91	1.43
Post-Stalin									
Less than secondary	1.76	1.93	1.80	2.07	2.65	2.62	2.05	1.96	2.12
Complete secondary	1.48	1.59	1.40	1.86	2.22	2.19	1.38	1.32	1.71
Complete higher	1.09	1.39	1.24	1.88	2.14	1.94	.93	.95	1.46

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**Table A-2. Mean Rating of Leaders of Institutions for Competence and Honesty,
by Generation within Levels of Education**

Generation within Levels of Education	Local Party	Local Soviet	Local Police	Managers of Industry	Academy of Sciences	Military	Politburo	KGB	All
COMPETENCE									
All respondents									
Prewar	1.99	2.15	2.04	2.49	2.71	2.71	2.04	2.17	2.30
Wartime	2.00	2.19	2.14	2.54	2.72	2.78	2.10	2.24	2.38
Postwar	2.12	2.16	2.20	2.52	2.80	2.82	2.23	2.59	2.44
Post-Stalin	2.07	2.13	2.19	2.57	2.90	2.88	2.29	2.79	2.48
Less than secondary									
Prewar	2.17	2.22	2.09	2.55	2.81	2.74	2.18	2.43	2.40
Wartime	2.12	2.40	2.44	2.63	2.77	2.98	2.17	2.39	2.53
Postwar	2.48	2.50	2.42	2.73	3.07	3.09	2.60	2.45	2.68
Post-Stalin	2.33	2.77	2.46	2.78	3.00	2.96	2.57	2.67	2.65
Complete secondary									
Prewar	1.97	2.16	2.03	2.45	2.72	2.72	2.04	2.03	2.26
Wartime	1.95	2.17	2.08	2.48	2.70	2.68	2.11	2.13	2.32
Postwar	2.18	2.24	2.20	2.53	2.76	2.92	2.37	2.61	2.47
Post-Stalin	2.18	2.20	2.25	2.65	2.96	2.96	2.39	2.81	2.56
Complete higher									
Prewar	1.67	2.00	1.92	2.43	2.51	2.59	1.73	1.85	2.16
Wartime	1.93	1.98	1.91	2.56	2.70	2.74	1.97	2.30	2.30
Postwar	1.97	2.00	2.13	2.45	2.76	2.66	2.02	2.61	2.34
Post-Stalin	1.94	2.02	2.11	2.46	2.82	2.78	2.15	2.78	2.37
HONESTY									
All respondents									
Prewar	1.53	1.78	1.55	1.95	2.24	2.27	1.41	1.16	1.80
Wartime	1.44	1.61	1.41	1.85	2.11	2.26	1.43	1.24	1.72
Postwar	1.34	1.50	1.32	1.86	2.12	2.14	1.18	1.10	1.61
Post-Stalin	1.31	1.51	1.33	1.87	2.20	2.09	1.19	1.17	1.61
Less than secondary									
Prewar	1.59	1.85	1.63	1.99	2.21	2.34	1.58	1.34	1.89
Wartime	1.55	1.69	1.45	1.78	2.04	2.44	1.47	1.31	1.84
Postwar	2.06	2.04	1.88	2.18	2.58	2.75	2.07	1.65	2.18
Post-Stalin	1.76	1.93	1.80	2.07	2.65	2.62	2.05	1.96	2.12
Complete secondary									
Prewar	1.59	1.79	1.59	1.90	2.35	2.29	1.41	1.10	1.80
Wartime	1.51	1.70	1.48	1.93	2.11	2.20	1.52	1.30	1.73
Postwar	1.43	1.55	1.30	1.84	2.13	2.19	1.26	1.13	1.63
Post-Stalin	1.48	1.59	1.40	1.86	2.22	2.19	1.38	1.32	1.71
Complete higher									
Prewar	1.31	1.63	1.29	1.93	2.09	2.08	1.00	.85	1.57
Wartime	1.21	1.35	1.22	1.80	2.17	2.15	1.20	1.02	1.54
Postwar	1.06	1.30	1.17	1.78	2.00	1.90	.88	.91	1.43
Post-Stalin	1.09	1.39	1.24	1.88	2.14	1.94	.93	.95	1.46

Table A-3. Pearson Correlations between Evaluations of Competence and Honesty of Leaders of Eight Soviet Institutions

Institution	Local Soviet	Local Party	Local Police	Managers of Industry	Academy of Sciences	Military	Politburo	KGB
COMPETENCE								
Local Soviet	1.0	.68	.59	.65	.53	.57	.60	.42
Local Party		1.0	.66	.52	.47	.53	.74	.58
Local Police			1.0	.51	.48	.55	.65	.64
Managers of Industry				1.0	.60	.59	.50	.46
Academy of Sciences					1.0	.65	.50	.49
Military						1.0	.54	.57
Politburo							1.0	.66
KGB								1.0
Average R								.55
HONESTY								
Local Soviet	1.0	.79	.73	.65	.64	.66	.70	.66
Local Party		1.0	.76	.61	.60	.62	.80	.77
Local Police			1.0	.62	.58	.61	.71	.73
Managers of Industry				1.0	.64	.65	.54	.52
Academy of Sciences					1.0	.76	.56	.53
Military						1.0	.60	.56
Politburo							1.0	.84
KGB								1.0
Average R								.66

Notes

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1. We refer to *nonconformity* and *unconventional activity* rather than *dissent* for two reasons: First, the term *dissent* implies active, programmatic opposition, while we are concerned with the disposition to overstep the bounds of politically correct behavior, programmatic or not. Second, most respondents who did overstep would not necessarily refer to themselves as *dissidents*.

2. In two cases, part of the respondent's LNP

occurred during Nikita Khrushchev's regime. In all other cases, the respondent's LNP both began and ended during Brezhnev's regime. In 92% of the cases, the respondent's LNP ended between 1978 and 1981, inclusive.

3. Since our concern is with adult political activity, we restrict our analysis to the 2,667 respondents who had reached age 18 at the start of their LNP. Field work for the survey was conducted by the National Opinion Research Center. The average length of the interviews was three hours. All but 10 of the interviews were conducted in Russian. The sample was a stratified random sample based on a list of adult emigrants from the USSR to the United States between the dates mentioned. Participation in the survey was voluntary. The response rate was 79%. For further information about the sample, see Anderson and Silver 1987a.

4. The respondent's ethnic identity comes into play in two ways. One is on questions asking specifically about ethnic relations and nationality policies in the USSR. Second, Soviet emigrants in our study came from two basic groups: persons in the third wave of the Jewish emigration (which includes many non-Jewish spouses and family members), and individuals who were political dissidents or other special cases involving family

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reunification (this group consists primarily of non-Jews, but it also includes many Jews). For this reason, non-Jewish respondents were more likely to be politically nonconformist than Jewish respondents. For further discussion of potential sources of bias in the survey, see Bahry 1985, 1987; Silver 1987; Anderson and Silver 1987a, 1987b; Millar 1986; and Swafford et al. 1987.

5. These concepts are widely used in the literature on criminology. For a brief statement, see von Hirsch 1976.

6. *Samizdat* (literally "self-publication") is underground material by Soviet authors. *Tamizdat* (literally "publication over there") is material by Soviet authors that has not been approved for publication or distribution in the Soviet Union but has been published abroad.

7. Thomas Schelling has observed that threats can often be made more credible if the actor who makes the threat appears to be irrational and unpredictable: "The essence of randomization in a two-person zero-sum game is to preclude the adversary's gaining intelligence about one's own mode of play—to prevent his deductive anticipation of how one may make up one's own mind, and to protect oneself from tell-tale regularities of behavior that an adversary might discern or from inadvertent bias in one's choice that an adversary might anticipate" (1963, 175).

8. Many more engaged in "private" nonconformity such as reading of *samizdat-tamizdat*. And many engaged in behaviors that are officially frowned upon but tacitly allowed, such as listening to Western radio broadcasts. In addition, almost 20% of the respondents indicated that they had attended unofficial art shows or other cultural events; but information from interviewers suggests that many people did not hear the word *unofficial* in the question and hence answered in terms of the regime-sanctioned cultural activities that they attended. See Bahry 1987.

9. Punishment includes any action taken against the respondent by any agency or official (including the KGB, the Party, supervisors at work, and other organizations or individuals sanctioned by the state). The penalties include reprimands at work or school; interrogations or reprimands by Party officials or the KGB; being threatened, harassed, followed, or arrested. Most official actions taken against the individual were reprimands, warnings, or interrogations.

10. It is important to note that the boundaries of the permissible fluctuate over time (Reddaway 1983). But there appear to be clear patterns of reprisal for the period under study.

11. This information is obtained by asking questions about any official actions that were taken in connection with the respondents' participation in other unconventional activities, their failure to participate in mobilized participation (such as voting),

or their making individual protests to officials, as well as the question, Other than what you may have already told me, did you have any (other) contact with the KGB?

12. The low frequency of contact with the KGB overall among SIP respondents could reflect a reluctance of the respondents to speak frankly about these contacts. However, the frequency of contact is consistent with Reddaway's (1983) estimates of some two hundred political arrests per year in the late 1970s in the entire USSR.

13. We also tested whether the effect of individual deterrence might depend on whether a person was arrested or interned rather than harassed or interrogated; but the severity of punishment made no difference in later political nonconformity. To be complete, the analysis should also include data on the incidence of recidivism among people who were nonconformists before their LNP but who were not penalized for it, but the survey does not provide this information.

14. This contrasts with the general Soviet philosophy of punishment, which focuses on rehabilitation rather than retribution (Hazard 1983, 99ff).

15. Two series of questions were asked. The first concerned the honesty of officials: "Here is a list of some institutions that exist in the Soviet Union. I would like you to think about how many of the people in charge of each of them you felt were *honest* during your last normal period. For each one, circle the number under the category that best describes how many of the people in charge were *honest*" (*almost all, most, some, hardly any, or none*).

The second series concerned the competency of officials: "Now turn the page over. On this side, think about how many of the people in charge at each of these institutions were competent, that is, did their jobs well. Remember, try to think about how you felt during your last normal period."

"People in charge" translates as *rukovoditeli*; "competent" translates as *kompetentnyi*; and "honest" translates as *chestnyi*. *Chestnyi* may be interpreted not simply as "honest" but also as "honorable" or "having integrity."

16. A factor analysis of the responses to the 16 items confirms that the responses to the competence items are distinct from the responses to the honesty items. The analytic results are not presented here.

17. *None* is assigned a value of 0; *hardly any*, 1; *some*, 2; *most*, 3; and *almost all*, 4. Averaging the evaluations across all eight institutions, we get a mean value on competence of 2.43, with a standard deviation of .782.

18. The "state-control" dimension is reflected in answers to questions about the respondent's preference for state ownership of heavy industry, state control of agriculture, and state provision of medical care. The "collective-control" dimension is reflected in answers to questions about limitations on the right to strike, control of population movement

through residence permits, and protecting the rights of society rather than the rights of individuals accused of crimes. See Silver 1987.

19. A Party *nomenklatura* post is a job or position in the state or cooperative sector for which a Party official controls personnel selection. The official can either directly determine or exercise a veto over who fills the post. Five percent ($N = 134$) of the respondents reported that their job during their LNP was a Party *nomenklatura* position.

20. Full specification of the relationships between experience with the KGB, subjective material satisfaction, and evaluation of the authorities requires a multiequation model. Our main concern here is whether the relationship between the respondents' evaluations of the authorities and their social backgrounds, subjective material satisfaction, and support for regime norms remains even after their punitive experiences are taken into account. The answer is yes, they do.

21. This is the r -square obtained by regressing the evaluation of the competency of the leaders of the KGB on the evaluations of the competency of the other institutional leaders.

22. This involves the use of an instrumental variable derived by regressing the evaluation of the KGB on the evaluations of the other seven agencies. The resulting regression coefficients were then used to estimate an expected evaluation of the KGB for each respondent based on the respondent's evaluation of the other agencies. The expected KGB evaluation was then subtracted from the respondent's actual (reported) KGB evaluation to derive a KGB differential—the difference between the actual and expected rating of the KGB's competence given the respondent's ratings of the other institutions. The mean value for the KGB differential is 0, with a standard deviation of .781, and with values ranging between -3.3 and 3.5 .

23. This can be shown by reversing the assumed direction of the causal relationship between support for regime norms and the evaluation of competence of the authorities from the direction specified in Tables 3 and 4. However, to study individual differences in the evaluation of the authorities, it seemed most reasonable to assume that these evaluations are contingent on the individual's subjective material welfare and on his or her affect toward the regime in general. The relation could in fact be reciprocal.

24. The mean on the faith-in-people measure is 1.172, the standard deviation is 1.117, and scores range from 0 to 3. The wording of the items and the method of calculating the combined measure is given in the notes to Table 6. Three additional items from Rosenberg's misanthropy scale were included in the survey. These were agree-disagree items rather than balanced items with arguments and counterarguments; they appear to have been subject to a substantial amount of acquiescence response bias. We

exclude them from the analysis for this reason.

25. This was one of a series of questions: "We are interested in the kinds of things people in the Soviet Union discuss with one another. Please look at this card, and tell me with which of these people you could have [*vy mogli by*] talked to during your last normal period about . . . the latest fashions or sports event? where to buy clothing on the black market? criticizing a government official?" The card listed the five groups mentioned in the text, and multiple answers were recorded for each question.

26. We performed a similar analysis of the impact of intimidation on individual contacting of government and party organs and on the use of *blat* or *protektsiia* (connections) in such bureaucratic encounters. The results suggest that the variables we examine here (in particular, perceptions of the competence of the KGB and of interpersonal trust) have little effect on such behaviors. Contacting involves a distinct type of activity with different costs and benefits that mobilize the public in a different way.

27. We counted *leadership* in a trade union as a conventional activity; but simple membership was excluded since it is all but mandatory. Party membership could not be included: in pretests of the questionnaire, a direct question about whether respondents belonged to the Party proved to be so sensitive that it was omitted from the survey. However, our measure of whether the respondents held Party *nomenklatura* posts in the LNP is a partial substitute for such a variable.

28. For each type of activity during the LNP, respondents were given one point if they belonged but never attended meetings, two points if they attended sometimes, three points if they attended regularly, and four points if they were leaders. For Komsomol members, the questionnaire asked only whether the respondents had ever belonged and whether they had been leaders. We counted only those leaders and members who had been in the Komsomol during their LNP.

29. The summary measure assigns two points for each type of nonconformist activity in which the respondent participated (but did not lead) during the LNP and four points for each type of activity in which the respondent took a leading role.

30. Our measure of intimidation is a binary variable derived from the question on competence of the KGB. Respondents who reported that *most* or *almost all* of the leaders of the KGB were competent are assigned a value of one; respondents who stated that *some*, *hardly any*, or *none* of the leaders of the KGB were competent are assigned a value of zero. Responses coded as *don't know*, *not ascertained*, and *refused to answer* are coded as missing.

31. A majority of the cases are scored as zero on each dependent variable. This suggests that it would be appropriate to use a method other than ordinary-least-squares (OLS) regression to test for the sources of compliant and nonconformist activism. In

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analyses not shown here, we applied probit analysis to test the same hypotheses. We tested first whether people engaged in *any* kind of nonconformist political behavior and then whether those who engaged in any such behavior were likely to become leaders. The results suggest that the independent variables play different roles in accounting for transitions across different thresholds of nonconformity. Although there are some differences in the relative strengths of the independent variables when this

alternative technique is used instead of OLS, the basic conclusions suggested by the probit analysis are similar to those produced by OLS.

32. However, we tested whether political nonconformists were less likely to be punished if they held a Party *nomenklatura* post or were compliant activists. The answer is *no*: the incidence of punishment depended more on the type of nonconformity than on job status or participation in approved activities.

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HOW ARE FOREIGN POLICY ATTITUDES STRUCTURED? A HIERARCHICAL MODEL

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It has long been assumed that foreign-policy attitudes of the mass public are random, disorganized, and unconstrained if they exist at all. Further, foreign-policy thinking has not been found to be structured along standard ideological (liberal-conservative) lines, partisan lines, or class lines. We attempt to move the discussion from a question of whether foreign-policy attitudes are structured to a question of how they are structured. We propose and estimate (using a LISREL model) a hierarchically organized foreign-policy belief structure in which specific policy preferences are derived from postures (broad, abstract beliefs regarding appropriate general governmental strategies). These postures, in turn, are assumed to be constrained by a set of core values about the international community.

It is by now accepted as a truism that structure and consistency are highly desirable properties of a political belief system—carrying important implications for the competence of the average citizen as well as the functioning of the democratic process. At the individual level, citizens with more consistent and highly related attitudes are assumed to be more thoughtful and efficient in their political reasoning. And at the system level, communication between political elites and the mass public is assumed to be greatly facilitated when the electorate imposes a common and meaningful structure on its attitudes.

Because of the importance of the topic, the past two decades have produced numerous studies investigating the degree to which citizens demonstrate any meaningful structure in their political attitudes (see Asher 1984; Kinder 1983; and Niemi and Weisberg 1984 for chronicals). The

seminal research was conducted by Converse (1964), who found very little consistency either among domestic attitudes, among foreign-policy attitudes, or between the two policy domains. As one recent reviewer of the debate has concluded, if we take ideological constraint to mean that specific policy positions are deduced from widely shared, sweeping ideological principles, then the consensus of research clearly indicates that most U.S. citizens are “innocent of ideology” (Kinder 1983, 401). For example, it is evident that specific issue positions in such diverse domains as social-moral policy, economic policy, and foreign policy do not derive from the single liberal-conservative dimension for which Converse searched in vain.

Yet, we would argue that it is inappropriate to conclude that mass attitudes lack any sort of meaningful structure; instead, analysts must move beyond the narrow

operationalization of constraint adopted by Converse to discover ways that citizens actually *do* organize their political attitudes.¹ Both the theoretical literature on schemata and the empirical findings of constraint research suggest that substantially more evidence of structure among political attitudes may be uncovered by adopting an approach that first, is domain specific and second, focuses on the connections between general and specific idea elements in a given domain. The recent schemata literature, for example, indicates that "domain-specific information is structured in memory in cohesive 'kernels' of thought" (Hamill, Lodge, and Blake 1985, 851). This work suggests that individuals bring different rules, criteria and processes to bear in different policy domains. Hamill, Lodge, and Blake, for instance, find that while individuals seem to structure their attitudes on economic issues according to a "rich-poor" schema, they are more likely to employ an ideological or partisan schema to structure attitudes on noneconomic issues. It is unlikely, consequently, that individuals would employ a generic criteria, or ideology, to structure attitudes in all policy domains.

The schema literature also suggests that attitude structure within a domain is more usefully studied by focusing on the relationship between general and specific attitudes (*vertical* constraint) than by focusing on the relationship between idea elements at the same level of abstraction (*horizontal* constraint). Briefly, schema theory assumes that people are "cognitive misers" who have limited ability for dealing with information and thus must use their old, generic knowledge to interpret new, specific information. From this perspective, attitude structure centers primarily on the linkages between abstract and concrete idea elements, where the former are assumed to "constrain" the latter. This emphasis is reminiscent of Converse's (1964) original conceptualization

of (psychological) constraint, which assumes that specific beliefs and attitudes derive from more "superordinate values or postures" (p. 211). This perspective provides a good fit to the available evidence generated from constraint research: studies which measure vertical constraint find considerable consistency between policy positions and more general idea elements (e.g., Conover and Feldman 1984; Lane 1962; Marcus, Tabb, and Sullivan 1974; Peffley and Hurwitz 1985), while research that measures horizontal constraint, or the relationship between attitudes at the same level of abstraction (e.g., Converse 1964), typically finds little consistency.

For these reasons, we argue that it is more productive to examine attitude structure using an intensive, domain-specific approach than an extensive, inter-domain approach. We have selected the foreign-policy domain for the focus of our examination for two related reasons. In the first place, foreign-policy attitudes have traditionally been assumed to be underdeveloped and disorganized, as will be discussed. Yet, more recent studies indicate that foreign-policy attitudes do exhibit structure, even if the structure does not necessarily conform to the neat liberal-conservative pattern envisioned by Converse and others.

Second, we examine the foreign-policy domain because, at present, we lack an adequate understanding of its antecedents and correlates. Standard approaches to the study of domestic attitudes are often found to be lacking when applied to foreign affairs. Partisan and liberal-conservative identifications, for example, are typically poor predictors of foreign-policy attitudes. And while social cleavages and group identifications are useful in predicting many domestic issues, social groups have not been found to differ greatly on foreign-policy issues, and the differences that do arise are often difficult to explain (Gamson and Modigliani 1969;

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Hamilton 1972). One's foreign-policy attitudes do not seem to be a matter of simple self-interest, either (e.g., see Lau, Brown, and Sears 1978). Our lack of knowledge in this important area is especially puzzling in light of much recent evidence that foreign-policy attitudes play a crucial role in influencing various forms of political evaluation, such as presidential voting (Aldrich, Sullivan, and Borgida 1986; Kessel 1984) and presidential support (Hurwitz and Peffley 1987).

It is our purpose to explore the properties of foreign-policy belief systems. Specifically, using a multiple indicator (LISREL) model, we estimate levels of foreign-policy issue consistency to determine whether the electorate is really as undisciplined and thoughtless in its approach to foreign-policy reasoning as suggested and to determine what general beliefs, if any, the mass public uses to structure more specific attitudes in this very important domain.

Background

The Earliest Work

The finding that U.S. citizens are not particularly sophisticated about foreign-policy matters is longstanding, going back to Almond's (1950) presentation of his "mood theory," where he argued, "foreign policy attitudes among most Americans lack intellectual structure and factual content. Under normal circumstances the American public has tended to be indifferent to questions of foreign policy because of their remoteness from every day interests and activities. . . . Foreign policy, save in moments of grave crisis, has to labor under a handicap; it has to shout loudly to be heard even a little" (pp. 69-71). A mass of survey evidence supports Almond's thesis that U.S. citizens are often unconcerned and uninformed

about foreign affairs (Erskine 1963; Free and Cantril 1968; Simon 1974). Moreover, the question of whether mass foreign-policy attitudes lack intellectual structure was answered in the affirmative with the publication of Converse's 1964 study which, as noted above, found little consistency among foreign-policy attitudes and even less consistency between foreign and domestic attitudes. Just as bothersome were Converse's (1964) findings that foreign-policy attitudes were not only unstable over the waves of the 1956-58-60 National Election Studies (NES) but were even less stable than domestic attitudes. Instability was also found to be the hallmark of foreign-policy attitudes in a more recent study by Converse and Markus (1979). Taken together, then, this evidence indicates little in the way of structure, integrity, or solidity in citizens' foreign-policy cognition.

More Recent Evidence

Scholars began to reexamine the structure of foreign-policy attitudes during the Vietnam War period. One of the first of these more recent studies, conducted by Verba et al. (1967) found Vietnam-related public opinion to be "relatively orderly. . . . The correlational analysis among scales shows patterns of consistency among the population" (p. 330). A dove-hawk continuum proved to be important for organizing the attitudes of many of the respondents. Verba et al.'s findings encouraged a large literature examining the structure of foreign-policy attitudes (e.g., Bardes and Oldendick 1978; Bennett 1974; Maggiotto and Wittkopf 1981; Modigliani 1972; Wittkopf 1981), which, almost without exception, supported the conclusion that foreign-policy attitudes are substantially more organized than Converse had determined them to be.

Skeptics of this recent literature might argue that findings of attitude structure

we now have several types of evidence that Converse may have seriously underestimated attitudinal stability, in general; more specifically, we also have evidence that foreign-policy opinions are every bit as stable as their domestic-policy counterparts. Achen (1975), Erikson (1978, 1979), and others argue that response inconsistencies over time are due primarily to random measurement error in the survey items used to assess these attitudes. When over-time correlations of foreign-policy attitudes in the 1956-58-60 NES panel (foreign aid, military involvement, and isolationism in Achen's study) are corrected for measurement error, they are quite high, approaching 1.0.²

In short, there is mounting evidence that foreign-policy attitudes are considerably more stable and organized than early research had found. This recent literature, in addition to its disagreements with the conclusions of Converse, shares several other characteristics. In the first place, the studies do not assume that foreign-policy attitudes are structured unidimensionally along the same liberal-conservative continuum on which attitudes in other domains fall. Rather, with few exceptions, people were found to use multiple dimensions to organize their foreign-policy beliefs—dimensions specific to the domain of foreign affairs, such as militarism, isolationism, and liberal internationalism, to name but a few. Second, many of the studies focus on the relationship between general and specific attitudes (vertical, rather than horizontal, constraint). Much of this work relies on exploratory factor analysis to uncover superordinate constructs (or factors) capable of explaining the covariation between specific foreign-policy opinions. And, as argued above, it is far more realistic to expect attitude structure, if it exists, to be vertical than to be horizontal. are specious, given Converse's (1964) discovery that foreign-policy attitudes are so remarkably unstable over time. In fact,

Some Problems with the Literature

There is, then, impressive evidence that the foreign-policy thinking of individuals is structured; unfortunately, though, the recent literature is plagued by a number of methodological and conceptual problems that must be addressed. In the first place, as noted, much of the work employs factor analysis to uncover evidence of attitude structure, a technique with several notable shortcomings. For one, the final solution of any exploratory factor model is heavily influenced by essentially arbitrary mathematical constraints rather than substantive or theoretical ones (Long 1984). Another drawback of the technique is that interpreting the substantive content of a factor involves a certain amount of arbitrariness on the part of the analyst (Long 1984); merely supplying a label of, say, *militarism* to a factor provides no new insight into the exact meaning of the construct.

A more general problem with the research, both factor analytic and correlational, is that it is largely descriptive, with few examples of theory-guided discussion. While there is a general consensus that foreign-policy attitudes are somehow interrelated, few authors have paid much attention to the questions of why or how they are related. And while there seems to be general agreement that belief structures are largely vertical (i.e., that specific foreign-policy attitudes are bound by more abstract, general beliefs), there is very little discussion of the utility of such an organization for the individual. Thus, while the recent work in the area has been encouraging and has demonstrated the inappropriateness of Converse's methods and conclusions, a great deal more needs to be done to determine precisely how, and why, foreign-policy attitudes are linked together.

In order to answer these questions and others, we begin with a realistic appraisal of the process by which the ordinary

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citizen attempts to make sense out of the complex international environment. The difficulties inherent in gaining an understanding of such a complicated universe argue for a hierarchical model of political reasoning, wherein citizens use general and abstract beliefs to constrain more specific foreign-policy attitudes and preferences.

An Information-Processing Perspective

We base our investigation of how people make sense out of international relations on an information-processing approach. Grounded in cognitive-psychology and social-cognition literatures, the information-processing perspective assumes that because humans have very severe cognitive limitations, they often behave as *cognitive misers*, coping with their shortcomings by taking shortcuts whenever possible (see Fiske and Taylor 1984 for a review of this perspective). Importantly, an individual's reliance on simplification is assumed to increase with the complexity of the environment. Exceedingly simple stimuli can be processed in a relatively veridical, faithful way, even by a cognitively limited perceiver. But a complicated and confusing world requires extensive use of heuristics to render the environment interpretable and manageable.

Among the various policy domains that comprise the political environment of the average citizen, the international sphere is exceptionally complex and ambiguous. International events that occur around the globe are remote, fluid, and extraordinarily complicated. They are also ambiguous in that information regarding the international scene is often distorted or deliberately withheld from the public. Consequently, citizens are forced to rely on the assessments of U.S. political elites and media commentators, who interpret

world events but who also, quite often, disagree with one another. In short, the complexity of international affairs makes the policy domain a very ambiguous and difficult one for the public to follow, at least without having to pay substantial costs.

Owing to the complexity of the domain, *cognitive heuristics* (information short-cutting) is of great importance in explaining how a "typical citizen" processes foreign-policy information, makes decisions on foreign-policy issues, or structures foreign-policy beliefs. Yet the standard guidelines used to make sense of politics (and political issues) have not been found useful in the foreign-policy domain. It has frequently been argued, for example, that many individuals support policies that enhance their well-being or self-interest. However, the connections between one's self-interest—narrowly defined—and foreign affairs are, doubtless, even more difficult to determine than in the domestic sphere. Thus, it is not surprising that studies have found only very weak relationships between self-interest and foreign-policy attitudes (Mueller 1973; Lau, Brown, and Sears 1978). Other common criteria by which policies are often evaluated include partisanship, liberal-conservative ideology, and social class. But, as noted previously, Converse found that foreign-policy attitudes are not organized along the liberal-conservative dimension; as well, he found virtually no systematic or predictable difference between Democrats and Republicans on foreign-policy issues, a finding replicated in more recent years (e.g., Pomper 1975; Verba et al. 1967). And with the exception of isolationist sentiments, foreign-policy opinions have not been found to be connected to social status (e.g., Gamson and Modigliani 1969).

In short, political judgments in the international sphere must be made under considerable uncertainty and without many of the interpretive aids commonly

used in the domestic arena. It would nevertheless be a mistake to assume that in the face of this uncertainty, most citizens will refrain from holding foreign-policy attitudes or will simply embrace "official" governmental policies as their own. Following the literature on social cognition in social psychology, we assume that people cope with uncertainty in international affairs much the same way they do in other content domains—by relying on their store of general knowledge to guide the processing of more specific information.

Among researchers who study schemata, such general-to-specific reasoning has been called "top-down" or "theory-driven" processing and is usually represented by a hierarchical structure in which abstract beliefs influence the response of the individual to more specific stimuli. It is theory driven in the sense that under conditions of uncertainty, people are assumed to behave as cognitive misers by using old, generic knowledge to interpret new, specific information. General beliefs permit economical judgments to be made under uncertainty because they provide interpretations for ambiguous stimuli, select information for storage or retrieval from memory, and "fill in" missing or ambiguous information with best guesses or default values (Taylor and Crocker 1981; Tversky and Kahneman 1981).

There are several examples of such hierarchical models of political reasoning in foreign affairs. Conover and Feldman (1984) found that their sample of university students used more general schemata to organize their more specific policy preferences in foreign affairs and other domains. And in his computer simulation of foreign-policy reasoning, Carbonell (1978) investigated a hierarchical belief structure in which higher-level goals (e.g., Communist containment) partially determine more specific policy goals (e.g., increasing military strength). Finally, in

his study of elite foreign-policy decision making, Jervis (1976) argued that the general beliefs held by diplomats and other experts exerted a powerful effect on their interpretations of foreign-policy events. These studies provide preliminary evidence that encourages us to investigate such hierarchical models as are common to the mass public with a conventional survey approach.

A Hierarchical Model of Foreign-Policy Attitudes

We expect citizens' foreign-policy attitudes to conform generally to the hierarchical model of constraint presented in Figure 1, where more abstract idea elements constrain more specific ones. At the most concrete level (the bottom or third tier of the figure), are preferences for specific foreign-policy issues—defense spending, nuclear-arms policy, international trade, and policies toward the USSR. Constraining these attitudes at the second (middle) tier of the attitude hierarchy are more abstract beliefs concerning the appropriate role of government in its handling of foreign affairs. These normative beliefs are collectively referred to as *postures*, for they convey the general stance or orientation the individual would like to see the government adopt in conducting foreign policy, without indicating what specific policies should be employed to attain the desired goal. Should the government adopt an aggressive or an accommodating stance in dealing with other countries? Should the government chart an internationalist or an isolationist course in international affairs? These are the basic themes defined in the factor-analytic studies. By relying on such general prescriptions to render more specific preferences, citizens are able to categorize and to evaluate economically a wide variety of concrete policies on the basis of whether those policies are consistent with their more general postures.

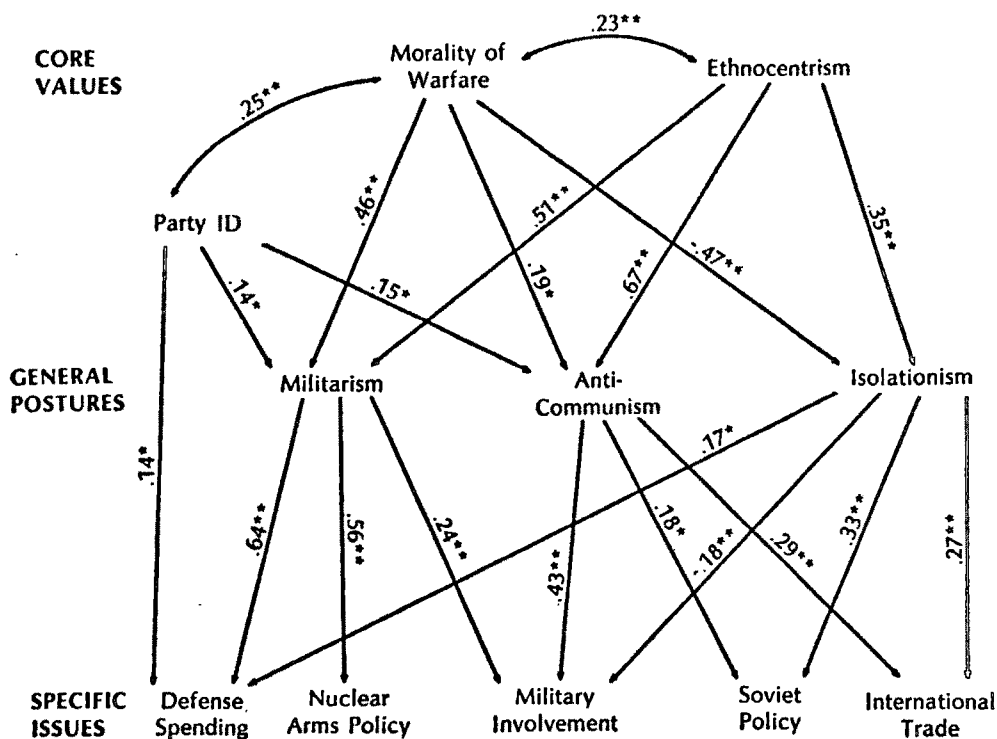
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Thus, to use a simple example, whether an individual favors sending U.S. troops to Central America will depend in large part on whether the individual favors an isolationist or an interventionist posture in world affairs.

What more fundamental idea elements might constrain foreign-policy postures? At the first (and uppermost) tier of the

hierarchy in Figure 1 are what we call *core values*, such as ethnocentrism and moral beliefs about killing in warfare. Core values are easily distinguishable from postures in that they do not refer directly to governmental actions or policies. While postures are normative beliefs regarding a government's behavior, values are more personal statements

Figure 1. Structural Relationships between Foreign-Policy Attitudes in Hierarchical Model of Constraint



*p < .05 **p < .01

N = 435

Chi-square = 483, df = 257, chi-square/df = 1.88

Note: Coefficients are standardized *betas* estimated by full-information maximum likelihood. Estimates were computed with LISREL 6. All nonsignificant paths are omitted from the figure. Respondents scoring on the lower end of the variables are more *accepting of warfare*, *ethnocentric*, *Republican*, *militaristic*, *anti-Communist*, *isolationist*, *supportive of defense spending*, *opposed to arms control*, *supportive of military involvement in Central America*, *opposed to relations with Russia*, and *in favor of trade restrictions*.

regarding the individual's priorities and concerns. The belief that one's country is superior to all others (i.e., ethnocentrism), for example, carries with it no explicit reference to governmental policy, in either broad or specific terms. Conceptually, values (and the fundamental beliefs that link values to more specific attitudes) have been assumed to occupy a central position in belief systems (Kinder 1983). Rokeach (1973) has argued, for example, that values "lead us to take particular positions on social issues" (p. 13), and Allport (1961) notes that "attitudes themselves depend on preexisting social values" (pp. 802-3). Several recent studies have found empirical support for this view. Values of economic individualism, racial equality, and gender equality have been found to be an important source of attitudes toward welfare policies, civil rights, and women's issues, respectively (Feldman 1983; Kinder and Sears 1981; Sears, Huddy, and Schaffer 1986; Sniderman, Brody, and Kuklinski 1984).

The particular postures and fundamental values that occupy the upper reaches of the hierarchical model will be described more fully below. At this point we wish to emphasize a characteristic shared by these two idea elements that makes them especially attractive as interpretive aids: they are "easy,"⁴ largely because a deep familiarity with foreign-policy events and information is not a prerequisite to possessing a belief at either the value or the postural level. Postures should make intuitive sense to even a rank amateur in the international domain, while fundamental values, by their very nature, are based on personal beliefs that are central to one's world view and are not dependent on substantial expertise in foreign affairs.

Moreover, at least two of the dimensions, ethnocentrism and anticommunism, may rest primarily on an individual's affective orientation to the United States (i.e., ethnocentrism) and its prin-

ciple adversary, the Soviet Union (i.e., anticommunism). While this affect may be based on more philosophical principles for some individuals, it need not be. By simply knowing one's likes and dislikes, it should be a relatively easy task to place oneself on dimensions like ethnocentrism and anticommunism.⁵ Finally, values and postures should be relatively easy dimensions for the public to use in view of the likelihood that most public rhetoric in the area of foreign policy will be pitched at a general, symbolic level rather than at the concrete, policy level.

By way of summary, we note that investigating this hierarchical model of foreign-policy constraint allows us to overcome many of the limitations of previous research. Most importantly, in contrast to many factor-analytic studies, we directly measure the relationship between general and specific elements in a foreign-policy belief system. Our focus on foreign-policy attitudes also allows us to investigate a variety of vertical linkages between abstract and concrete elements that might be missed in a more superficial analysis. Further, we explore a common framework of ideological structure that is capable of being assessed using a conventional survey approach.

Methods

Sample and Measurement

The data for investigating the structure of foreign-policy attitudes come from a telephone survey conducted by the Survey Research Center (SRC) at the University of Kentucky, Lexington during February 1986. A variant of random-digit dialing was used to select the 501 Lexington residents who comprised the sample.⁶ The professional interviewers at the SRC asked respondents a battery of questions concerning their attitudes on foreign affairs during the course of each inter-

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view, which lasted an average of 10 to 15 minutes.

To estimate our hierarchical model of foreign-policy constraint, it was necessary to measure foreign-policy attitudes at several levels of abstraction in order to tap not only specific policy preferences but also foreign-policy postures and core values. The survey questions are presented in the Appendix; the vast majority of items were pretested in a pilot study of Lexington residents ($N = 128$), most of whom were students at the University of Kentucky.

Postures

We expect an individual's concrete issue preferences to be based on three global postures that describe the general direction the government should take in international affairs: *militarism*, *isolationism*, and *anticommunism*. Our selection of these postures has been motivated by the work of Kegley and Wittkopf (1982), Maggiotto and Wittkopf (1981), Schneider (1984), and others who have written about foreign-policy attitudes from a historical perspective. It is the contention of these authors that the contemporary U.S. public makes two basic foreign-policy choices: (1) whether or not to become involved with other nations and (2) if involvement is desired, whether the involvement should be militaristic or non-militaristic (e.g., diplomatic). Consequently, we include the isolationism and militarism postures to represent these two basic choices the mass public must face.

If *isolationism* provides guidance regarding the appropriate *extent* of involvement and *militarism* provides guidance regarding the *type* of involvement, *anticommunism* provides guidance regarding the *targets* of involvement. A central question in U.S. foreign policy in the postwar period has been, What is the appropriate posture of the United States toward Communist-bloc nations? Since

World War II, the Soviet Union and Communist countries have been viewed by U.S. presidents as the major international threat to the nation's interests and security (Kegley and Wittkopf 1982); and most major foreign-policy developments over this period (e.g., containment, *détente*, and others) have been justified as a means of coping with this threat.

All of these dimensions have consistently shown up in factor-analytic studies as superordinate constructs structuring more specific foreign-policy attitudes. Moreover, we believe these broad dimensions underlie much of the public discourse in the international sphere.⁷ Our definition and operationalization of each construct deserves some elaboration.

Militarism. The dimension of *militarism* is anchored, on the one end, by a desire that the government assume an assertive, militant foreign-policy posture through military strength and on the other by a desire for a more flexible and accommodating stance through negotiations. The specific items designed to tap this dimension emphasize "toughness" as opposed to "flexibility" in dealing with other countries (A.1 in the Appendix), the view that the best way to achieve peace is through military strength rather than negotiations (A.2), and the belief that the United States should be willing to "go to the brink of war" to preserve its military dominance (A.3). We expect this dimension to be an important predictor of the contemporary divisions that exist in public support for either a defense buildup at home or the use of U.S. military force overseas.

Anticommunism. Beliefs about the appropriate posture of the United States toward Communist-bloc countries should also play a central role in shaping concrete policy attitudes (see Gamson and Modigliani 1969 for corroborating evidence on this point). Four items were used to assess *anticommunism*: respondents were asked

whether the United States should do everything it can to "prevent the spread of communism" (B.1) or "check the spread of Soviet influence" (B.2) in the world, whether communism "is an acceptable form of government for some countries" (B.3), and whether or not the United States should try hard to "get along with Russia" (B.4). This posture should predict not only specific policy preferences in U.S.-Soviet relations, but also specific defense spending, military involvement, and nuclear-arms attitudes.

Isolationism. The *isolationism* posture is defined by a general desire that the government avoid any ties or entanglements with other countries, whatever the nature of the relationship (i.e., whether political, diplomatic, or militaristic). Since isolationism has longstanding roots in U.S. history (see McCormick 1985), it is not surprising that it has been used with some frequency to describe the foreign-policy orientations of the U.S. public (see, for example, McClosky 1967 and Sniderman and Citrin 1971).

While a number of isolationism scales exist, commonly used indexes either rely on questions too specific for use at the postural level (e.g., McClosky 1967) or rest on the questionable assumption that isolationism and internationalism lie at opposite poles of a single dimension (e.g., Watts and Free 1978). To avoid such problems, isolationism was measured by two Likert-scale items that expressed only isolationist (not internationalist) sentiments pitched at a fairly general level of abstraction. Thus, isolationists in our study are more likely to agree that "we shouldn't risk our happiness by getting involved with other countries" (C.1) and that the United States should turn inward and focus on problems at home rather than world affairs (C.2). This posture should constrain any policy that pertains to international involvement.

Specific Policy Preferences

To establish the range of applicability of our postural variables, we asked respondents to indicate their more specific foreign-policy preferences in several areas, including defense spending, nuclear-arms policy, military involvement (in Latin America), policy toward the Soviet Union, and international trade. The measurement of these more specific attitudes was an easier task, since there are a wealth of possible survey items to be culled from various national surveys, such as those used by the National Election Studies and the Chicago Council on Foreign Relations. As quick perusal of the appropriate section of the Appendix demonstrates, we followed the practice of measuring each policy attitude (except Soviet relations) with several indicators.

Core Values

One's placement on the postural dimensions is assumed to be guided largely by two core values: ethnocentrism and the morality of killing in warfare.

Ethnocentrism. *Ethnocentrism* is defined here as the belief that one's country is superior to all others; we expect such beliefs to underlie all three of the postures we have included in this study.⁸ If an individual truly believes that the United States is vastly superior to other countries in the world, this belief would certainly bolster the idea that the appropriate posture of the government should be the aggressive pursuit of our national interests (i.e., militarism) as well as the protection of our system from the threat posed by Communist countries, whose interests and values are seen to be antithetical to ours. Moreover, to the degree that ethnocentrism fosters a self-centered or parochial view of the world, the tendency may be to draw inward into an isolationist shell rather than to push outward in the world.

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The questions measuring ethnocentrism appear in the Appendix; they ask respondents whether other countries should try to make their governments as much like ours as possible (I.1), whether they agree that our government is the best form of government yet devised (I.2), whether other countries have governments that are just as good as ours (I.3), and how the United States compares with other countries in the world (I.4).

Morality of Warfare. Beliefs about the morality of killing in warfare are also presumed to constrain the postures described above. Clearly, one's willingness to endorse a militaristic posture should be related to one's values regarding the "rightness" of military solutions. Moreover, a strong sense that warfare is wrong should lead individuals to reject internationalism, given the tendency for many types of involvements to become military in nature. And finally, we would expect those most wary of warfare to advocate more restraint in dealing with the Soviets given the possibility that an aggressive containment policy with the Soviet Union could result in exacerbating, rather than alleviating, international tensions.

Briefly, an individual's values concerning the morality of warfare—whether taking the life of one's enemy is viewed as unconscionable or whether it is seen as a matter of necessity, perhaps even virtue—were determined by asking respondents whether it is "acceptable to kill one's enemies when fighting for one's country" (J.1) and whether a "person who loves his fellow man should refuse to fight in any war" (J.2).

As was the case with our selection of postures, we incorporated these two values into our model on theoretical grounds. It can be seen that we selected values of large range, which should be logically linked to all three of the postural positions. This is not to suggest that our list of values is exhaustive, as other values

(liberty, for instance) can and should be explored.

Methodology

We used Joreskog's (1973) method for the analysis of covariance structures (LISREL) to estimate the hierarchical model of foreign-policy attitudes.⁹ The covariance-structures model holds several advantages over other techniques used to estimate attitude structure and constraint. For one, the arbitrary mathematical constraints imposed on the exploratory factor model are replaced in LISREL with substantive constraints that can be "tested" with the data at hand. In addition, when one has multiple indicators of many of the idea elements in the constraint model, as we do, problems of measurement error that have plagued earlier studies of constraint (see, e.g., Achen 1975; Erikson 1978, 1979) are not so severe, since the structural relationships between latent idea elements estimated by LISREL are, in a sense, corrected for attenuation due to random measurement error.

Model Specification

Our model assumes that more concrete idea elements are constrained, or determined by more abstract attitudes at the next higher level of abstraction. Thus, each of the postures is assumed to be constrained by both of the core values, and each of the specific policy attitudes is assumed to be constrained by all three of the postures. The exogenous variables in the model, the two core values, are linked by unexplained covariances.

Partisanship and liberalism-conservatism were also included in the model as exogenous variables influencing postures and policy preferences alike. These variables, measured in the standard seven-point format developed by the NES-SRC, are included in the model for two reasons: We have noted, first, that past research

suggests that foreign-policy attitudes do not seem to be organized around these more domestic orientations. We should like to see whether this independence holds for the foreign-policy attitudes measured in this study. Second, these factors serve as control variables in the model to ferret out spurious associations between foreign-policy attitudes related only through their mutual dependence on partisanship and liberalism-conservatism.¹⁰

Findings

The estimated epistemic correlations between the indicators and the latent idea elements are presented in Table 1. Generally, the indicators appear to be fairly reliable measures of the theoretical constructs.

Figure 1 presents the LISREL estimates of the structural relationships between idea elements at different levels of

Table 1. Epistemic Correlations between Indicators and Factors

Theoretical Construct	Indicator	Epistemic Correlations	Construct R ²
General Postures			
A. Militarism	1. be tough or be flexible	.55	.69
	2. negotiate or maintain military strength	.50	
	3. maintain dominance	.55	
B. Anticommunism	1. contain communism	.75	.61
	2. contain soviets	.65	
	3. communism acceptable	.41	
	4. get along with Russia	.25	
C. Isolationism	1. avoid involvements	.60	.30
	2. not worry about world affairs	.75	
Specific Issues			
D. Defense spending	1. level of spending	.61	.52
	2. defense or nondefense cuts	.69	
E. Military involvement	1. Central America	.78	.37
	2. El Salvador	.73	
F. Nuclear arms	1. nuclear freeze	.56	.43
	2. nuclear-weapon increase	.77	
G. Soviet relations	1. cultural ties	1.0	.18
H. International trade	1. import restriction	.78	.18
	2. job protection	.62	
Core Values			
I. Ethnocentrism	1. others should imitate U.S.	.61	
	2. U.S. government best	.46	
	3. other governments as good	.46	
	4. how U.S. compares	.36	
J. Morality of war	1. killing acceptable	.58	
	2. should refuse to fight	.55	

Note: Coefficients are standardized *betas* estimated by full-information maximum likelihood. Estimates are computed from LISREL 6.

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abstraction. Overall, the hierarchical model fits the data quite well. The *chi-square-to-degrees-of-fit* statistic is 257 and, with 482.87 degrees of freedom, the *chi-square-to-degrees-of-freedom* ratio is 1.88.¹¹ The most general feature to note about the results in Figure 1 is the high level of interconnectedness between foreign-policy idea elements. In almost every case, attitudes at the postural and specific policy levels are significantly tied to at least two of the idea elements measured at a higher level of abstraction. And in several instances, the linkages are quite strong. Finally, as indicated by the *r-squares* in Table 1, a substantial proportion of the variation in most of the endogenous attitudinal elements (i.e., postures and specific policy preferences) is "explained" by more abstract beliefs (i.e., core values and postures, respectively). This is no small achievement in light of the expectations created by prior studies. Aside from the factor-analytic studies, a generation of research has cast severe doubt on the prospect of finding any evidence of consistency among foreign-policy attitudes. The general pattern of our results, however, clearly indicates that when constraint is measured as a series of vertical relationships between attitudes at different levels of abstraction, the degree of structure among foreign-policy attitudes is generally impressive.

The size and significance of the relationships between foreign-policy attitudes stand in striking contrast to the estimated linkages between these attitudes and partisanship and liberalism-conservatism. Our results thus echo those of previous studies, which failed to detect much evidence of a connection between these identifications and foreign-policy preferences; not only are most of the linkages statistically insignificant (and thus omitted from the figure) but those that are significant are uniformly low. Independent of their core values, for example, Republicans are only slightly more likely than

Democrats to opt for militaristic and anti-Communist postures in the international arena; and independent of their postures, they are only marginally more likely to favor increases in defense spending. Significant liberal-conservative differences (not shown) emerge only on the *militarism* dimension.¹²

Core Values and Postures

A closer inspection of the connections among foreign-policy attitudes turns up a number of interesting findings. Focusing first on the many arrows connecting idea elements at the top of Figure 2, it is evident that individuals' core values are an important foundation of their foreign-policy postures. Collectively, the two values (along with party identification and liberalism-conservatism) account for between 30% and 70% of the variance in the postures.

The relationships uncovered here provide valuable insights into the various ways that core values anchor the general orientations people develop to guide their foreign-policy thinking. And for the most part, the linkages that emerge are consistent with our conceptualization of the particular postures and core values included in the study. For example, as one might predict, respondents desiring a more militant stance in world affairs are likely to reject the idea that killing in warfare is immoral ($\beta = .46$); they also tend to endorse the ethnocentric view that their country is without equal in the world ($\beta = .19$).

Both of the core values affect the degree to which an individual prefers an isolationist posture in world affairs. One fairly strong basis for isolationism is an expressed concern about killing in warfare ($\beta = -.47$). The implication is that the desire to retreat from international affairs may stem, in part, from apprehension about the possibility of international involvements leading to military engage-

ments. We also find that a self-centered belief in the superiority of the United States ($\beta = .35$) underlies isolationist currents in public attitudes.

Ethnocentric values also contribute heavily to individuals' anti-Communist orientations ($\beta = .67$). On reflection, an anti-Communist posture seems a natural counterpart to an ardent faith in the "American way." As Levinson (1957) has argued, "Ethnocentric thinking in the sphere of international relations, like other forms of ethnocentrism, is based on a rigid and pervasive distinction between in-groups and out-groups . . . the focal out-group at any time being those nations who are seen, rightly or wrongly, as different from ours. . . . Other nations are seen as inferior, envious and threatening" (pp. 38-39). For many U.S. citizens, the longstanding "out-group" in foreign affairs has been the Soviet Union and other Communist countries, which many minds represent as an anti-ideal group whose interests and values run contrary to their own.

Postures and Specific Issues

A casual inspection of the lower portion of Figure 2 reveals that the three postures are crucial considerations in guiding an individual's preferences on a variety of concrete issues in foreign affairs. The effect of isolationism is as one might expect: the tendency is to reject consistently those foreign-policy options that call for more active involvement between the United States and other countries. Thus, isolationists, more so than non-isolationists, are opposed to committing U.S. troops overseas ($\beta = -.18$) and to renewed cultural relations with the Soviet Union ($\beta = .33$) but favor the imposition of trade barriers to protect U.S. jobs ($\beta = .27$). On the other hand, isolationists are in favor of increased defense spending. Though this relationship is not particularly large ($\beta = .17$), it is apparent that

the isolationist's preferred method of responding to external threat is to increase the U.S. arsenal on the home front (probably for *defensive* purposes) rather than to increase military (*offensive*) commitments overseas. In sum, while the *isolationism* dimension is a more powerful determinant of nonmilitary positions than of military positions, it still appears to provide an important cue in guiding people's preferences across a wide range of different types of issues.

An individual's affinity for an assertive, versus an accommodating, posture by the government is primarily an important predictor of military-type issues. Very clear and strong differences arise on the issues of defense spending and nuclear policy between militarists and accommodationists, with the former being much more likely to favor general increases in defense spending and to oppose arms policies that require the U.S. to cut back its nuclear arsenal. Individuals who favor a more militant U.S. posture in the abstract also tend to favor sending U.S. troops to various trouble spots under more specific circumstances.

The third posture, anticommunism, shows up as a significant predictor of both military and nonmilitary issues. Individuals favoring a tougher posture toward Communist countries are more likely than not to favor sending U.S. troops to Latin America ($\beta = .43$) and placing restrictions on foreign imports ($\beta = .29$) but oppose resuming cultural ties with the Soviet Union ($\beta = .18$). The rather modest size of the coefficient for Soviet policy may be explained by the tendency for our postural variable to reflect more militant sentiments toward the containment of communism, sentiments that may not be relevant for the rather non-controversial and diplomatic policy expressed in our measure of Soviet policy.

The absence of significant linkages between the anti-Communist posture on the one hand and defense spending and

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nuclear policy on the other is surprising. Nuclear-arms policies presumably involve the Soviet Union in some way and proposals to increase defense spending are often justified on the grounds that they provide a necessary deterrent to the Soviet Union and its allies. Our findings suggest, however, that one's position on these issues is, first and foremost, a reflection of the degree to which one favors an assertive, versus an accommodating, approach to foreign affairs. It may be that defense-spending and nuclear-policy issues are accounted for quite well by the *militarism* dimension, leaving little room for influence from *anticommunism*, which, after all, overlaps considerably with the *militarism* dimension ($r = .60$).¹³

The Degree of Specific-Issue Constraint

Our data also shed some light on the question of what kinds of foreign-policy issues are more apt to be grounded in general considerations and principles. Judging from the r -squares of the issue variables in Table 1, it is clear that individuals' positions on military issues, such as defense spending, nuclear policy, and military involvement, are better accounted for by the three postures than are the nonmilitary issues of international trade and Soviet policy. The differences here may be due to either or both of two reasons. First, it may be that the majority of postures included in this study (i.e., *militarism* and *anticommunism*) are more relevant guides for evaluating military, than for evaluating nonmilitary, issues. Yet it is also difficult to imagine general principles other than the postures measured here that might be used by the average citizen to anchor opinions on issues like international trade and diplomatic relations with the Soviet Union solidly. Liberal-conservative and partisan identifications, for example, provide no significant connective glue on these issues.

Moreover, it should be noted that *isolationism* and *anticommunism* did prove to be important predictors of these issues; the problem is simply that these dimensions failed to account for a substantial proportion of the variance on the non-military issues.

It seems more plausible to make a second argument, that military issues are more highly constrained because they are inherently more threatening to the public, are more often the object of media coverage, and are generally more salient in the mass public's mind. Because the public is more likely to think seriously about issues like military involvement and defense spending, these issues are more likely to be linked to general postures. Under normal circumstances, nonmilitary issues are, as a group, less likely to penetrate the mass public's awareness (Bennett 1974). It is therefore these nonsalient issues that come close to fitting the stereotype of foreign-policy attitudes found in much of the literature, where public thinking is portrayed as being relatively undeveloped and disorganized; though, even here, it is important to keep in mind that non-military issues in this study were not found to be completely random or disorganized, only more so than military issues.

Conclusions and Implications

How do individuals make sense out of a bewildering international environment, given the irrelevance of standard political heuristics? To answer this question, we resisted the assumption of unidimensional ideology by earlier studies, as well as the exploratory procedures of factor analysts. Instead, using an intensive, domain-specific approach, we found the foreign-policy views of respondents in our study to be constrained to an impressive degree. Thus, we find core values like ethnocentrism and the morality of war to structure foreign-policy postures like militar-

ism, anticommunism, and isolationism; which in turn are important determinants of individuals' preferences across a wide range of specific policies, such as defense spending, Soviet relations, involvement of U.S. troops overseas, international trade, and nuclear arms. This means that earlier findings that international attitudes are unrelated to a liberal-conservative dimension should not be interpreted to mean that foreign-policy attitudes lack structure. On the contrary, the general attitudes that constrain specific preferences are distinctive to the foreign-policy domain.

Critics might well argue that it is contradictory to view the electorate as structured and organized in its thinking when, at the same time, it is largely ignorant of the substance of such attitudes (we do *not* challenge the robust finding that citizens know little about foreign affairs). Sniderman and Tetlock (1986, 79) ask the question, "How is it possible for ordinary citizens to put together a consistent outlook on politics, given that they know so little about it?" They (and we) answer this question by emphasizing the functional nature of heuristics; that is, individuals organize information because such organization helps to simplify the world. Thus, a paucity of information does not *impede* structure and consistency; on the contrary, it *motivates* the development and employment of structure. Thus, we see individuals as attempting to cope with an extraordinarily confusing world (with limited resources to pay information costs) by structuring views about specific foreign policies according to their more general and abstract beliefs.

Our findings should be a source of comfort to those who appreciate the benefits of public involvement in governmental affairs. When Converse initiated the debate over constraint more than two decades ago, he justified the importance of the research by referring to the inherent benefits of mass-elite communication.

When the attitudes citizens possess are not consistent nor tied together by an underlying ideology, the public will not be able to follow the themes and rhetoric of candidates and incumbents, and, moreover, the elected officials will not readily understand what general public impulses helped to carry them to power. The results of this study suggest that the public does possess general orientations that help to inform and anchor its opinions on specific foreign-policy issues.

The results of this study should also provide some relief to analysts who have been frustrated by the seeming "random" nature of political behavior in the international realm, where traditional perspectives for explaining behavior in domestic politics have not been very useful. The postures that respondents used to anchor their specific policy preferences should prove to be important analytical yardsticks for future efforts to interpret and predict mass reactions to foreign-policy events. It is both more powerful and more parsimonious for students of political behavior to examine these general orientations than to focus on the opinions to specific issues, which change over time.

Our study also provides some new insights into the question of when, and under what circumstances, the public will become interested (or disinterested) in world affairs and which issue preferences are likely to be more well developed than others. Some of the specific policies, such as defense spending, military involvement, and nuclear policy, are clearly better explained by the higher-level beliefs (postures) than are others (i.e., the non-military issues). We speculate that one of the reasons why foreign policy appears to be salient during the current administration is that President Reagan has consistently spoken to precisely those military issues most likely to be constrained by the commonly shared postures. In comparison, President Carter focused primarily on less accessible and less salient foreign-

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policy issues—on human rights and non-military international relations—which do not appear to be constrained by the postures. Thus, political elites who focus on military concerns or who frame their appeals in terms of popular themes, like militarism, anticommunism, and isolationism, are at least assured of a wider public audience. Taking some exception with Almond's (1950) earlier conclusions, we would argue that foreign policy does not necessarily have to "shout loudly to be heard only a little"; it simply has to speak clearly and in familiar terms, perhaps rattling a few sabers for emphasis.

Finally, there are several limitations on the present study that deserve special mention. In the first place, our sample overrepresents better-educated individuals, who may be expected to exhibit a greater degree of interconnectedness on political attitudes. Further, we must acknowledge the possibility that constraint is higher during an administration noted for its controversial, bold, and consistent style of international relations; there is little mistaking the president's feelings toward the Soviets, toward military buildup, or toward support for regimes friendly to the U.S. We mention these caveats not because we believe our findings are inherently time bound or lacking in external validity but rather because we recognize the need to replicate these results across different regimes and different populations.

Appendix: Lexington Survey Items

For *Some people . . . Others . . .* statements, respondents were asked whether they *agreed* or *disagreed* with the statement and whether they agreed or disagreed *strongly* or *not so strongly*, producing a five-point scale with *uncertain* and *don't know* responses at the midpoint of the scale. For straightforward state-

ments, respondents were asked which viewpoint came closer to their own opinion and whether it was *somewhat close* or *very close* to their own view, producing a five-point scale with *depends* and *don't know* responses at the midpoint of the scale. *Don't know* responses were assigned a value equal to the midpoint of the scale for all of the five-point scales. On average, the percentage of *don't know* responses was fairly small—just above 3%, or about 16 cases—ranging from a low of 1% to an atypical high of 12%.

General Postures

A. Militarism

1. Some people feel that in dealing with other nations our government should be strong and tough. Others feel that our government should be understanding and flexible.
2. Some people feel that the best way to ensure peace is through military strength. Others feel that the best way to peace is to sit down with other nations and work out our disagreements.
3. The U.S. should maintain its dominant position as the world's most powerful nation at all costs, even going to the brink of war if necessary.

B. Anticommunism

1. The United States should do everything it can to prevent the spread of communism to any other part of the world.
2. The United States should do everything it can to check the spread of Soviet influence in the world.
3. Communism may have its problems, but it is an acceptable form of government for some countries.
4. Some people say that our government should try very hard to get along with Russia. Others say that

it would be a mistake to try very hard to get along with Russia.

C. Isolationism

1. We shouldn't risk our happiness and well-being by getting involved with other nations.
2. The United States shouldn't worry about world affairs but just concentrate on taking care of problems here at home.

Specific Issues

D. Defense Spending

1. Considering the situation today at home and abroad, do you think the total amount the United States is spending for defense and military purposes should be increased, kept at the present level, reduced, or ended altogether?
2. In your opinion, what should be the government's main approach in reducing the federal deficit? If you had to choose, would you rather see the government cut spending for defense or cut non-defense spending?

E. Military Involvement

1. Would you *strongly favor, not so strongly favor, not so strongly oppose, or strongly oppose* sending U.S. troops to Central America to stop the spread of communism?
2. Please tell me whether you would *strongly favor, not so strongly favor, not so strongly oppose, or strongly oppose* the use of U.S. troops if the Communists took over in El Salvador, Central America?

E. Nuclear Arms

1. Do you think the United States should freeze the production of nuclear weapons on its own, do so only if the Soviet Union agrees to a nuclear freeze as well, or do you think the United States should not

freeze the production of nuclear weapons at all?

2. Would you *strongly favor, not so strongly favor, not so strongly oppose, or strongly oppose* building more nuclear weapons to increase the U.S. arsenal?

G. Soviet Relations

1. Would you *strongly favor, not so strongly favor, not so strongly oppose, or strongly oppose* resuming cultural and educational exchanges with the Soviet Union?

H. International Trade

1. Some people feel we should restrict the sale of foreign products like Japanese cars in the U.S. to protect American jobs. Others feel restrictions are wrong because they lead to higher prices for American products at home.
2. Would you *strongly favor, not so strongly favor, not so strongly oppose, or strongly oppose* restricting foreign imports to protect American jobs?

Core Values

I. Ethnocentrism

1. Other countries should try to make their governments as much like ours as possible.
2. While the American form of government may not be perfect, it is the best form of government yet devised.
3. Many other countries have governments that are just as good as ours.
4. How do you think the United States compares with other countries in the world? Do you think that it is really no better than many other countries, that it is better than many countries but not necessarily the best, or that it is absolutely the best country in the world?

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J. Morality of War

1. It is certainly acceptable to kill one's enemy when fighting for one's country.
2. A person who loves his fellow man should refuse to fight in any war.

Notes

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1. It is not our intention to engage in Converse bashing. Because of his pioneering work in the field, all participants in the constraint debate are intellectually indebted to him.

2. Using data from a five-wave panel, Feldman (1985) also finds responses to domestic and foreign-policy issues to be almost perfectly stable. In addition, Feldman makes the point that the practice of measuring response stability with correlation coefficients (as practiced by Converse [1964]; and Converse and Markus [1979]) can seriously underestimate the true stability of foreign-policy attitudes because such coefficients are sensitive to the variance of the underlying variables. Because responses to domestic-affairs questions tend to be more polarized and more variable than responses to foreign-affairs items, the over-time correlations for domestic attitudes are much higher, even though they are actually no more stable than foreign-policy responses.

3. As an example, Bardes and Oldendick (1978) factor-analyze an initial pool of several hundred items to generate five factors—*militarism*, *non-military involvement*, *world problems*, *détente*, and *international organizations*—which, they argue, organize the specific foreign-policy attitudes of most individuals.

4. We do not use this term in the same sense as Carmines and Stimson (1980).

5. See Brady and Sniderman's (1985) recent article on the tendency for people to use their affect toward various groups as simple guides for placing themselves and these groups on various issue dimensions, a method the authors term the *likability heuristic*.

6. The demographic characteristics of the sample come reasonably close to approximating those found in the Lexington population. According to the 1980 census, blacks comprise 13% of the Lexington population; our sample includes the same percentage of blacks. The percentage of women in Lexington is 52%; in our sample the percentage is 50%. More educated and affluent individuals are slightly over-

represented in our sample. The median income in Lexington is \$15,915, while the median-income category in the sample is \$20,000–\$30,000 (the median is located at the lower end of this category, however). Respective *population* and *sample* percentages for education are *less than high school*, 29% and 14%; *completed high school*, 28% and 26%; *some college*, 18% and 26%; and *graduated college*, 25% and 34%.

7. We examined two additional postures that proved to be rather poor predictors of specific policy preferences. Both postures—nationalism and human rights—are attempts to tap the trade-off between national interests and moral considerations. The dimension of nationalism is anchored by the desire to promote the United States' interests overseas versus the desire to balance U.S. interests with the legitimate claims of other nations. Human rights was measured by agreement with the statement that the "U.S. should not support governments that abuse the human rights of its citizens, even if that country could help us militarily or economically." Neither posture proved useful as an explanation of individuals' preferences on specific foreign-policy issues.

8. Observers of the U.S. people as early as Tocqueville (1955, 235) have commented on our ethnocentric mentality: "The inhabitants of the United States . . . have an immediately high opinion of themselves and are not far from believing that they form a species apart from the rest of the human race."

9. LISREL combines elements of factor analysis found in psychometrics with structural-equation modeling found in path analysis and econometrics. In the factor-analysis portion of the LISREL procedure, the analyst specifies a measurement model where error-free latent variables are constructed from several imperfectly measured indicators. In the structural-equation portion of the LISREL model, the causal relations between the latent variables are specified. By analyzing the covariances between all the observed variables, LISREL uses maximum-likelihood-estimation techniques to obtain, simultaneously, estimates of the epistemic correlations between the latent variables and their indicators and estimates of the relationships between the latent variables.

10. The model assumes that attitudes at the same level of abstraction are related to one another only by their common dependence on more abstract attitudes. Since this assumption did not appear to be justified in the case of defense spending and nuclear policy (i.e., making cuts in the nuclear arsenal may imply making cuts in defense spending for many respondents), we allowed the disturbances of the equations for these two variables to be correlated by freeing an off-diagonal element in the *psi*-matrix of the LISREL model. To anticipate the estimation of the model below, the standardized correlated error term between these two attitudes is .25, indicating

that there is a significant source of (residual) covariation between defense-spending attitudes and nuclear-policy attitudes that is not accounted for by the three postures. We found little evidence of similar correlated disturbances between other attitudes in the model.

11. Wheaton, et al. (1977) consider a *chi-square*-to-degrees-of-freedom ratio of about 5.0 or less as "beginning to be reasonable." Two other goodness-of-fit statistics (not reported in Table 1)—the adjusted goodness-of-fit index (.893) and the root-mean-square residual (.074)—also show little evidence of model weakness.

12. The impact of ideological self-placement on militarism is .13, significant at the .05 level. Significant covariances are also found between liberalism-conservatism and ethnocentrism (.20), morality of warfare (.15), and party identification (.22).

13. Though anticommunism and militarism are related, we found little evidence of multicollinearity as a problem in these data, judging from the magnitude of the correlations between estimates of the parameters of the model (provided as part of the technical output available in LISREL 6).

14. A closer look at the foreign-policy attitudes typically used to illustrate the stereotypical view shows a preponderance of nonmilitary types of issues, such as diplomatic relations with China (e.g., Erikson, Luttbeg, and Tedin 1980) and foreign-aid policy (Converse and Markus 1979). Bennett (1974) has also noted that more salient foreign-policy-issue attitudes are more likely to be organized in the public's mind.

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POLITICAL CLEAVAGES AND CHANGING EXPOSURE TO TRADE

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Combining the classical theorem of Stolper and Samuelson with a model of politics derived from Becker leads to the conclusion that exogenous changes in the risks or costs of countries' external trade will stimulate domestic conflict between owners of locally scarce and locally abundant factors. A traditional three-factor model then predicts quite specific coalitions and cleavages among owners of land, labor, and capital, depending only on the given country's level of economic development and its land-labor ratio. A preliminary survey of historical periods of expanding and contracting trade, and of such specific cases as the German "marriage of iron and rye," U.S. and Latin American populism, and Asian socialism, suggests the accuracy of this hypothesis. While the importance of such other factors as cultural divisions and political inheritance cannot be denied, the role of exogenous changes in the risks and costs of trade deserves further investigation.

Why countries have the political cleavages they do and why those cleavages change are among the enduring mysteries of comparative politics. Among the many factors that have been adduced as partial explanations are preexisting cultural and religious divisions, the rapidity and timing of industrialization or of the grant of mass suffrage, the sequence of "crises" of modernization, the electoral system, and—most recently—the product cycle (see, *inter alia*, Binder et al. 1971; Duverger 1959; Kurth 1979a, 1979b; Lipset and Rokkan 1967; Rokkan 1970, 1981).

Without denying the importance of any of these variables, I want to suggest the relevance of a factor that has, until now, been widely neglected: externally induced changes—in countries with different factor endowments—in exposure to international trade.

To be sure, some studies of individual countries, and even a few comparative inquiries, have argued the significance of changing international trade in particular circumstances: one thinks, in particular, of Abraham 1981, Gerschenkron 1943, Gourevitch 1977 and 1986, Rosenberg 1943, Sunkel and Paz 1973. One author, Cameron (1978), has even suggested a relation, at least in recent decades, between exposure to trade and the rate of growth in state expenditure.

Arguing much more generally, I shall try to show that basic results of the theory of international trade—including, in particular, the well-known Stolper-Samuelson Theorem (Stolper and Samuelson 1941)—imply that increases or decreases in the costs and difficulty of international trade should powerfully affect domestic political cleavages and should do so differently, but predictably,

in countries with different factor endowments. Moreover, I shall suggest that these implications conform surprisingly well with what has been observed about patterns of cleavage and about changes in those patterns in a great variety of countries during four periods of global change in exposure to trade, namely the "long" sixteenth century, the nineteenth century, the Depression of the 1930s, and the years since World War II.

Nonetheless, what I present here remains conjectural and preliminary. The evidence I shall be able to advance is suggestive rather than conclusive. It is principally the clarity of the logical case that seems to me to justify further refinement and testing.

The Stolper-Samuelson Theorem

In 1941 Wolfgang Stolper and Paul Samuelson solved conclusively the old riddle of gains and losses from protection (or, for that matter, from free trade). They showed that in any society protection benefits—and liberalization of trade harms—owners of factors in which that society is *poorly* endowed, relative to the rest of the world, as well as producers who use the scarce factors intensively.¹ Conversely, protection harms—and liberalization benefits—owners of factors the given society holds *abundantly* relative to the rest of the world, and producers who use the abundant factors intensively.² Thus, in a society rich in labor but poor in capital, protection would benefit capital and harm labor; and liberalization of trade would benefit labor and harm capital.

So far, the theorem is what it is usually perceived to be: merely a statement, if an important and sweeping one, about the effects of tariff policy. The picture is altered, however, when one realizes that exogenous changes can have exactly the same effects as increases or decreases in protection. A cheapening of transport

costs, for example, is indistinguishable in its impact from an across-the-board decrease in every affected state's tariffs (Mundell 1957, 330); so is any change in the international regime that decreases the risks or the transaction costs of trade. The converse is of course equally true: when a nation's external transport becomes dearer, or its trade less secure, it is affected exactly as if it had imposed a higher tariff.

The point is of more than academic interest because we know, historically, that major changes in the risks and costs of international trade have occurred: notoriously, the railroads and steamships of the nineteenth century brought drastically cheaper transportation (Landes 1969, 153–54, 196, 201–2; Hobsbawm 1979, Chap. 3); so, in our own generation, did supertankers, cheap oil, and containerization (Rosecrance 1986, 142). According to the familiar argument of Kindleberger (1973) and others, international hegemony decreases both the risks and the transaction costs of international trade; and the decline of hegemonic power makes trade more expensive, perhaps—as, according to this interpretation, in the 1930s—prohibitively so. Analyzing a much earlier period, the Belgian historian Henri Pirenne (1939) attributed much of the final decline of the Roman Empire to the growing insecurity of interregional, and especially of Mediterranean, trade after 600 A.D.³

Global changes of these kinds, it follows, should have had global consequences. The "transportation revolutions" of the sixteenth, the nineteenth, and scarcely less of the mid-twentieth century must have benefited, in each affected country, owners and intensive employers of locally abundant factors and must have harmed owners and intensive employers of locally scarce factors. The events of the 1930s should have had exactly the opposite effect. What, however, will have been the *political* consequences of those shifts

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of wealth and income? To answer that question we require a rudimentary model of the political process and a somewhat more definite one of the economy.

Simple Models of the Polity and the Economy

I shall assume of domestic political processes only two things: (1) that the beneficiaries of a change will try to continue and accelerate it, while the victims of the same change will endeavor to retard or to halt it; and (2) that those who enjoy a sudden increase in (actual or potential)⁴ wealth and income will thereby be enabled to expand their political influence as well (cf. Becker 1983). As regards international trade, (1) implies that the gainers from any exogenous change will seek to continue and to expand free trade, while the losers will seek protection (and, if that fails, imperialism);⁵ (2) implies that those who gain, or are positioned to gain, economically from exogenous changes in international trade will increase their political power as well.

Economically, I propose to adopt with minor refinements the traditional three-factor model—land, labor, and capital—and to assume, for now, that the land-labor ratio informs us fully about any country's endowment of those two factors. No country, in other words, can be rich both in land and in labor: a high land-labor ratio implies abundance of land and scarcity of labor; a low ratio signifies the opposite. (I shall later relax this assumption.) Finally, I shall simply define an *advanced* economy as one in which capital is abundant.

This model of factor endowments inevitably oversimplifies reality and will require amendment. Its present simplicity, however, permits us in theory to place any country's economy into one of four cells (see Figure 1), according to (1)

whether it is advanced or backward and (2) whether its land-labor ratio is high or low. We recognize, in other words, only economies that are (1) capital rich, land rich, and labor poor; (2) capital rich, land poor, and labor rich; (3) capital poor, land rich, and labor poor; or (4) capital poor, land poor, and labor rich.

Political Effects of Increasing Exposure to Trade

I shall now try to demonstrate that the Stolper-Samuelson Theorem, applied to our simple model, implies that increasing exposure to trade must result in *urban-rural conflict* in two kinds of economies and in *class conflict* in the two others.

Consider first the upper right-hand cell of Figure 1: the advanced (therefore capital-rich) economy endowed abundantly in labor but poorly in land. Expanding trade must benefit both capitalists and workers; it harms only land-owners and the pastoral and agricultural enterprises that use land intensively. Both capitalists and workers—that is to say, almost the entire urban sector—should favor free trade; agriculture should on the whole be protectionist. Moreover, we expect the capitalists and the workers to try, very likely in concert, to expand their political influence. Depending on pre-existing circumstances, they may seek concretely an extension of the franchise, a reapportionment of seats, a diminution in the powers of an upper house or of a gentry-based political elite, or a violent "bourgeois" revolution.

Urban-rural conflict should also arise in backward, labor-poor economies (the lower left-hand cell of Figure 1) when trade expands, albeit with a complete reversal of fronts. In such "frontier" societies, both capital and labor are scarce: hence both are harmed by expanding trade and will seek protection. Only land is abundant, and therefore only agri-

Figure 1. Four Main Types of Factor Endowments

		Land-Labor Ratio	
		High	Low
Advanced Economy		Abundant: Capital Land Scarce: Labor	Abundant: Capital Labor Scarce: Land
		Abundant: Land Scarce: Capital Labor	Abundant: Labor Scarce: Capital Land
Backward Economy			

culture will gain from free trade. Farmers and pastoralists will try to expand their influence in some movement of a "Populist" and antiurban stripe.

Conversely, in backward economies with low land-labor ratios (the lower right-hand cell of Figure 1), land and capital are scarce and labor is abundant. The model therefore predicts *class conflict*: labor will pursue free trade and expanded political power (including, in some circumstances, a workers' revolution); landowners, capitalists, and capital-intensive manufacturers will unite to support protection, imperialism, and a politics of continued exclusion. (Lest the picture of a rising in support of freer markets seem too improbable a priori, I observe at once its general conformity with Popkin's 1979 astute interpretation of the Vietnamese revolution.)

The reverse form of class conflict is expected to arise in the final case, that of an advanced but land-rich economy (the upper left-hand cell of Figure 1) under increasing exposure to trade. Because both capital and land are abundant, capitalists, capital-intensive industries, and agriculture will all benefit from, and will endorse, free trade; labor being

scarce, workers and labor-intensive industries will embrace protection and (if need be) imperialism. The benefited sectors will seek to expand their political power, if not by disfranchisement then by curtailment of workers' economic prerogatives and suppression of their organizations.

These implications of the theory of international trade (summarized in Figure 2) seem clear, but do they in any way describe reality? I shall address that question more fully below, but for now it is worth observing how closely the experience of three major countries—Germany, Britain, and the United States—conforms to this analysis in the period of rapidly expanding trade in the last third of the nineteenth century; and how far it can go to explain otherwise puzzling disparities in those states' patterns of political evolution.

Germany and the United States were both still relatively backward, that is, capital-poor, societies: both, in fact, imported considerable amounts of capital in this period (Feis 1965, 24-25 and Chap. 3). Germany, however, was rich in labor and poor in land; the United States, of course, was in exactly the opposite posi-

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Figure 2. Predicted Effects of Expanding Exposure to Trade

		Land-Labor Ratio	
		High	Low
Advanced Economy		Class cleavage: Land and capital free-trading, assertive Labor defensive, protectionist	Urban-rural cleavage: Capital and labor free-trading, assertive Land defensive, protectionist (Radicalism)
		Urban-rural cleavage: Land free-trading, assertive Labor and capital defensive, protectionist (U.S. Populism)	Class cleavage: Labor free-trading, assertive Land and capital defensive, protectionist (Socialism)
Backward Economy			

tion. Again, the demonstration is easy: the United States imported—and Germany exported (not least to the United States)—workers.⁶ The theory, of course, predicts class conflict in Germany, with labor the “revolutionary” and free-trading element and with land and capital united in support of protection and imperialism. Surely this description will not ring false to any student of German socialism or of Germany’s infamous “marriage of iron and rye.”⁷ For the United States, conversely, the theory predicts—quite accurately, I submit—urban-rural conflict, with the agrarians now assuming the “revolutionary” and free-trading role and with capital and labor uniting in a protectionist and imperialist coalition. E. E. Schattschneider (1960) or Walter Dean Burnham (1970) could hardly have described more succinctly the history of populism and of the election of 1896.⁸

Britain, on the other hand, was already an advanced economy in the later nineteenth century, one in which capital was so abundant that it was exported in vast quantities (Feis 1965, Chap. 1). That it was also rich in labor is demonstrated by

its extensive exports of that factor to the United States, Canada, Australia, New Zealand, and Africa.⁹ Britain therefore falls into the upper right-hand quadrant of Figure 1 and is predicted to exhibit a rural-urban cleavage, with fronts opposite to those found in the United States: capitalists and labor unite in support of free trade and in demands for expanded political power, while landowners and agriculture support protection and imperialism.

While this picture surely obscures important nuances, it illuminates a crucial difference between Britain and, for example, Germany in this period: in Britain, capitalists and labor *did* unite effectively in the Liberal party and forced an expanded suffrage and curtailment of (still principally landowning) aristocratic power; in Germany, with liberalism shattered (Sheehan 1978), the suffrage for the powerful state parliaments was actually contracted, and—far from eroding aristocratic power—the bourgeoisie grew more and more *verjunkt* in style and aspirations.

Political Effects of Declining Exposure to Trade

When declining hegemony or rising costs of transportation substantially constrict external trade, the gainers and losers are simply the reverse of those under increasing exposure to trade: owners of locally scarce factors prosper, owners of locally abundant ones suffer. The latter, however, can invoke no such simple remedy as protection or imperialism; aside from tentative "internationalist" efforts to restore orderly markets (Gourevitch 1986, Chap. 4), they must largely accept their fate. Power and policy, we expect, will shift in each case toward the owners and intensive users of scarce factors.

Let us first consider the situation of the highly *developed* (and therefore, by our earlier definition, capital-rich) economies. In an economy of this kind with a high land-labor ratio (the upper left-hand cell of Figure 1), we should expect intense *class* conflict precipitated by a newly aggressive working class. Land and capital are both abundant in such an economy; hence, under declining trade, owners of both factors (and producers who use either factor intensively) lose. Labor being the only scarce resource, workers are well positioned to reap a significant windfall from the protection that dearer or riskier trade affords; and, according to our earlier assumption, like any other benefited class they will soon try to parlay their greater economic into greater political power. Capitalists and landowners, even if they were previously at odds, will unite to oppose labor's demands.

Quite to the contrary, declining trade in an advanced economy that is labor rich and land poor (the upper right-hand cell of Figure 1) will entail *urban-rural* conflict. Capital and labor, being both abundant, are both harmed by the contraction of external trade. Agriculture, as the

intense exploiter of the only scarce factor, gains significantly and quickly tries to translate its gain into greater political control.

Urban-rural conflict is also predicted for backward, land-rich countries under declining trade; but here agriculture is on the defensive. Labor and capital being both scarce, both benefit from the contraction of trade; land, as the only locally abundant factor, retreats. The urban sectors unite, in a parallel to the "radical" coalition of labor-rich developed countries under expanding trade, to demand an increased voice in the state.

Finally, in backward economies rich in labor rather than land, class conflict resumes, with labor this time on the defensive. Capital and land, as the locally scarce factors, gain from declining trade; labor, locally abundant, loses economically and is soon threatened politically.

Observe again, as a first test of the plausibility of these results—summarized in Figure 3—how they appear to account for some prominent disparities of political response to the last precipitous decline of international trade, the Depression of the 1930s. The U.S. New Deal represented a sharp turn to the left and occasioned a significant increase in organized labor's political power. In Germany, a depression of similar depth (gauged by unemployment rates and declines in industrial production [Landes 1969, 391]) brought to power first Hindenburg's and then Hitler's dictatorship. In both, landowners exercised markedly greater influence than they had under Weimar (Abraham 1981, 85–115 and Chap. 4; Gessner 1977); and indeed a credible case can be made out that the rural sector was the principal early beneficiary of the Nazi regime (see, *inter alia*, Gerschenkron 1943, 154–63; Gies 1968; Holt 1936, 173–74, 194ff.; Schoenbaum 1966, 156–63).¹⁰ Yet this is exactly the broad difference that the model would lead us to anticipate if we accept that by 1930 both countries were

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Figure 3. Predicted Effects of Declining Exposure to Trade

		Land-Labor Ratio	
		High	Low
Advanced Economy	Class cleavage: Labor gains power. Land and capital lose. (U.S. New Deal)		Urban-rural cleavage: Land gains power. Labor and capital lose. (Western European Fascism)
Backward Economy	Urban-rural cleavage: Labor and capital gain power. Land loses. (South American Populism)		Class cleavage: Land and capital gain power. Labor loses. (Asian & Eastern European Fascism)

economically advanced—although Germany, after reparations and cessions of industrial territory, was surely less abundant in capital than the United States—but the United States remained rich in land, which in Germany was scarce. Only an obtuse observer would claim that such factors as cultural inheritance and recent defeat in war played no role; but surely it is also important to recognize the sectoral impact of declining trade in the two societies.¹¹

As regards the less-developed economies of the time, it may be profitable to contrast the Depression's impact on such South American cases as Argentina and Brazil with its effects in the leading Asian country, Japan. In Argentina and Brazil, it is usually asserted (Cardoso and Faletto 1979, 124–26 and Chap. 5; Skidmore and Smith 1984, 59–60; Sunkel and Paz 1973, 352–54), the Depression gave rise to, or at the least strengthened, "Populist" coalitions that united labor and the urban middle classes in opposition to traditional, landowning elites. In Japan, growing military influence suppressed representative institutions and nascent workers' organizations, ruling in the interest—albeit

probably not under the domination—of landowners and capitalists (Kato 1974; Reischauer 1974, 186–87, 195–99). (Similar suppressions of labor occurred in China and Vietnam [Clubb 1972, 135–40; Popkin 1979, xix, 215].)

In considering these contrasting responses, should we not take into account that Argentina and Brazil were rich in land and poor in labor (recall the extent of immigration, especially into Argentina), while in Japan (and, with local exceptions, in Asia generally) labor was abundant and land was scarce (respectively, the lower left- and right-hand cells of Figure 3)?

A Preliminary Survey of the Evidence

I want now to undertake a more systematic, if still sketchy, examination of the historical evidence that bears on the hypotheses developed here. This effort will serve principally to suggest directions for further research; it can in no way be described as conclusive.

The "Long" Sixteenth Century

It has long been recognized that improvements in navigation and shipbuilding permitted, from about 1450 on, a previously unimagined expansion of trade, which eventuated in the European "discovery" and colonization of the Americas (Cipolla 1965). Among social scientists, Immanuel Wallerstein (1974) has studied this period most intensively; and it is worth emphasizing that the present analysis conforms with essential aspects, and, indeed, permits some clarification, of his.

Within the context of the age, what Wallerstein calls the *core* economies of the new world system—those, essentially, of northwestern Europe—were defined by their abundance in capital and labor, and by their relative scarcity of land. The *periphery* can be described as the exact inverse: rich in land, poor in both capital and—often leading to the adoption of slavery or serfdom—labor. Under expanding trade, the regimes of the core come to be dominated by a "bourgeois" coalition of capital and skilled labor (the Dutch Republic, the Tudors), and of the manufactures that use both intensively; the older, landed elites lose ground. Conversely, in the periphery, land—in the persons of plantation owners and *Gutsherren*—suppresses both capital and labor and, indeed, almost all urban life.

So far the equation seems apt. Can we, however, not go on to define that Wallersteinian chimera, the *semiperiphery* (Wallerstein 1974, 102–7), as comprising economies that fall into the lower right-hand cell of Figure 1, economies poor in capital and land, rich in labor? That would, I suspect, accurately describe most of the southern European economies in this period; and it would correctly predict (see again Figure 2) the intense class conflict (including the German Peasants War [Moore 1967, 463–67]) and the wholly retrograde and protectionist policies adopted by a peculiarly united class of

landowners and capitalists in many of these regions.¹²

The Nineteenth Century

We can again proceed regionally, generalizing on the sketch of Britain, Germany, and the United States developed earlier for this period. For the period just before the great cheapening of transportation—roughly at the middle of the nineteenth century¹³—Britain can stand as the surrogate for the advanced and labor-rich economies of northwest Europe generally, including Belgium, the Netherlands, and northern France (Hobsbawm 1962, Chap. 9; Landes 1969, Chap. 3). For this whole region, as for Britain, the model predicts that expanding trade would engender rural-urban conflict: capitalists and workers, united in support of free trade and greater urban influence, oppose a more traditional and protectionist landed sector. It does not seem to me farfetched to see the powerful liberalism and radicalism of this whole region in the later nineteenth century (Carstairs 1980, 50, 62; Cobban 1965, 21–28, 58–67; Daalder 1966, 196–98; Lorwin 1966, 152–55)—or, for that matter, much of the conflict between secularism and clericalism—in this light.

Almost all of the rest of Europe at the dawn of this period can be compared with Germany: poor in capital and in land, rich in labor.¹⁴ (The land-labor ratio seems as a rule to have declined as one moved from north to south within the economically backward regions of Europe [see figures for 1846 in Bowden, Karpovich, and Usher 1937, 3].) As it does for Germany, the model predicts for these other countries, particularly in southern Europe, class conflict as a consequence of increasing exposure to trade: workers (including agricultural wage laborers) press for more open markets and greater influence; capitalists and landowners unite in support of protection and more

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traditional rule. In its main aspect, this seems to me only a restatement of a central tendency that has long been remarked, namely that class conflict in the nineteenth century came at an earlier phase of industrialization, and more bitterly, to southern and central than to northwestern Europe (e.g., Lipset 1970, 28–30; Macridis 1978, 485–87; cf. Thomson 1962, 375–78); and it seems to me a more credible account of these regions' extremism than Duverger's (1959, 238) famous invocation of an allegedly more mercurial "Latin" temperament.

The United States, finally, represents the land-rich, but labor- and capital-poor "frontier societies" of this period generally: most of both Americas, Australia, New Zealand, even those parts of central and southern Africa that would soon be opened to commercial agriculture. Here, expanding trade benefits and strengthens landowners and farmers against protectionist capitalists and workers (although, as in the United States, the protectionist forces may still prevail); rural-urban conflict ensues, precipitated by demands from the rural sector.

Again, this does not at first glance appear wide of the mark. In many of the Latin American societies, this period cemented landed rule (Skidmore and Smith 1984, 50; Sunkel and Paz 1973, 306–21); in the United States and Canada, it was characterized by conflicts between the industrial East and the agricultural West (Easterbrook and Aitken 1958, 503–4); in almost wholly agricultural Australia, trade precipitated a cleavage between free-trading landowners and increasingly protectionist rural and urban wage labor (Gollan 1955, esp. 162–69; Greenwood 1955, 216–20).

In all of these cases, as I have emphasized before, other factors were surely at work and important aspects are neglected by the present analysis; but it is essential also not to ignore the benefits and costs of expanding trade to the various sectors.

The Depression of the 1930s

Here the fit between theory and reality seems quite strong. Not only the United States but Canada, Australia, and New Zealand were by this time advanced, land-rich economies. Labor, their only scarce factor, gained from the collapse of international trade: workers became more militant, policy shifted to the left. Most Latin American societies remained land-rich but backward; and for them this was quite generally the period of "Populist" coalitions of the two scarce factors, labor and capital. In developed northern Europe, owners and exploiters of the locally scarce factor of land grew more assertive, and generally more powerful, wherever previous developments had not caused them to disappear; capitalists and workers lost ground. Finally, throughout the backward regions of the world economy, where labor was abundant and land was scarce—not only in Asia but in southern and eastern Europe—labor lost to a renascent coalition of the locally scarce factors of land and capital: in Spain, Italy, Rumania, Hungary, and Poland, to name only the most prominent cases (Carsten 1967, Chaps. 2, 5 and pp. 194–204).

After World War II

Under U.S. hegemony, and with new economies in transportation and communication, the West since World War II has experienced one of history's more dramatic expansions of international trade (Organization for Economic Cooperation and Development [OECD] 1982, 62–63). Again, the theory would lead us to expect different regional consequences.

In the developed, labor-rich and land-poor economies—including now not only most of Europe but Japan—the model would predict an "end of ideology," at least as regards issues of class: labor and capital, both beneficiaries of expanding

trade, unite to advance it and to oppose any remaining pretensions to rule by the landowning groups.¹⁵ Conversely, in the land-rich and still underdeveloped economies of Latin America, expanding trade displaces the Depression-era "Populist" coalitions of labor and capital and brings renewed influence to the landed sectors. The areas of Asia and of southern Europe that are economically backward and abundant only in labor experience labor militancy and, in not a few cases, revolutionary workers' movements. Finally, and perhaps more as a statement about the future, the few economies rich in both capital and land—principally those of North America, Australia, and New Zealand—should, as they become seriously exposed to international trade, experience class conflict and a considerable suppression of labor. Capital and agriculture will for the most part unite in support of the free trade that benefits them; labor, as the locally scarce factor, will favor protection and imperialism.

Further Implications

To the extent that the model has gained any credibility from the foregoing brief survey, it may be useful to observe some of its other implications for disciplinary riddles and conjectures. Take first Gerschenkron's (1962) observation, and Hirschman's (1968) subsequent challenge and amendment of it, that "latecomers" to economic development tend to assign a stronger role to the state. From the present perspective, what should matter more, at least among labor-rich economies, is whether development *precedes* or *follows* significant exposure to trade. In an economy that has accumulated abundant capital before it is opened to trade, capital and labor will operate in relative harmony, and little state intervention will be required. Where trade precedes development, assertive labor faces—as it did in Imperial Germany—the united opposition

of capitalists and landowners. To the extent that labor wins this struggle, it will require a strong state to administer the economy; to the extent that capital and land prevail, a state powerful enough to suppress labor is needed. Either route leads to a stronger state.

Even this generalization, however, applies only to economies where labor is abundant, and land scarce. Hence Hirschman's observation that "latecomers" in Latin America do not behave as Gerschenkron predicts should not surprise us. Where land is abundant, and labor scarce—as has generally been true of the Americas—"late" economic modernization (i.e., one that follows significant exposure to trade) radicalizes owners of *land* rather than owners of labor. In such "frontier" economies, labor and capital again find themselves in the same political camp, this time in support of protection. In the absence of class conflict, no powerful state is required.

This last point, of course, sheds some light on Sombart's old question, Why is there no socialism in the United States? If this model is right, the question is appropriately broadened to, Why is there no socialism in land-rich economies? Simply put, socialism develops most readily where labor is favored by rising exposure to trade and capital is not; labor is then progressive and capital is reactionary. But labor is never favored by rising trade where it is scarce. Powerful socialist movements, the present model suggests, are confined to backward and labor-rich economies under conditions of expanding trade (the less-developed European societies in the later nineteenth century, Asia after World War II).

A third riddle this approach may help resolve is that of the coalitional basis and aims of the North in the U.S. Civil War.¹⁶ As Barrington Moore, Jr. posed the question in a memorable chapter of *Social Origins of Dictatorship and Democracy* (1967, Chap. 3), What was the connection

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between *protection* and Free Soil in the platform of the Republican party or of the North more generally, and Why did so broad a coalition support both aims?

If, as seems apparent, labor was scarce in the United States, then the nineteenth century's increasing exposure to trade should have depressed, or at least retarded the advance of, wages. By definition, slaves already received a lower wage than they would voluntarily accept (Else, why coerce them?); and increased trade could reasonably be seen as intensifying, or at least as retarding the demise of, slavery. Conversely, protection in a labor-scarce economy might so raise the general wage level (while, paradoxically, also increasing returns to scarce capital) as to make manumission feasible. Hence to link protection and abolition might seem a wholly sensible strategy. Moreover, because protection in that period would benefit workers and capitalists generally, it could attract the support of a very wide coalition. At least some of the mystery seems dissolved.

Relaxing the Reliance on Land-Labor Ratios

For the sake of logical completeness, and to fill a nagging empirical gap, let us now relax the assumption that the land-labor ratio informs us completely about the relative abundance of these two factors. We admit, in other words, that a country may be rich or poor in *both* land and labor. Four new cases arise in theory if (as I suspect) rarely in practice (see Figure 4): economies may be, as before, advanced or backward (i.e., capital rich or capital poor); but they may now be rich in both land and labor or poorly endowed in both factors.

Two cases—that of the advanced economy rich in both factors and of the backward one poor in both—are theoretically improbable¹⁷ and politically uninterest-

ing: if all factors were abundant relative to the rest of the world, the society would unanimously embrace free trade; if all were scarce, it would agree on protection. Let us consider, then, the remaining two possibilities.

In an advanced economy where both land and labor are scarce, expanding trade will benefit only capital. Agriculture and labor—*green* and *red*—will unite in support of protection and, if need be, imperialism; only capitalists will embrace free trade. When trade contracts in such an economy, the scarce factors of land and labor gain, and capital loses, influence; farmers and peasants are likely to seek expanded mass participation in politics and a radical curtailment of capitalist power.

In a backward economy with abundant land and labor (a possibility considered explicitly by Myint [1958, 323]), change in exposure to trade again mobilizes a coalition of red and green, but with diametrically opposed positions. Expanding trade now *benefits* farmers and workers but *harms* capitalists; and the labor-landowner coalition pursues a wider franchise, free trade, and disempowerment of capital. Contracting trade, however, benefits only the owners of capital and injures both workers and farmers; again intense conflict between capital and the other two factors is predicted, ending in either a capitalist dictatorship or an anti-capitalist revolution.

It is tempting, if speculative in the extreme, to see in the red-green coalitions of Scandinavia in the 1930s (Gourevitch 1986, 131–35; Hancock 1972, 30–31; Rokkan 1966, 84) the natural response to trade contraction of (by then) capital-rich but land- and labor-poor economies; and, conversely, to view modern Russian history, at least until well after World War II, as that of a backward but land- and labor-rich economy,¹⁸ which, in a time of expanding trade, indeed forged an anti-capitalist coalition of peasants and work-

**Figure 4. Predicted Effects on Economies That Are Rich or Poor
in both Land and Labor**

	Land and Labor both Abundant	Land and Labor both Scarce
Advanced Economy	Ø	Expanding trade: Capital assertive, free-trading Land and labor protectionist, defensive Declining trade: Land and labor gain power. Capital loses.
Backward Economy	Expanding trade: Land and labor free-trading, assertive Capital defensive, protectionist Declining trade: Capital gains power. Land and labor lose.	Ø

ers and, when trade contracted, experienced (as Stalin's enemies alleged at the time) a dictatorship of state capital over both workers and farmers.

Certainly so long as we cling to the view that land can only be abundant where labor is not, and vice-versa, we can offer no trade-based account of red-green coalitions; indeed, changing exposure to trade must drive the two factors apart, for it always helps the one and hurts the other. On the one hand, this reflects reality—coalitions of labor and agriculture have been rare, and have failed even where much seemed to speak for them (e.g., in U.S. populism and on the German left); on the other, it leaves the few actual red-green coalitions, particularly those that arose in circumstances of changing exposure to trade, as standing refutations of the model.

Possible Objections

At least three objections can plausibly be raised to the whole line of analysis that

I have advanced here:

First and most fundamentally, it may be argued that the effects sketched out here will not obtain in countries that depend only slightly on trade. A Belgium, where external trade (taken as the sum of exports and imports) roughly equals GDP, can indeed be affected profoundly by changes in the risks or costs of international commerce; but a state like the United States in the 1960s, where trade amounted to scarcely a tenth of GDP, will have remained largely immune (OECD 1982, 62-63).

This view, while superficially plausible, is incorrect. The Stolper-Samuelson result obtains at any margin; and, in fact, holders of scarce factors have been quite as devastated by expanding trade in almost autarkic economies—one need think only of the weavers of capital-poor India or Silesia, exposed in the nineteenth century to the competition of Lancashire mills—as in ones previously more dependent on trade. (Cf. Thomson 1962, 163-64, on the vast dislocations that even slight exposure

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to trade occasioned in previously isolated areas of nineteenth-century Europe.)

Second, one can ask why the cleavages indicated here should persist. In a world of perfectly mobile factors and rational behavior, people would quickly disinvest from losing factors and enterprises (e.g., farming in Britain after 1880) and move to sectors whose auspices were more favorable. Markets should swiftly clear, and a new, if different, political equilibrium should be achieved.

To this, two answers may be given. First, in some cases trade expands or contracts so rapidly as to frustrate rational expectations. Especially in countries that experience a steady series of such exogenous shocks—Europe, for example, since 1840—divisions based on factor endowments (which ordinarily change only gradually)¹⁹ will be repeatedly revived. Second, often enough some factors' privileged access to political influence makes the extraction of rents and subsidies seem cheaper than adaptation: Prussian *Junker*, familiarly, sought (and, rather easily, won) protection rather than adjustment. In such circumstances, adaptation may be long delayed, sometimes with ultimately disastrous consequences.

Finally, it may be objected that I have said nothing about the outcome of these conflicts. I have not done so for the simple reason that I cannot: history makes it all too plain—as in the cases of nineteenth-century Germany and the United States—that the economic losers from trade may win politically over more than the short run. What I have advanced here is a speculation about *cleavages*, not about outcomes. I have asserted only that those who gain from fluctuations in trade will be strengthened and emboldened politically; nothing guarantees that they will win. Victory or defeat depends, so far as I can see, on precisely those institutional and cultural factors that this perspective so resolutely ignores.

Conclusion

I have not claimed that changes in countries' exposure to trade explain all, or even most, of their varying patterns of political cleavage. It would be foolish to ignore the importance of ancient cultural and religious loyalties, of wars and migrations, or of such historical memories as the French Revolution and the *Kulturkampf*. Neither have I offered anything like a convincing empirical demonstration of the modest hypotheses I have advanced; at most, the empirical regularities that I have noted or have taken over from such authorities as Gerschenkron and Lipset can serve to suggest the plausibility of the model and the value of further refinement and testing of it.

I have presented a theoretical puzzle, a kind of social-scientific "thought-experiment" in Hempel's (1965) original sense: a teasing out of unexpected and sometimes counterintuitive implications of theories already widely accepted (Chap. 7). For the Stolper-Samuelson Theorem is generally, indeed almost universally, embraced; yet, coupled with a stark and unexceptionable model of the political realm, it plainly implies that changes in exposure to trade must profoundly affect nations' internal political cleavages. Do they do so? If they do not, what is wrong—either with our theories of international trade or with our understanding of politics?

Notes

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1. In fact, the effect flows backward from products and is an extension of the Heckscher-Ohlin theorem: under free trade, countries export products whose manufacture uses locally abundant, and import products whose manufacture uses locally scarce, factors intensively (cf. Leamer 1984, esp. 8-10).

2. Admittedly, this result depends on simplifying assumptions that are never achieved in the real world, among them perfect mobility of factors within national boundaries, a world of only two factors and two goods, and incomplete specialization. Still, as an approximation to reality, it remains highly serviceable (cf. Ethier 1984, esp. 163-64, 181).

3. Later historians have, of course, largely rejected Pirenne's attribution of this insecurity to the rise of Islam and its alleged blockade of Mediterranean commerce (Havighurst 1958). It can hardly be doubted, however, that the decline of Roman power by itself rendered interregional trade far less secure.

4. As transportation costs fall, states may offset the effect by adopting protection. Owners of abundant factors then still have substantial *potential* gains from trade, which they may mortgage to pressure policy toward lower levels of protection.

5. Countries that lack essential resources can only beggar themselves by protection. Ultimately, those in such a society who seek protection from trade must advocate conquest of the missing resources—as indeed occurred in Japan and Germany in the 1930s.

6. Between 1871 and 1890, just under two million Germans emigrated to points outside Europe; in the same years, some seven million immigrants entered the United States (Mitchell 1978, Tbl. A-5; Williams, Current, and Freidel 1969, 158).

7. The Stolper-Samuelson analysis also helps to clear up what had seemed even to the perspicacious Gerschenkron (1943, 26-27) an insoluble riddle: why the *smallholding* German peasants had quickly become as protectionist as the *Junker*. Not only landowners, we now see, but all enterprises that *used land intensively*, will have been harmed by free trade. On the other hand—and later the distinction will become crucial—agricultural *wage labor* should have been free trading.

8. That the farmers of the Great Plains were hardly prospering in these years is no refutation of the analysis advanced here. Their *potential* gains were great (see n. 4), and their suffering could plausibly be attributed not to expanded trade but to the obstacles or exploitation laid upon that trade by

other sectors. As in Marxist analysis, the older relations of production and of politics could be seen as "fetters."

9. Emigrants from the United Kingdom to areas outside Europe totalled 5.1 million between 1871 and 1890 (see Mitchell 1978, Tbl. A-5).

10. Certainly they had been among its earliest and strongest supporters: virtually every study of late Weimar voting patterns (e.g., Brown 1982; Childers 1983; Lipset 1960, 138-48) has found a large rural-urban difference (controlling for such other variables as religion and class) in support for National Socialism.

11. Historians have, of course, often recognized declining trade's sectoral effects on Weimar's final convulsions; the controversial essay of Abraham (1981) is only the best-known example. They may, however, have exaggerated agriculture's woes (see Holt 1936; Rogowski 1982).

12. Sabeian (1969, Chap. 3) and Blicke (1981, 76-78) link the Peasants War convincingly to the density and rapid growth of population in the affected areas, i.e., to an increasing abundance of labor.

13. "The world's trade between 1800 and 1840 had not quite doubled. Between 1850 and 1870 it increased by 260 percent" (Hobsbawm 1979, 33).

14. Finer distinctions would require a more precise definition of factor abundance and scarcity. The one commonly accepted for the case of more than two factors stems from Vanek's extension of the Heckscher-Ohlin Theorem (Leamer 1984, 15); it defines a country as abundant (or scarce) in a factor to the extent that its share of world endowment in that factor exceeds (or falls short of) its share of world consumption of all goods and services. Leamer's (1984, App. D) Factor Abundance Profiles are a tentative effort to apply this definition to present-day economies. To do so with any precision for earlier periods hardly seems possible.

15. Zysman seems to me to have captured the essence of European and Japanese agricultural policy in this period: "The peasantry could be held in place [by subsidies and price supports] even as its economic and social positions were destroyed" (1983, 24).

16. I am grateful to David D'Lugo and Pradeep Chhibber for having raised this issue in seminar discussion.

17. More precisely, they are inconsistent with balanced trade (cf. Leamer 1984, 8-10; Leamer 1987, 14-15).

18. There can be no doubt of Russia's abundance of land: as late as 1960, its population per square kilometer of agricultural land (35.7) was comparable to that of the United States (40.9) or Canada (28.4) and strikingly lower than those of even the more thinly populated nations of western and central Europe (e.g., France, 133; Poland, 146) (World Bank 1983). On the other hand, Myint's (1958, 323-31)

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insightful analysis suggests how even sparsely populated regions can have great reserves of underemployed labor under conditions of primitive markets and social structures; and he takes episodes of extremely rapid economic growth, such as the USSR exhibited in the 1930s, as putative evidence of such "surplus" labor (Myint 1958, 323-24, 327).

19. The chief exception to this rule arises from extensions of trade to wholly new areas with quite different factor endowments. In 1860, for example, Prussia was abundant in land relative to its trading partners; as soon as the North American plains and the Argentine *pampas* were opened, it ceased to be so. I am grateful to my colleague Arthur Stein for having pointed this out.

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PUBLIC OPINION AND THE U.S. SUPREME COURT: FDR'S COURT-PACKING PLAN

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I show the intimate connection between the actions of the justices and support for the Supreme Court during one of the most critical periods of U.S. political history, the four months of 1937 during which Franklin D. Roosevelt sought legislation to "pack" the high bench with friendly personnel. Over the period from 3 February through 10 June 1937, the Gallup Poll queried national samples on 18 separate occasions about FDR's plan. These observations constitute the core of my analyses. I demonstrate the crucial influence of judicial behavior and the mass media in shaping public opinion toward the Supreme Court. This research illuminates the dynamics of public support for the justices, contributes to a clearer understanding of an important historical episode, shows the considerable impact of the mass media on public attitudes toward the Court, and adds more evidence on the role of political events in the making of public opinion.

Political institutions require some minimal level of public support to survive and to thrive. It is, of course, exceedingly difficult to document the consequences of popular support or the lack thereof; but most political scientists seem to agree that it matters. For the Supreme Court, which has no budgetary power or military authority, standing with the public takes on an especially large dimension. Students of the Court and public opinion have emphasized the lack of information, instability of attitudes, and shallowness of support for specific decisions among the U.S. citizenry (Adamany and Grossman 1983; Casey 1974, 1976; Daniels 1973; Dennis 1975; Dolbeare 1967; Dolbeare and Hammond 1968; Kessel 1966; Murphy and Tanenhaus 1968a, 1968b; Tanenhaus and Murphy 1981). That characterization of the mass public—as ill informed about and indifferent toward the Court—undoubtedly captures the essence of opinion

at the level of individuals. But in a recent article, I argued that "the responsiveness of public support for the Court in the aggregate to political events and shifts in the behavior of the justices stands in stark contrast to the conventional image of the American citizenry as out of touch with and unmoved by the Supreme Court" (Caldeira 1986, 1210).

Here I intend to show the intimate connection between the actions of the justices and support for the Supreme Court during one of the critical periods of U.S. political history, the four months of 1937 during which Franklin D. Roosevelt sought legislation to permit him to "pack" the high bench with friendly personnel. The argument is disarmingly simple: the justices themselves helped to shape events and build up institutional support with a series of well-timed decisions. More specifically, I demonstrate that in two crucial actions, the Court let the air out of FDR's sails; the president, despite a

magical touch, had little success in persuading the public of his program's urgency; and the exposure of the proposal in the mass media expanded the "scope of conflict" and helped the justices appreciably. I do not, of course, wish to suggest that the Supreme Court routed President Roosevelt in the battle over constitutional politics in the 1930s. For, in the words of one scholar, the Court won the battle, and FDR won the war (Burns 1956). Nevertheless, the Supreme Court did influence public opinion in a significant fashion in the short run. This research is of consequence because it illustrates the dynamics of public support for the justices, contributes to a clearer understanding of an important historical episode, shows the considerable impact of the mass media on public attitudes toward the Court, and adds more evidence on the role of political events in the making of public opinion (see Dalton and Duval 1986; Kernell 1976, 1986; Lehne and Reynolds 1978; Ostrom and Simon 1985). The results suggest, again, that public support in the aggregate for the Supreme Court responds in a rational and explicable manner to the actions and decisions of political elites (see Caldeira 1986).

Quite unlike most issues, FDR's proposal forced the public to choose between the widely approved policies of an extremely popular president and the institutional integrity of a controversial Supreme Court. Seldom in U.S. constitutional history have the president and the Court gone head-to-head in battle so directly for so long (see Murphy 1962; Nagel 1965; Schmidhauser and Berg 1972; Scigliano 1971). For five months, the mass media, Congress, and the president focused on little else. Indeed the political controversy was sufficiently warm to motivate George Gallup to mount an extended series of polls of the public on the question of the proposed change. Over the period from 3 February through 10 June 1937, the Gallup Poll queried

national samples on 18 separate occasions about "Court packing" and related issues. This set of observations constitutes the core of the statistical analyses in the present paper. These data have with certain exceptions lain fallow for nearly 50 years (cf. Cantwell 1946; Handberg 1984). It is, altogether, an exceedingly rich lode of data on perceptions of the Supreme Court and on the dynamics of public support for political institutions.

The Context of "Court Packing"

Scholars have told the tale, on several occasions and in considerable detail, of the fight in 1937 over President Roosevelt's proposal to pack the Court; here I review the basic chain of events before moving on to the description and analysis of the data (see Adamany 1973; Alsop and Catledge 1938; Baker 1967; Irons 1982; Leuchtenberg 1966, 1969, 1985; Mason 1956).¹ From the outset of the Roosevelt administration, the president and the Court were at cross-purposes. He did not make the decision to attack the Court overnight. The justices, or a majority of them, wanted to constrain national power; Roosevelt, with a "New Deal," set out to expand the horizons of government. After a period of peace between the two branches, the Court in May of 1935 struck down a large segment of the New Deal—the Railway Pension Act, the National Industrial Recovery Act, and the Frazier-Lemke Act—and reversed FDR's dismissal of a member of the Federal Trade Commission. Then, in January 1936, the Court invalidated part of the Agricultural Adjustment Act and in May found the Guffey Coal Act and the Municipal Bankruptcy Act contrary to the Constitution. To add insult to injury, the Court in *Morehead v. ex rel. Tipaldo* struck down the State of New York's attempt to set a minimum wage for women. These and other cases signaled a

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stubborn resistance on the part of the Court to the heart of the New Deal and, for that matter, the emerging modern state. In the view of four, and probably five, of the justices, neither the national nor the state governments had the constitutional capacity to take the actions FDR and others saw as necessary for economic recovery.

In May 1935, Roosevelt criticized the Supreme Court for "relegat[ing] us to the horse-and-buggy definition of inter-state commerce." Still, he took no action against the justices. Others, allies of the New Deal in Congress, introduced a variety of bills to strike at the Court or to amend the Constitution to clarify national legislative powers. Proposed methods of curbing the Supreme Court included the creation of new seats, mandatory retirement at a certain age, abolition of judicial review, the agreement of an extraordinary majority on the Court to invalidate a federal law, and a requirement of advisory opinions on the constitutionality of proposed statutes. During the latter part of 1935 and first half of 1936, the antagonism between the Court and the New Deal was a hot topic in the halls of Congress and in the press. The issue was weighty enough for the Gallup Poll to have asked the public several times in this period about restrictions on the Supreme Court's power to invalidate legislation (see Cantril and Strunk 1951; Murphy 1962, 61). Nevertheless, FDR bided his time, discussing the matter with advisors and maintaining a studied silence on the issue during the presidential campaign of 1936.

Finally, after the election, FDR and Attorney General Homer Cummings fastened upon a proposal from Professor Edward S. Corwin of Princeton; the president would request legislation to permit him to nominate one additional justice for every sitting member who had served 10 or more years and had declined to retire at the age of 70. This proposal, if enacted,

would have given Roosevelt six nominations to the Court. Emerging from a veil of secrecy, the president announced the plan on 5 February to members of the cabinet, leaders of Congress, and a press conference. He added several bonuses to the bill as a smokescreen for the real purpose: additional lower federal judges, reassignment of judges by the Chief Justice to the busiest courts, and a proctor to supervise the flow of litigation. All of this FDR portrayed as a plan to make the justices more efficient and to bring them abreast of their work.

In the wake of the announcement, debate about the proposed restructuring of the Supreme Court poured forth and dominated the public agenda. Story after story—discussions of the plan from every conceivable angle—filled the nation's newspapers. Meanwhile, leaders in Congress, FDR's allies, had little enthusiasm for the program. Over the next few months, as we shall see, a number of political events shaped the ebb and flow of public support for curbing the Court.

Political Events and Public Opinion

Many ingredients influence the outcomes of political campaigns; President Roosevelt's failed attempt to "pack" the Court was the result of a complicated train of events and nonevents. And the first half of 1937 was filled with events of far-reaching historical importance (for a list, see Cantwell 1946, 925). Yet, after a careful review of the period, I have identified four crucial events as particularly instrumental in the battle between the Supreme Court and the president. These events appear over and over again in narratives on the critical junctures in the debate on the Court. Furthermore, of the train of events from February through June, this set of four was the most visible and logically related to shifts in public

opinion. First, on 4 March in a speech at the Democratic Victory Dinner and again in a fireside chat on 8 March, President Roosevelt took the case for Court packing to the people. At the dinner with the faithful, he called for partisan loyalty on the issue. Over the air waves, he called attention to the Supreme Court's alleged usurpation of political power. These speeches and the attendant follow-up by lieutenants, I believe, increased public support for the plan; FDR certainly intended them to do so.

Second, on 29 March, the Court announced the decision in *West Coast Hotel v. Parrish*, upholding the State of Washington's minimum-wage law and overruling the odious precedent of *Adkins v. Children's Hospital*. This decision marked an apparent retreat from the nullification of state legislative power in *Morehead v. Tipaldo*, an opinion delivered less than a year before. The hospitable attitude the Court exhibited in *Parrish* provided a stark contrast to FDR's claims that the justices opposed all forms of social progress. Opponents of the plan, naturally, cited the decision in *Parrish* as proof in part that the nation did not need to restructure the Supreme Court.

Third, on 4 April, the Court validated a major element of the New Deal, the Wagner Act, in four separate cases; *NLRB v. Jones and Laughlin Steel Company* has come down in history as the landmark. Because of the Wagner Act's sweeping nature and the justices' previously demonstrated hostility to broad exertions of national power, the Supreme Court's decision came as somewhat of a surprise. Chief Justice Hughes articulated a vision of national power ample enough to accommodate much of the New Deal. With this display of liberality toward the regulation of interstate commerce, the Supreme Court by deed called the urgency of FDR's scheme into serious question.

Fourth, on 18 May, Justice Van Devan-

ter announced his retirement from the Court. As one of the "Four Horsemen," Van Devanter had contributed a consistent vote in a 5-4 coalition against President Roosevelt's measures and variants of the "little New Deals" from the states. Moreover, although not a productive writer, Van Devanter functioned as the intellectual leader of the Supreme Court's conservatives. With Justice Van Devanter out of the picture and a Rooseveltian in his place, the administration could hope for a 5-4 majority in most of the crucial cases. FDR could not, of course, count on ironclad support from the justices; but the future looked much brighter after 18 May.

In sum, then, I anticipate that President Roosevelt's speeches, the Court's decisions in *West Coast Hotel v. Parrish* and *NLRB v. Jones and Laughlin Steel*, and Justice Van Devanter's departure influenced public support for the proposed restructuring. Later, I shall set out more specific expectations.

Effects of Mass Communications

Even a casual perusal of the literature quickly establishes that scholars have not arrived at a consensus on the impact of mass media in politics. There are, in fact, at least two main questions at issue: Do the media influence what people think *about*? and Do the media persuade the public in one direction or another? Until quite recently, most scholars agreed that "because individuals were firmly anchored in an original set of beliefs and social network of friends, family, and fellow workers, . . . messages from the media worked primarily to reinforce the dispositions first held" (MacKuen 1931, 21; see, for example, Klapper 1960). The press, Cohen said in a conventional assertion, "may not be successful much of the time in telling people what to think, but it is stunningly successful in telling readers what to think *about*. . . . It [puts] a claim

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on their attention, powerfully determining what they will be thinking about, and talking about, until the next wave laps their shore" (Cohen 1963, 13). Lamentably, the earliest researchers targeted the capacity of the mass media to influence *evaluations* of political candidates and issues to the exclusion of the *agenda-setting* function of the various modes of communication.

It is now clear, after a spate of scholarly activity in the 1970s, that the press and broadcasting play large roles in bringing issues and personalities to the forefront of public consciousness and keeping them there; the mass media, in sum, help to set the national agenda (e.g., Funkhouser 1973). At the crudest level, some analysts model public opinion as a simple, "mirror image" of an issue's or personality's salience in the mass media: the greater the coverage, the greater the awareness among the citizenry. Others have taken a more sophisticated approach. Thus, for example, MacKuen (1981) has demonstrated the significant impact of media attention on the concerns of the public, but he also finds events and objective conditions to be crucial elements of the equation (see also Erbring, Goldenberg, and Miller 1980; Iyengar, Peters, and Kinder 1982).

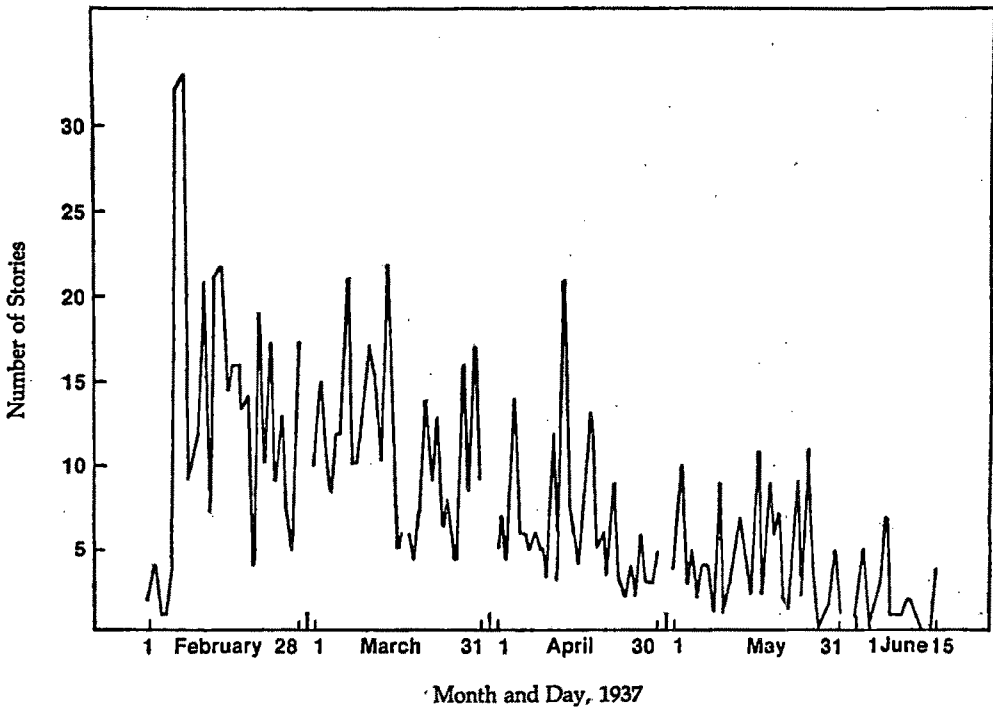
Quite apart from agenda setting, in the last few years, several students of persuasion have mounted a concerted challenge to the conventional wisdom that the mass media exercise "minimal effects" on political decisions and outcomes. In a study of newspapers during the congressional election of 1974, Miller and his associates discovered that readers of highly critical papers displayed more distrust in government than did others: "media style in reporting . . . events . . . had an effect on the degree of popular disaffection found in America" (Miller, Goldberg, and Erbring 1979, 80). Similarly, Coombs (1981) reports that in 1972 and 1974, newspaper endorsements had a

measurable impact on a candidate's chances of victory at the polls.

Here I anticipate a significant relationship between the media's coverage of the Supreme Court and support for Court packing (on the Court and the press, see Grey 1972; Newland 1964). There is, unfortunately, no readily available, comprehensive indicator of the extent of the mass media's attentiveness to the Court and President Roosevelt's plan. Of the major newspapers, only the *New York Times* has an index that reaches back into the 1930s. So far as I can tell, the radio networks have not published or archived indexes of their broadcasts (but see Murrow 1937). For my indicator of the media's coverage, I have adopted the number of stories in the *New York Times*. That measure, taken weekly in the first six months of 1937, correlates strongly with the number of stories on the Court collected in *The Reader's Guide to Periodical Literature*. To be sure, the number of stories in the *New York Times* at best taps indirectly the amount and kind of information the public possesses on a particular subject; after all, its readers constitute a highly educated, elite stratum of U.S. society. Nevertheless, as the correlation between coverage in the *New York Times* and *The Reader's Guide* attests, the coverage in this elite newspaper tracks closely with other sources of information. Furthermore, reports in the *New York Times* undoubtedly diffuse throughout the nation in a "two-step flow" of information. (For a defense of a similar measure, see MacKuen 1981, 63-64.)

Figure 1 displays the number of stories on the Supreme Court in the *New York Times* on a daily basis from 1 February through 15 June 1937. Coverage of the Supreme Court varied a good deal in intensity, from a high in early February to a low in May, and declined across the period. Discussion of the Court did occur daily until late in May, so the public was subjected to a continuous barrage of

Figure 1. Coverage of Supreme Court, 1-15 February



information. The peaks in coverage, naturally, correspond to key events in the campaign over Court packing. Thus, for example, the announcement of the decision in *NLRB v. Jones and Laughlin Steel* set off a raft of stories on it and related matters. What is most remarkable here is the length and intensity of attention to the Supreme Court. Surely the Court has not since then surfaced so long and so prominently on the public agenda, even during the salad days of the Warren Court.

Specifically, I expect a negative relationship between the intensity of coverage and support for Court packing. Public attention to the Court operated, I believe, in classic fashion; Schattschneider wrote of the "socialization of conflict" (1960; see also Truman 1951). That is, as the mass media focused on the issue, more and

more groups, institutions, and individuals entered into the fray. After a reading of the news for that period, one gathers the cumulative impression, especially in the latter half, of a cacaphony of voices and actors for and against the plan. Enemies of FDR and of Court packing did all they could to mobilize potential opponents. And, of course, as more interest attended the issue, more questions inevitably arose. In his study of presidential popularity, Mueller (1973) has characterized a similar phenomenon over the course of a chief executive's term as a "coalition of minorities"; the more exposure a president receives, the more critics he acquires. In any event, the number of stories in the *New York Times* provides a neat index of the expansion of conflict. Extended focus on a subject should, in addition, foster

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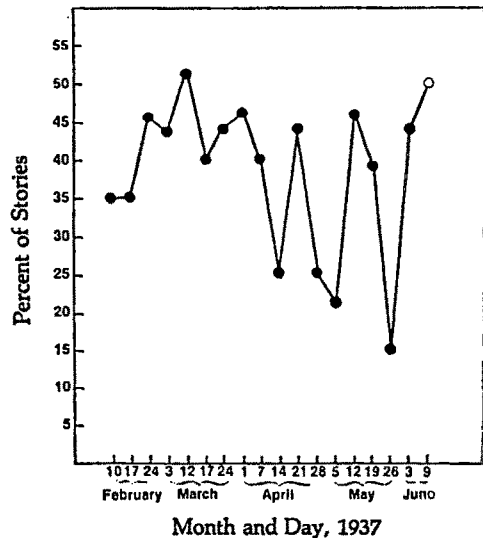
more coherence among the citizenry. Accordingly, media coverage did indeed help to crystallize public opinion toward the president's plan; as the number of stories increased, the percentage of the public with no opinion decreased ($b = -0.06$; $r^2 = .15$).

Figure 2 portrays the directional balance of stories on the Supreme Court in the *New York Times* from February through mid-June 1937. To create this variable, I have divided stories into categories of *positive*, *negative*, and *neutral* toward the Supreme Court. I then excluded the neutral reports. The percentages in Figure 2 represent the portion of the remaining stories I could classify as supportive of Court packing.² Throughout the period under consideration, the coverage of the issue ran against President Roosevelt. In only two weeks did favorable views of Roosevelt's position on the Court hit 50% or higher—immediately after his fireside chat and in the final part of the series as interest in the issue faded. Actually, these last two observations are probably somewhat misleading because the number of stories by that time had decreased to a handful. On average, about 39% of the categorized stories placed FDR's proposal in a favorable light. If these data are at all representative of the mass media's treatment of Court packing—and I believe they are—then FDR clearly received little aid and comfort from the press.³ Naturally, I expect that as the content of coverage became more positive toward the proposal, the segment in the nation in favor of Court packing grew apace. There is, incidentally, no relationship between the level of media coverage and the direction of the stories.

Methods, Models, and Data

To test for the effects of the four events, I have chosen a multiple interrupted time-series design (see McDowall et al. 1980).

Figure 2. Direction of Stories on Court Packing



In its most elementary form, interrupted time-series analysis "attempts to answer a simple question: Does the occurrence of a particular event change a variable's behavior over time?" (Lewis-Beck 1986, 209). Since I specify other independent variables, the model differs from the typical one. Under the best circumstances, I would include for each event a term to pick up a shift in the slope and one to detect a change in the intercept.⁴ Two considerations militate against that strategy here. First of all, the number of observations involved ($N = 18$) is small, and the period of time short (18 weeks). The inclusion of nine parameters for the four events (two for each plus one for the trend), as normal practice indicates, brings on unacceptable levels of multicollinearity and exhausts degrees of freedom. Signs of coefficients go astray. Second, even if full specification were a technical option, I would probably argue for a more limited and parsimonious approach. Because of the brevity of the series in question, a shift in the slope for

an event seems implausible. Instead, we need to monitor sudden, step-level changes in public opinion as a result of the four political events—in other words, to watch for shifts in the intercept.

These ruminations imply the following equation to model the hypothesized consequences of political occurrences:

$$S_t = b_0 + b_1 FDR_{1t} - b_2 PAR_{2t} \\ - b_3 NLRB_{3t} - b_4 VDV_{4t} \\ - b_5 TREND_{5t} + e_t$$

where S_t = 18 time-series observations on the dependent variable, percentage in the sample in favor of Court packing; FDR_{1t} = a dichotomous dummy variable scored 0 for observations before 10 February and 1 after the fireside chat; PAR_{2t} = a dichotomous variable scored 0 before 1 April (the Court announced *West Coast Hotel v. Parrish* on 29 March) and 1 thereafter; $NLRB_{3t}$ = a dummy variable denominated 0 before 12 April and 1 afterward (*NLRB v. Jones and Laughlin Steel* came down on 12 April); VDV_{4t} = a dichotomous variable scored 0 before Justice Van Devanter's resignation on 18 May and 1 thenceforth; and $TREND_{5t}$ = a dummy variable counter for time from 1 to 18. In this model, b_1 , b_2 , b_3 , and b_4 estimate any post intervention changes in the intercept. So, for instance, b_1 represents the immediate impact of President Roosevelt's speech. Based on the considerations outlined in the Political Events and Public Opinion section, I anticipate $b_1 > 0$ and b_2 , b_3 , and $b_4 < 0$.

In the previous section, I discussed the sources and properties of the two indicators of the mass media's treatment of the Supreme Court. For the direction of stories in the *New York Times*, as in Figure 2, I rely on a simple percentage. The indicator of media attention poses one problem. With such a profusion of stories at the outset of the period (see Figure 1), the series contains a formidable trend downward ($NYST_t = 83.9 - 4.24$

$TREND_t$; $r^2 = .85$). The series is, in econometric parlance, "nonstationary." To make the series stationary, I have differenced the number of stories—change in attention is a function of this week's coverage minus last week's coverage. The final model, then, is

$$S_t = b_0 + b_1 FDR_{1t} - b_2 PAR_{2t} \\ - b_3 NLRB_{3t} - b_4 VDV_{4t} \\ - b_5 TREND_{5t} + b_6 NYBAL_{6t} \\ - b_7 CNYST_{7t} + e_t$$

where the notation through $TREND_{5t}$ remains as before; $NYBAL_{6t}$ = the balance of stories in the *New York Times* in favor of Court packing and FDR's approach to the Court, counted weekly, in percentages; $CNYST_{7t}$ = the change in the number of stories from one period to the next; and e_t is a term for errors. On the basis of the discussion in the Effects of Mass Communications section, I expect $b_6 > 0$ and $b_7 < 0$. In the initial runs, the coefficient for $TREND_{5t}$ failed to reach statistical significance; therefore, in the interest of conserving degrees of freedom, I dropped it in subsequent analyses. To estimate the coefficients, I have used a method equivalent to generalized least squares.⁵

Now is the time to cast an eye on and describe the dependent variable, support for Court packing. Figure 3 records support for, opposition to, and lack of, opinion on FDR's plan among 18 national samples.⁶ Support for FDR's proposal varied from a high of 46% immediately after his fireside chat to a low of 31% in the wake of Justice Van Devanter's resignation. Over the entire period, support averaged about 39%. Opposition to Court packing ranged from a low of 41% on 24 March to a high of 49% on 3 March. On average, about 46% of each sample indicated opposition to President Roosevelt's proposed legislation. And it is clear that, after a surge from an early push by FDR, the public support for re-

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structuring the Court rapidly melted. Without extensive analysis of the original surveys, one cannot know with certainty, but I surmise from the patterns that most of the ups and downs in Roosevelt's support probably resulted from movements to and from the realm of no opinion. Opposition remained relatively steady. The proportion with no opinion bobbed around a good deal, from a low of 9% on 3 March to a high of 25% on 19 May.

The Results

Table 1 presents the products of a multiple regression of support for Court packing on the independent variables, corrected for autocorrelation. In general, the results are crisp: five of the six coefficients reach statistical significance, the direction of signs makes sense, and the fit is tight ($R^2 = .88$). Figure 4, a plot of the actual observations of support for Court packing on the predicted values, illustrates the high quality of the statistical explanation; only a few substantial residuals materialize.

The coefficients in Table 1 dramatically demonstrate the considerable latitude the Supreme Court has in shaping the contours of public opinion. President Roosevelt's efforts to persuade the Democratic party and the public of the plan's urgency, on 4 March at the victory dinner and over the air on 8 March, did make a statistically significant—if substantively marginal—impact on support for his bill. In the short run, at least, FDR's speeches boosted support by nearly 3% ($b_1 = 2.7$). Nevertheless, in comparison to the Supreme Court's actions, the president's campaign made only a modest dent in public opinion.

Somewhat surprisingly, the overruling of *Adkins v. Children's Hospital* in *West Coast Hotel v. Parrish* on 29 March made little or no difference in the pattern of public support ($b_2 = -1.2$; $t = -.6$). In the scholarly literature and the textbooks,

Figure 3. Support for Court Packing

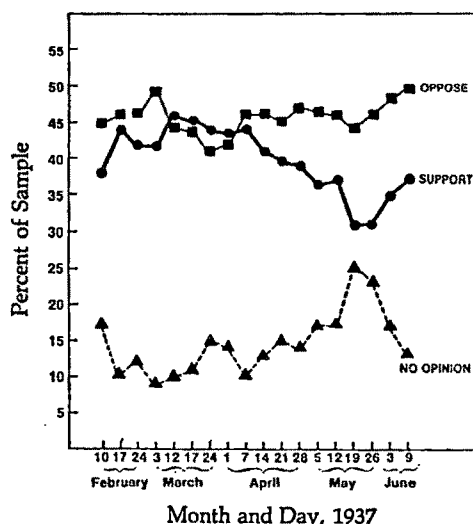


Figure 4. Support for Court Packing: Actual vs. Predicted

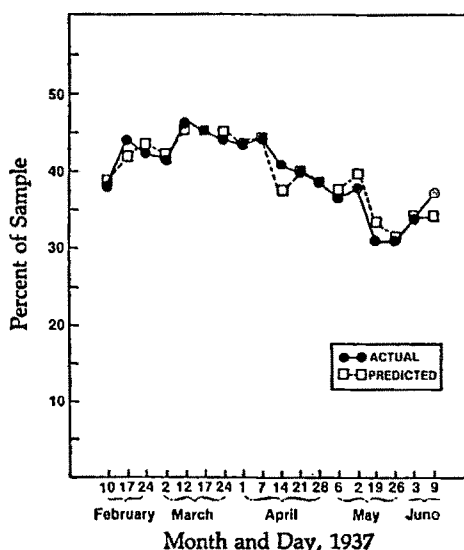


Table 1. Support for Court Packing

Independent Variable	<i>b</i>	<i>t</i> -Ratio	Level of Significance ^a
Intercept	38.97	16.35	.0001
Political events			
FDR's speeches	2.67	1.73	.0500
Overruling of <i>Adkins</i>	-1.16	-.64	n.s.*
Upholding of Wagner Act	-4.31	-2.48	.0150
Van Devanter's resignation	-5.63	-4.18	.0010
Communications			
Change in media coverage	-.04	-1.68	.0500
Direction of media coverage	.08	1.43	.0750

Note: $R^2 = .88$; $df = 11$; $N = 18$.

^aTwo-tailed test of significance.

*Statistically significant at .10 or better.

of course, *Parrish* marks a watershed in constitutional development; but after fits and starts in the Court's hospitality toward state regulation of business, it is doubtful whether many observers saw the decision that way. Moreover, because the Supreme Court in *Parrish* dealt with a state law, the connection between the decision and the New Deal was only indirect. This is so even though the overruling of *Adkins* signaled a retreat and commentators rank it as important as *NLRB v. Jones and Laughlin Steel*. It expressed a change in attitude, and that was apparently too subtle for the public to detect; for we know that few members of the relevant political elites actually read the Court's opinions or make themselves familiar with their contents. And, clearly, even a smaller segment of the mass public monitors as much as the general direction of the Court's jurisprudence, let alone specific decisions. It is probably too much to expect the public to react sharply to subtle shifts in public policy.

Upholding the Wagner Act, a contemporary linchpin of the New Deal and salient because of the recent sit-down

strikes, helped enormously in building opposition to the president's measure. As a result of *NLRB v. Jones and Laughlin Steel*, FDR and his allies lost more than 4% of the public ($b_3 = -4.3$; $t = -2.3$). In the short run, the president's loyalists tried to shore up support and portrayed the decision on the Wagner Act as a temporary, strategic retreat on the part of the justices. That approach obviously did not work. Key political actors, inside and outside the halls of Congress, who understood the decision could readily see the radical departure from the narrow conception of interstate commerce dominant on the Court during the 1930s that Chief Justice Hughes's opinion implied. These results suggest that elite views diffused among the general public in the well-known two-step flow of information and opinions.

Justice Van Devanter's resignation—expected all along—sent the enemies of the Supreme Court into a tailspin. The announcement of Van Devanter's exit alone subtracted more than 5% ($b_4 = -5.6$) from the segment supportive of restructuring the Court. With that resignation, the prospect of a new justice, and

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the streak of 5-4 decisions, it must have seemed clear to many that FDR had a chance to have a sympathetic if small coalition on the Court. Van Devanter's departure, on the heels of *NLRB v. Jones and Laughlin Steel*, made it difficult for even the most loyal New Dealers to demonstrate the need for radical judicial reform.

Two crucial events—*Jones and Laughlin Steel* and Justice Van Devanter's resignation—spelled doom for FDR's bill to enlarge the Court and pack it with justices favorable to the New Deal. Between the two decisions, the Supreme Court decreased support for President Roosevelt's proposal by nearly 10%. Commentators and the justices themselves speak of the Supreme Court as relatively helpless in the arena of public opinion; but these results suggest the contrary. Planned or not, the Court's actions played a dramatic role in defeating the president. Chief Justice Hughes's active participation and superb timing in supplying information and surfacing in the news on occasion during the crisis indicate a great deal of political savvy (Mason 1956). Hughes, of course, had served in many political roles, and he behaved in a manner we might expect from an experienced politician.

The mass media's treatment of the Supreme Court made an appreciable impact on the shape of public sentiment for restructuring. For each 12% increase in favorable coverage of the plan in the *New York Times*, support for Court packing increased 1% ($b_6 = .08$). Since on average 40% of the stories put FDR's plan in a positive light, the *New York Times'* treatment contributed about 3.2% to the coalition in support of Court packing. Change in media coverage, on the other hand, detracted from the plan's support: for each increase of 25 stories, the segment in favor of the proposal declined by about 1% ($b_7 = -.4$). Thus, as I suggested, the bright light of increased scrutiny actually hurt its chances. This finding

dovetails nicely with results I have reported elsewhere: in a study of public support for the Court in 1966-84, increases in media coverage of the Court increased support for the justices. The opponents of FDR managed to expand the scope of conflict to include all manner of people and groups heretofore unorganized. Extended media attention to the issue aided and abetted that mobilization. The intensity and direction of media coverage apparently exercised an equal influence on support for Court packing—after standardization, for media attention, $B_6 = -.20$; for the balance of coverage, $B_7 = .19$. In sum, then, media coverage made a difference in the battle over public opinion.

Conclusions

Public support for Court packing varied considerably over the space of 18 weeks and defies a simple account; but I have tried to trace meaningful shifts and to frame and evaluate explanations of the dynamics of public opinion on this issue. How can we explain the ebb and flow of sentiment for the president's plan (and presumably against the Supreme Court)? The rival hypotheses included political events—FDR's speeches, the Court's decisions in *Parrish* and *Jones and Laughlin Steel*, and Van Devanter's resignation—and the salience and direction of coverage in the mass media. Political events and mass communication both shaped public attitudes on the issue in the aggregate. Even more important, for present purposes, the actions taken by the Court and the justices played a crucial part. Historians will no doubt continue to argue about the political motivations behind and the timing of the Supreme Court's decisions; but regardless of the intentions of the participants, the Supreme Court's behavior made a difference. And to the outside observer, the Supreme Court did

not seem to be a passive party, sitting idly by on the sidelines.

Scholarship on the origins and defeat of FDR's plan to restructure the federal judiciary, like so much else written on the Supreme Court, often subscribes to a mythical view of public attitudes toward the high bench (for an old but classic example, see Lerner 1937). Thus, for example, a recent student of the skirmish states that "for various reasons, Americans generally have regarded the judicial process as somehow outside or above politics" (Nelson 1986, 14). Trying to fathom FDR's failure in the arena of public opinion, a distinguished historian of the period has concluded that the "greater the insecurity of the times, the more people cling to the few institutions that seem unchangeable" (Leuchtenberg 1963, 235-36). Perhaps members of the public hold the Supreme Court in reverence and that is why FDR's proposal went up in flames. Yet the evidence accumulated over the years goes against that notion of the relationship between the public and the Court. I prefer, instead, a much more straightforward account: the Supreme Court outmaneuvered the president. Through a series of shrewd moves, the Court put President Roosevelt in the position of arguing for a radical reform on the slimmest of justifications.

Those moves, of course, spelled an important jurisprudential retreat on the part of the Court. President Roosevelt, in essence offered the Supreme Court a choice between substantive policy and structural integrity. The Court wisely chose to give up on the substantive issues and preserve its structural integrity. And public opinion, as we have seen, supported the Court in this choice. Perhaps one lesson we can extract from these results is that if the justices wish to gain public support in battles with the popularly elected branches, they must first

assess whether the public stands with the president and Congress on the substantive issue; and, if so, the Supreme Court can preserve its institutional integrity by retreating on the issue. The results I have reported here suggest that the public will support existing constitutional arrangements or, at a minimum, existing arrangements as they pertain to the judiciary, so long as these arrangements are the sole issue. It is not at all clear what the public would have done if the Court had insisted on continuing its opposition to the New Deal. Congress and the president cannot successfully diminish the Supreme Court's structural integrity unless they have a major substantive dispute with the Court on issues to which the public has a strong commitment. Even then, we do not know whether the public would choose structural integrity or substantive policy. We do know that even an overwhelmingly popular public official cannot attack the structural integrity of the Court without the benefit of a very important policy issue for which he or she has overwhelming public support.

The research reported here raises many related questions. Did the political events in the spring of 1937 exert a differential impact across strata in the population? It is quite plausible, for example, that Democrats and union members responded differently to the issue than did Republicans and white-collar workers. Did FDR's fireside chat make a bigger mark among his loyalists and in parts of the nation more sympathetic to the New Deal than among others? Observations over time can help us to come to grips with that sort of question. In the cross-sectional case, can we identify the social, political, and economic bases of support for and opposition to the Supreme Court? The answers to these and other questions have important implications for the study of public opinion and the Supreme Court.

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Notes

For some of the data presented here, I appreciate the assistance of the staff of the Roper Opinion Research Center. Michael S. Lewis-Beck advised on some statistical issues, and Peverill Squire made thoughtful comments on an earlier draft.

1. In this section and the next, I have relied heavily on the historical accounts cited here. To smooth the narration, I have refrained from making references to particular books or articles along the way. In his three articles, Leuchtenberg (1966, 1969, 1985) provides an especially good collection of sources on Court packing.

2. If a story, letter to the editor, or editorial praised, endorsed, or sympathized with President Roosevelt's approach to the Court, I coded it as *anti-Supreme Court*. If, on the other hand, coverage placed the Court in a positive light or reported criticism of FDR's proposal, I designated it as *pro-Supreme Court*. Letters to the editor and editorials created few problems. Straight reports proved more difficult. If a story reported on someone who criticized the proposed legislation, I classified it *pro-Court*. Since few people or institutions adopted a neutral stance, I had relatively little trouble in making judgments.

3. Naturally I would like to have data on the balance of coverage from a wide variety of newspapers. That would have entailed far more greater resources than I had at my disposal. Since the data from the *New York Times* mask the variations among newspapers across the nation and attenuate the relationship between the direction of coverage and changes in public opinion, I have undoubtedly underestimated the impact of the mass media on support for Court packing.

4. For simple interrupted time-series analysis, the multiple regression equation takes on the form

$$Y_t = b_0 + b_1 X_{1t} + b_2 X_{2t} + b_3 X_{3t} + e_t$$

where Y_t = N time-series observations on the dependent variable; X_{1t} = a dummy variable counter for time from 1 to N ; X_{2t} = a dichotomous dummy variable scored 0 for observations before the event and 1 for observations after; X_{3t} = a dummy variable counter of time scored 0 for observations before the event and 1, 2, 3, . . . for observations after the event (see Lewis-Beck 1986, 215).

5. More specifically, I used the AUTOREG procedure in SAS. It first estimates the model with ordinary least squares and then obtains estimates of the autoregressive parameters. Accordingly, the program transforms the original data by the appropriate autoregressive model. Finally, after making adjustments, SAS reestimates the ordinary least-squares regression. If all goes well, the procedure should eliminate the problems caused by autocorrelation. (See SAS Institute [1982, 187–202].) For

this equation, I encountered no significant autoregressive parameters [$AR(1) = .17$; $AR(2) = .00$]. Thus, the results reported in Table 1 are the product of ordinary least squares.

6. In the first half of the series, the Gallup Poll asked, "Are you in favor of President Roosevelt's proposal regarding the Supreme Court?" Respondents could answer *yes*, *no*, or *no opinion*. Later, for inexplicable reasons, Gallup shifted the wording slightly to "Should Congress pass the president's Supreme Court plan?" Changes in wording are always cause for worry, but my analysis indicates that this particular alteration made little difference. First of all, relationships between support for Court packing and various independent variables such as partisanship, attitude toward FDR, and demographics did not change. Second, in the context of the fully specified model, a dummy variable for question wording does not reach statistical significance. Third, the substance of the question did not change, even though the wording did. Before and after the change, respondents had to make a decision on FDR's proposed law; Court packing constituted the stimulus in both versions.

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THE CORE OF THE CONSTITUTION

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It is often argued that the United States Constitution was designed so as to create a stable political order. Yet in the literature on the formal theory of democracy, there has been very little examination of constitutional provisions for their stability-inducing properties. In this paper we demonstrate that bicameralism and the executive veto tend to create stability, that the legislative override of the executive veto tends to undermine this stability, and that the interaction of bicameralism and the executive veto is likely to produce stable outcomes despite the destabilizing impact of the veto override.

Designing a constitution that would ensure stability in the laws was a major concern of some of the central participants in the Constitutional Convention. In his defense of the resulting document in *The Federalist Papers*, for example, James Madison argued that the proposed Senate, with its slow turnover and insulation from the public, was needed to help avoid instability: "The mutability in the public councils arising from a rapid succession of new members, however qualified they may be, points out, in the strongest manner, the necessity of some stable institution in the government" (*Federalist* no. 62 [p. 380]). In Madison's view this "mutability in the public councils" cost greatly: "To trace the mischievous effects of a mutable government would fill a volume" (p. 380).

The costs of instability were both external and internal in nature. Externally, instability causes the nation to forfeit "the respect and confidence of other nations" (p. 380): "Every nation . . . whose affairs betray a want of wisdom and stability, may calculate on every loss which can be

sustained from the more systematic policy of its wiser neighbors" (p. 381). Internally, the consequences of instability were even worse: "The internal effects of a mutable policy are still more calamitous. It poisons the blessings of liberty itself. It will be of little avail to the people that the laws are made by men of their own choice if the laws be so voluminous that they cannot be read, or so incoherent that they cannot be understood; if they be repealed or revised before they are promulgated, or undergo such incessant changes that no man, who knows what the law is today, can guess what it will be tomorrow" (p. 381). This instability had distributional consequences in the society: "Another effect of public instability is the unreasonable advantage it gives to the sagacious, the enterprising, and the moneyed few over the industrious and uninformed mass of the people. Every new regulation concerning commerce or revenue, or in any manner affecting the value of the different species of property, presents a new harvest to those who watch the change, and can trace its consequences" (p. 381). Com-

merce could also expect to suffer from unstable government: "The want of confidence in the public councils damps every useful undertaking, the success and profit of which may depend on a continuance of existing arrangements. What prudent merchant will hazard his fortunes in any new branch of commerce when he knows not but that his plans may be rendered unlawful before they can be executed?" (p. 381).

The Senate, with its members' six-year terms, their election by state legislatures, and the chamber's slow rate of turnover, could be expected to help avoid this instability. The independently elected president with his veto power was also expected to help. Hamilton argued in *Federalist* number 73 about the consequences of the veto: "It may perhaps be said that the power of preventing bad laws includes that of preventing good ones; and may be used to the one purpose as well as to the other. But this objection will have little weight with those who can properly estimate the mischiefs of that inconstancy and mutability in the laws, which form the greatest blemish in the character of genius of our governments. They will consider every institution calculated to restrain the excess of law-making, and to keep things in the same state in which they happen to be at any given period as much more likely to do good than harm; because it is favorable to greater stability in the system of legislation" (pp. 443-44).

Rapid turnover of legislators was one cause of instability in the laws, and Madison was also concerned about "the propensity of all single and numerous assemblies to yield to the impulse of sudden and violent passions, and to be seduced by factious leaders into intemperate and pernicious resolutions" (*Federalist* no. 62 [p. 379]). But there is reason to think that Madison's views on the instability of republics were also buttressed by a deeper theoretical under-

standing of the instability of majority rule. Theorists of democracy have long been troubled by the fact that a key element of most democratic systems—majority rule—has some undesirable properties. Among these is the fact that for any option that might be chosen by a majority of voters there is usually some other option, preferred by a different majority, which can upset it. Recognition of this problem can be traced back at least two hundred years, to Condorcet's seminal *Essai* (1785) and McGrath (1983, chap. 3) points out that Madison was well acquainted with Condorcet's work; Madison even wrote a review (now unfortunately lost) of the *Essai* and its arguments.

For Madison, then, there were theoretical as well as empirical reasons for designing a government that did not depend on simple majority rule. The Constitutional Convention's chosen design involved, in part, the separation of powers. That the separation of powers has indeed played a major role in creating a stable republic has been argued by many generations of historians and political scientists.

Yet while the formal literature on the theory of democracy has explored the nature of majority-rule instability in great detail—see Cohen 1979; McKelvey 1976, 1979; Miller 1980; Schofield 1978; and Shepsle and Weingast 1984, for example—there has been almost no study of whether a separation-of-powers system has the stability-inducing properties that Madison and Hamilton desired. As far as we can find, only Cox and McKelvey 1984 and Cox 1980 even address the relationship between bicameralism and stability. The former informally describes general conditions for stability in multicameral systems (p. 77). The latter argues that dividing a legislature into two chambers whose members have completely separate Pareto sets will ensure stability in two dimensions (see p. 10). Cox further

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conjectures that adding more policy dimensions can destabilize the situation: it appears that as many veto groups (e.g., chambers or committees) as dimensions are needed to guarantee stability.

Our work is in some ways more developed and in other ways more limited than that of Cox and McKelvey. For the set of constitutional provisions governing the legislative process—bicameralism, the executive veto, and the legislative veto override—we provide tools and formal results for characterizing the existence, shape, and location of any set of stable points. However, we assume that each legislator and the executive has “Euclidian preferences,” that is, a most-preferred point and circular indifference curves in the issue space. We also restrict our analysis to the case of two dimensions. If legislators and the executive are considering an issue involving no more than two dimensions, the constitutional rules we are considering may well create stability. For more complex issues, the constitutional structure may be an insufficient guarantor of stability, and additional institutional features may be required.

Pure Majority Rule and the Bicameral Game

In a social institution, one alternative *dominates* another alternative when there is a set of individuals whose members all prefer the first alternative to the second and who can, given the rules of the game, enforce the first alternative over the second. The *core* of this institution is the set of undominated alternatives. In a pure majority-rule legislature, the core is the alternative that can gain a majority of votes against any other in a two-way vote. When there are three or more legislators and two or more dimensions by which options can be evaluated, a simple majority-rule legislature—playing what we will call a “unicameral game”—gener-

ally lacks a core. That is, for each possible legislative choice there almost always exist other options that are majority preferred (McKelvey 1976, 1979).

For example, consider the illustration in Figure 1. If the six legislators vote by simple majority rule, legislators H_1 , H_2 , H_3 , and S_3 all prefer alternative p to c : p lies inside the (circular) indifference curves through c of legislators H_1 , H_2 , H_3 , and S_3 . A different majority coalition prefers e to p (indifference curves—not drawn—through p of S_1 , S_2 , S_3 , and H_3 all contain e). Yet another coalition (H_1 , H_2 , S_1 , S_2) prefers c to e . No alternative can gain the support of a four-member majority coalition against all other alternatives.

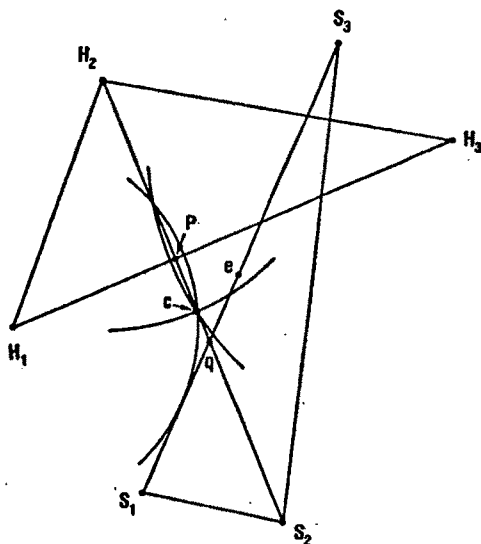
Various constitutional provisions can help overcome this generic instability of simple majority rule. Suppose H_1 , H_2 , and H_3 are all assigned to one chamber (the House), while S_1 , S_2 , and S_3 are all assigned to a different chamber (the Senate). For a bill to pass, it must gain the support of a *particular* four-person coalition—a *joint majority*—consisting of a majority of *each* chamber. These same six legislators no longer generate a majority-rule cycle. There is now a core that includes the point c : no joint majority will upset c in favor of some other point. The institution of bicameralism thus induces stability, given these six legislators' preferences.

The following section introduces theorems about when bicameralism has this stability-inducing effect. We also show how to find the shape of the bicameral core when it exists.

The Bicameral Game

Madison's analysis of bicameralism in *Federalist* numbers 62 and 63 lays out, in surprisingly accurate terms, the conditions under which bicameralism can avoid instability. The basic logic, he suggested in discussing the likelihood that the House

Figure 1. A Six-Person Legislature with a Bicameral Core but No Unicameral Core



and Senate would agree on undesirable policies, was that "the improbability of sinister combinations will be in proportion to the dissimilarity in the genius of the two bodies" (no. 62 [p. 379]). In our terms, when the ideal points of the members of the two chambers are sufficiently distant from each other, legislative outcomes will be stable: the bicameral game will have a core. Moreover, the outcomes will always lie within the legislative Pareto set: clearly undesirable policies are thereby avoided, as Madison desired.

The required separation between chambers was to be achieved by insulating the Senate from the popular passions to which the House would be exposed. If the Constitution was designed so as to induce in senators preferences substantially different from those induced in representatives, how great must these interchamber differences be for bicameralism to produce a core? To answer this question it is first useful to introduce several definitions and lemmas.

Lemmas about Bisectors

For the members of one chamber, draw the contract curve between the ideal points of each pair of members and extend each contract curve indefinitely in each direction. Some of these contract curves as extended (henceforth we refer to them simply as *contract curves*) will meet both of the following conditions:

1. The number of ideal points lying on the contract curve plus the number of ideal points to one side constitute a majority of the chamber's members
2. The number of ideal points lying on the contract curve plus the number of ideal points to the other side constitute a majority of the chamber's members

A chamber-member/chamber-member contract curve meeting both conditions is a *chamber bisector*. It is easily proved (Hammond and Miller 1987, 5, Lemma 1) that every chamber will have at least one chamber bisector.

If a contract curve is a chamber bisector, it is *attractive both ways*. That is, for any point not on the chamber bisector, there is at least one point on the chamber bisector that is majority preferred; see Davis, DeGroot, and Hinich 1972, where chamber bisectors are known as *medians*, or, in n -dimensional space, *median hyperplanes*. A simple geometric version of this assertion will help intuitive understanding:

LEMMA 1. *A chamber bisector is attractive both ways* (Davis, DeGroot, and Hinich 1972, Theorem 2).

Proof. The definition of *chamber bisector* implies that there will always be a majority of members on or to one side of the chamber bisector. It is therefore only necessary to prove that any point w on the *other* side of the chamber bisector from this majority can be beaten by some point on the bisector supported by this

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majority. In particular, consider the point x on the chamber bisector that is closest to w . (It will be the base of a perpendicular dropped to the bisector through w .) The point x will be closer to the ideal points of every legislator on the chamber bisector. It will also be closer to the ideal points of every legislator on the other side of the chamber bisector from w . Thus, the proposed majority will be willing to upset any point on the opposite side of the chamber bisector. The same argument applies to the analogous majority on or to the other side of the chamber bisector. Hence every point off the chamber bisector can be upset by some point on the chamber bisector. QED

There is nothing about a chamber bisector's properties that tells us what is in the core of the bicameral game. But Lemma 1 will be useful in telling us what points are *not* in the core.

Next, draw each House-member/Senate-member contract curve and extend the curve indefinitely. A joint majority consists of a majority of the House plus a majority of the Senate. When there is an odd number of members in each chamber, one or more of these cross-chamber contract curves will meet both of the following conditions:

1. The number of ideal points lying on the contract curve plus the number of ideal points to one side constitute a joint majority
2. The number of ideal points lying on the contract curve plus the number of ideal points to the other side constitute a joint majority

When there is an even number of members in one or both chambers, one or more of these cross-chamber contract curves will meet both of the following conditions:

3. The number of ideal points lying on the contract curve plus the number of ideal points to one side constitute a joint

majority

4. The number of ideal points lying on the contract curve plus the number of ideal points to the other side constitute no fewer than one less than a majority in each chamber

A House-member/Senate-member contract curve meeting one of these pairs of conditions is a *bicameral bisector*. In Figure 1, the H_2S_2 contract curve is a bicameral bisector. As with a chamber bisector, it is easily proved that a bicameral bisector will always exist in a bicameral legislature.

If a House-member/Senate-Member contract curve is a bicameral bisector, it is *attractive one way* and may be, but is not necessarily, *attractive both ways*. If it is attractive both ways, for any point not on the bicameral bisector, there is a point on the bisector preferred by a joint majority. For an odd number of members in each chamber the following holds:

LEMMA 2. *If each chamber has an odd number of members, each bicameral bisector has a joint majority on or to each side and is thus attractive both ways.*

Proof of this lemma is almost identical to that of Lemma 1.

If one or both chambers has an even number of members, the bicameral bisectors will commonly have a joint majority only on or to one side—that is, Conditions (3) and (4) are usually met—and each bisector will be attractive only one way.

We can now state the most general conditions for a bicameral game to have a core:

THEOREM 1. *A point x is in the core if and only if no straight line through x leaves a majority of both chambers to one and the same side of that line.*

Proof consists of showing that a core exists when, for each majority in one

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chamber that wants to move in one direction from x , at least half of the other chamber will want to move in the opposite direction from x (see Appendix).

Theorem 1 defines the most general conditions under which a bicameral core will exist. But the theorem is not very useful for determining if a core exists with any particular preference profile: we would have to examine every possible x in the policy space to see if it meets both conditions. And finding one such point tells us little about the existence or location of any other such point.

However, identification of the chamber and bicameral bisectors leads directly to techniques for deducing when a core will exist and establishing what it looks like. Let us first look at the example in Figure 1. The House-Senate bicameral game has a core, which is that portion of the H_2S_2 bicameral bisector lying between House chamber bisector H_1H_3 and Senate chamber bisector S_1S_3 ; that is, the core is line segment pq . The reason? By Lemma 2, H_2S_2 is attractive both ways, so points not on H_2S_2 are not in any core. Furthermore, while a chamber bisector is attractive both ways when there is just a single chamber (Lemma 1), in a bicameral legislature it is attractive in a given direction only if a majority of the other chamber concurs. For example, points above H_1H_3 are attracted to H_1H_3 (from H_2 's general direction) because all three members of the Senate would agree with H_1 and H_3 to move to H_1H_3 . However, points below H_1H_3 are not necessarily attracted to H_1H_3 (from S_2 's general direction) because a majority of the senators would not concur. A similar argument applies to points above and below S_1S_3 . Points on H_2S_2 above H_1H_3 and below S_1S_3 are thus eliminated from any core. What remains is pq .

Note in this example that neither chamber has a core, nor does the unicameral legislature formed by combining the two chambers into one large chamber. Yet the bicameral game does have a core.

If one chamber has an even number of members, this increases the size of the core. Instead of having a single pivotal member at the end of a bicameral bisector, there are generally two bicameral bisectors leading to two pivotal members of the chamber with an even number of members. The area between these bicameral bisectors contains candidates for the core (see Hammond and Miller 1987, fig. 5 for an example).

What underlies the Figure 1 example is the fact that there is a set of points on a bicameral bisector that lies between all House chamber bisectors on the one hand and all Senate chamber bisectors on the other. With this observation, we now present a theorem about the existence of a core in bicameral games with one bicameral bisector:

THEOREM 2. *If (1) a bicameral legislature has only one bicameral bisector and (2) there is a point x on the bicameral bisector such that the chamber bisectors from one and only one chamber intersect at the bicameral bisector in each direction from x , then x is a core point.*

(Claim 2 in Cox 1980, p. 10 is implied by this theorem: nonoverlapping of chamber Pareto sets is sufficient for stability but not necessary.) This theorem applies to examples like Figure 1. In effect, for each x on pq , each House majority wants to move in one direction along pq and each Senate majority wants to move in the opposite direction along pq . Stalemate—stability—is the result. (See Appendix for formal proof.)

Bicameral games do not necessarily have a core. In general, the more the chambers resemble each other—the more overlapping their sets of ideal points—the less likely it is that bicameralism will create a core. While the authors of the Constitution desired the House and Senate to be substantially different from each other, it would seem for contem-

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porary Congresses that the ideal points of the two chambers are rather similar, perhaps due to the impact of the 17th Amendment requiring the popular election of senators, perhaps due to the development of the party system.

As a result, there will usually be more than just the one or two bicameral bisectors we have analyzed so far. In these more complex cases, determining whether a core exists is eased by the following theorem:

THEOREM 3. *When there is an odd number of members in each chamber, there are three or more bicameral bisectors, and the bicameral bisectors do not all intersect at the same point, then there is not a core in a bicameral game.*

The proof is simply that since each bicameral bisector is attractive both ways (Lemma 2), each bisector attracts points from off itself, including points on other bicameral bisectors. This means that every point in the space is attracted by some other point. Hence there is no core. Since it seems unlikely that three or more bicameral bisectors will all intersect at precisely the same point, we pose the following:

CONJECTURE 1. *When each chamber has an odd number of members and there are three or more bicameral bisectors, the bicameral game usually has no core.*

Examples of cases in which one or both chambers has an even number of members (see Hammond and Miller 1987, Fig. 11) lead us to venture the following proposition:

CONJECTURE 2. *When there are three or more bicameral bisectors and there is an even number of members in one or both chambers, there is usually a core in a bicameral game.*

The Preservation of Prior Cores in Bicameral Games

Bicameral games can have a core when neither chamber has a core. The significance of this result might be undercut if bicameralism could at times destroy any core that the larger unicameral game (combining all members in one chamber) might have. We can demonstrate, though, that when a unicameral game does have a core, any bicameral game derived from the unicameral game by partitioning its members into two chambers (holding constant each legislator's ideal point) will still have a core. In fact, it is easily proved that the bicameral core will include the unicameral game's core:

THEOREM 4. *If a unicameral game has a core, then the bicameral core exists and includes the unicameral core.*

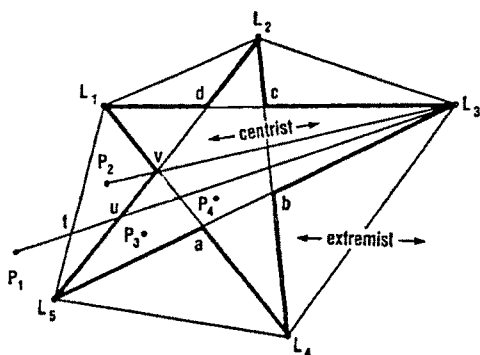
Proof. By contradiction: if the unicameral game has a core but the derived bicameral game has no core, this means that for every x in the issue space, there is a joint majority favoring some y to x . This implies that a majority of *all* legislators prefer the y to x . But this contradicts the condition that the unicameral game has a core. Hence, if the unicameral game has a core, the bicameral game must have a core. QED

This theorem means that bicameralism cannot be used as an institutional device for destabilizing simple majority rule in those cases in which simple majority rule produces stable outcomes. However, since bicameralism can increase the size of the core when there is a simple-majority-rule core, bicameralism can allow a different outcome than would occur due to simple majority rule.

The Executive-Veto Game

A bicameral game is not guaranteed to have a core. However, bicameralism is not the only constitutional provision that

Figure 2. A Five-Person Unicameral Legislature Facing a Chief Executive at Four Different Locations



can induce stability. We have already cited Hamilton's comment in *Federalist* number 73 that the executive veto would help maintain stability. It might be argued, on the basis of the historical record, that the veto is of little consequence in relations between president and Congress since it is used so infrequently. However, Hamilton suggested in *Federalist* number 74 that the veto does not have to be used to be effective: "A power of this nature in the executive will often have a silent and unperceived, though forcible, operation. When men, engaged in unjustifiable pursuits, are aware that obstructions may come from a quarter which they cannot control, they will often be restrained by the bare apprehension of opposition from doing what they would with eagerness rush into if no such external impediments were to be feared" (p. 446). If Hamilton was right, then we can expect the veto power to induce a core in an executive-veto game even if it is seldom exercised.

Since a simple veto allows the executive to veto any move from the executive's ideal point, that ideal point must be a part of a core. No further proof about this "executive veto game" is required to assert:

THEOREM 5. *The executive-veto game has a core that includes the executive's ideal point.*

We will next demonstrate that the core of the executive-veto game is not necessarily restricted to the executive's ideal point.

The Unicameral Executive-Veto Game

Here we explore executive veto games between an executive and a single legislative chamber. In Figure 2 the executive is located at any of the points labeled P and faces a five-person chamber. There are five chamber bisectors (L_1L_3 , L_3L_5 , L_5L_2 , L_2L_4 , and L_4L_1) which together enclose a five-pointed star defined by the $L_1L_3L_2L_4L_1$ sequence of points. Label an executive ideal point lying outside the star—outside the space completely bounded by chamber bisectors—as *extremist* and one lying on or inside the star—on or inside a space bounded on every side by chamber bisectors—as *centrist*.

If the executive's ideal point is centrist, the following is easily proved:

THEOREM 6. *If the chamber has no core and if the executive has centrist preferences, the core of a unicameral executive-veto game contains only the executive's ideal point.*

For example, in Figure 2 consider the executive ideal point at P_3 . We treat the executive as a separate chamber. There are three "bicameral bisectors" (not drawn)— P_3L_2 , P_3L_3 , and P_3L_5 . Since each bicameral bisector is attractive both ways, the executive can use some legislative coalition to upset every point in the space with an option closer to his or her own ideal point; the only point that cannot be upset is the ideal point. For similar reasons, an executive ideal point at P_4 is the only point in the core.

If the executive has extreme preferences—at P_1 or P_2 outside the star—the bicameral bisectors are P_1L_3 and P_2L_3

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respectively. With P_1 , the first chamber bisector that crosses the P_1L_3 bicameral bisector is L_2L_5 ; the point of intersection is u . The L_2L_5 chamber bisector attracts points from its right (the executive concurs) but not from the left (the executive would veto). Hence the core is P_1u . With the executive ideal point at P_2 , the core is P_2v .

The Bicameral Executive-Veto Game

The following theorem helps identify the core of a bicameral executive-veto game:

THEOREM 7. *The bicameral executive-veto core includes (but is not restricted to) the House executive-veto core, the Senate executive-veto core, and the House-Senate bicameral core when it exists.*

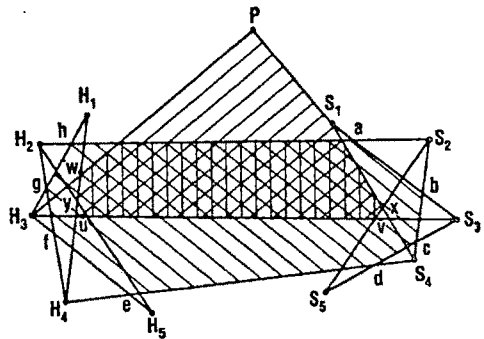
Proof consists of showing that if any two of the three institutions have a "bicameral" core, adding a third institution does not change the first two institutions' joint preference for points in their core over points not in their core. Any "bicameral" core cannot somehow be overruled just because some third institution prefers otherwise.

Figure 3 shows a five-member House and five-member Senate, with executive at P . The core of the bicameral House-Senate game is the uv segment of the H_3S_3 bicameral bisector. The executive-House core is the Pw segment of the PH_3 "bicameral bisector"; the executive-Senate core is the PS_1 segment of the PS_1 "bicameral bisector." The resulting bicameral executive-veto core, encompassed by the $PS_1xvuywP$ lines, is shaded by lines sloping down to the left.

The Veto-Override Game

A unicameral executive-veto game—an executive facing a single chamber—is

Figure 3. A Bicameral Legislature Facing a Chief Executive



guaranteed to be stable. A second legislative chamber is thus unnecessary for stability. The legislature's authority to override the executive's veto does resurrect the specter of instability. However, in *Federalist* number 73 Hamilton defends the override provision with a probabilistic argument about the inability of a bicameral legislature to override the veto. As he remarked, "It is to be hoped that it will not often happen that improper views will govern so large a proportion as two thirds of both branches of the legislature at the same time; and this, too, in defiance of the counterpoising weight of the executive. It is at any rate far less probable that this should be the case than that such views should taint the resolutions and conduct of a bare majority" (p. 446). We will argue that Hamilton's intuitions were largely correct. The stability induced by the veto power is undermined by the veto override, but the requirement of a *dual* override makes it likely that there will be some set of points that cannot be upset.

A result of Greenberg (1979) plays an essential role in our analysis. Define D as the number of policy dimensions under consideration, L as the total number of members in the (unicameral) legislature, m as a bare majority of L , and k as the

number of votes required to upset x in favor of some y ($m \leq k \leq L$). A *chamber k -core* is a set of points such that (1) for each y not in the set, there is some x in the set that is preferred by k or more members, and (2) no point in the set is preferred by k or more members to some other point in the set. A chamber k -core is, in effect, a generalization of a core for $k \geq m$.

We can now state

LEMMA 3. If $k > [D/(D + 1)]L$, a unicameral game has a chamber k -core (Greenberg 1979, Theorem 2).

When there are two dimensions ($D = 2$), for example, a chamber k -core is guaranteed to exist when $k > (2/3)L$. Assuming $k > m$, the following are easily shown:

LEMMA 4. If a chamber has a majority rule core, it necessarily has a chamber k -core.

(If a chamber has a chamber k -core, though, it does not necessarily have a majority rule core.)

LEMMA 5. If a chamber has no k -core, it has no majority-rule core.

Now define a *chamber k -sector* as a chamber-member/chamber-member contract curve that has k members on or to one side and the remaining members on the other side. Assuming at least k votes are needed to approve a motion, whenever $k > m$, the following is obvious:

LEMMA 6. A chamber k -sector is attractive only one way.

In Figure 2, there are four chamber k -sectors, L_1L_3 , L_2L_4 , L_3L_5 , and L_4L_1 . Assuming $k = 4$, together they define the chamber 4-core: it is the pentagon, $abcdva$, internal to the five-pointed star fixed by the five legislators' ideal points.

For a bicameral legislature the analogue of a chamber k -core is a *bicameral k -core*.

It is a set of points that no coalition of k_H House members plus k_S Senate members can upset. To find a bicameral k -core, define a *bicameral k -sector* as a House-member/Senate-member contract curve with k_H House members and k_S Senate members on or to one side of the line. Assuming k members in each chamber are needed to approve a motion, it is obvious that

LEMMA 7. A bicameral k -sector is attractive only one way.

In the bicameral legislature in Figure 3, with $k_H = k_S = 4$, the bicameral 4-core is the $abcdefgha$ region (shaded by lines sloping down to the right) circumscribed by the bicameral 4-sectors (H_2S_2 and H_4S_4) and the chamber 4-sectors (H_1H_3 , H_2H_4 , and H_3H_5 for the House and S_1S_3 , S_2S_4 , and S_3S_5 for the Senate). This bicameral 4-core contains points that no coalition of four or more House members plus four or more Senate members can upset.

Now assume that k is the number of members required to override a veto. Call a chamber majority with at least k members an *override majority* (also called a *k -majority*). Similarly, a *joint override majority* (a *joint k -majority*) has at least k_H members in the House and at least k_S members in the Senate. For simplicity in the following analysis we assume chambers of equal (and odd) size and $k_H = k_S = k$.

Given these preliminaries, what conditions are necessary for a core to exist in a veto override game? There are three different cases to examine, whether we are considering a unicameral or bicameral legislature.

For the first case, assume that a k -core (unicameral or bicameral) does not exist; if $k \leq (2/3)L$, for example, a k -core will not necessarily exist. If no k -core exists, there can be no veto-override core. The reason is simple. Since there is no k -core, by definition this means that for any x in

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the executive-veto core (which always exists) there is always some override majority (or joint override majority) that can upset x in favor of some y , another override majority that can upset the y in favor of some z , and so forth. In effect, the legislature can eliminate any point in the executive-veto core but, with no k -core, is incapable of replacing the executive-veto core with any points of its own. Hence we can state

THEOREM 8. *If there is no legislative k -core, there exists no veto-override core.*

For the second case, assume that there is a legislative k -core but that the executive-veto core (which always exists) does not intersect it. In this case, too, there will be no veto-override core. The reason? The executive-veto core always attracts points lying outside it. When the core of the executive-veto game does not intersect the k -core, this means that there is an executive-led coalition (containing the executive plus a legislative majority or joint majority) that can upset any point in the k -core. But since the k -core is also attractive (due to the bicameral and/or chamber k -sectors that bound it), any point in the executive-veto core can be overridden by some override majority (or joint override majority) preferring a point in the k -core. Outcomes thus bounce back and forth between the executive-veto core and the legislative k -core. So when the executive-veto core does not intersect the legislative k -core, the veto-override game has no core.

For the third case, assume that the executive veto core *does* intersect the legislative k -core. In this case the veto-override game has a veto-override core, which is the area of intersection of the executive-veto core and the legislative k -core. Consider some point x in this intersection. Since x is in the executive-veto core, no executive-led coalition can upset x . But since x is also in the legis-

lative k -core, there is no legislative override majority that can upset x in favor of something else. Hence x is in the veto-override core. The following theorem thus holds:

THEOREM 9. *For a veto-override game (unicameral or bicameral), there is a veto-override core if and only if (1) the legislature has a k -core, and (2) the core of the executive-veto game intersects this k -core.*

Note an immediate corollary of Theorem 9:

THEOREM 10. *If unanimity is required for the legislature to override, a veto-override game always has a core.*

The reason is that the legislature's entire Pareto set becomes the k -core here, and an executive-veto core always intersects this Pareto set.

We now illustrate these various points.

Unicameral Veto-Override Games

Define an executive's ideal point as *k-centrist* if it lies in a chamber k -core. Otherwise, the executive's ideal point is *k-extremist*.

Now consider a five-person legislature with no core, as in Figure 2. Assume $k = 4$. As previously noted, the legislative 4-core here is the internal pentagon, $abcdva$. With a k -extremist ideal point at P_1 , the executive-veto core is the P_1u segment of the P_1L_3 bicameral bisector. Since the executive-veto core does not intersect the legislative 4-core, there is no veto-override core: the executive-veto core attracts points from the legislative 4-core, and vice versa. An executive ideal point at P_3 is also unstable: P_3 is the unique executive-veto core, but it does not intersect the legislative 4-core, hence there is no veto-override core. However, the executive with preferences at P_2 generates an executive-veto core, P_2v , which inter-

sects the legislative 4-core at v . Since v is in the executive-veto core, there is no executive-led coalition that will upset it. Since v lies in the 4-core, there is no four-legislator override coalition that can upset it. Hence v is the unique core to this veto-override game.

In general, if an executive has a k -centrist ideal point, the executive-veto core will always intersect the legislative k -core. Hence the following corollary to Theorem 9:

THEOREM 11. *If there exists a unicameral k -core and the executive has a k -centrist ideal point, the unicameral veto-override game has a core.*

Bicameral Veto-Override Games

Analysis of bicameral veto-override games is similar to that of unicameral veto-override games. Consider Figure 3. The core of the bicameral executive-veto game is the $PS_1xvuywP$ area shaded by diagonal lines sloping down to the left. Assuming $k = 4$, the bicameral 4-core is the $abcdefgha$ region, as previously noted, shaded by lines sloping down to the right. The intersection of the bicameral executive-veto core and the bicameral 4-core is shaded by vertical lines. The joint-override majority eliminates points in the bicameral executive-veto core above the H_2S_2 bicameral 4-sector. The coalitions supporting the executive-veto core eliminate points in the bicameral 4-core below the H_3S_3 bicameral bisector as well as points lying to the left of the H_1H_4 and H_2H_5 chamber bisectors and the PH_3 bicameral bisector and to the right of the S_1S_4 and S_2S_5 chamber bisectors. But some portions of the executive-veto core intersect the bicameral 4-core. This region, shaded by the vertical lines, is the bicameral veto-override core.

The executive ideal point in Figure 3 lies *outside* the bicameral k -core. The follow-

ing summarizes what happens if it is *inside*:

THEOREM 12. *If there is a bicameral k -core and the executive's ideal point lies inside it, the bicameral veto-override game has a core.*

The reason of course is that the executive-veto core here necessarily intersects the bicameral k -core. If there is no bicameral k -core, the bicameral veto-override game will have no core. But the following theorem holds:

THEOREM 13. *If either chamber has a chamber k -core, (1) the bicameral legislature has a bicameral k -core, and (2) this bicameral k -core is not restricted to the chamber k -core.*

Finally, the following theorem may seem obvious but will help clarify the discussion in the next section:

THEOREM 14. *If a House-Senate bicameral game has a core, the veto-override game has a core that includes the bicameral core.*

Likelihood of a Bicameral Veto-Override Core

Does the Constitution, which requires a two-thirds vote in each chamber to override a presidential veto, guarantee stability? The answer is that the Constitution does not *guarantee* stability, but we will argue that stability is nonetheless very likely in the two-dimensional case.

Lemma 3 (Greenberg 1979) guarantees a chamber k -core if $k > (2/3)L$. With a 435-member House, a two-thirds override requires 290 votes. But $290/435 = 2/3$, hence no House k -core is guaranteed to exist. With a 100-member Senate, 67 votes are required for override; this is more than two-thirds. The Senate is thus guaranteed to have a k -core. Hence by Theorem 13 the Congress is guaranteed to

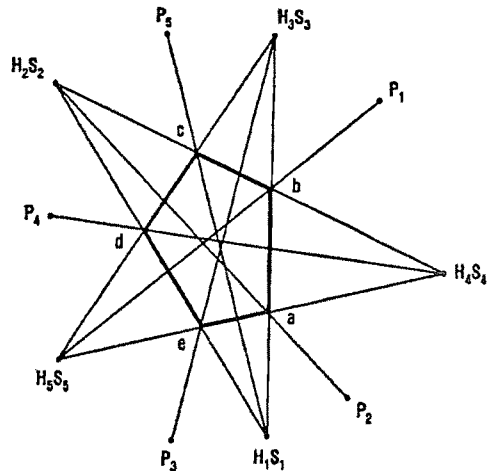
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have a bicameral k -core. But there is no guarantee that the executive-veto core will intersect this bicameral k -core.

Nonetheless, we argue that Hamilton's probabilistic argument is likely to be correct. Let us establish a framework for analyzing his argument. If there is sufficient separation between the ideal points of the House and Senate, as in Figure 3, there will be a core to the bicameral game (Theorem 2) and hence a core to the bicameral veto-override game (Theorem 14). At the other extreme, if there are no differences at all between House and Senate ideal points (in terms of paired House-Senate members), then we have a bicameral version of the unicameral veto-override game. In this case, bicameralism does not add any stability since one chamber is simply the mirror image of the other. The bicameral veto-override game's potential for stability here will be limited to conditions similar to those for unicameral veto-override games (Theorem 9).

Now consider this case in which there is no difference between chambers (see Fig. 4). Each chamber has five members and all five House-Senate pairs perfectly coincide. Assume $k = 4$. An executive inside the bicameral 4-core (labeled $abcdea$ and shown by the heavy outline) will generate an override core (Theorem 12). With an executive ideal point outside the bicameral 4-core, there are five rays on which an executive must lie for there to be a bicameral override core. These rays are the five executive-legislative bisectors— $P_1H_5S_5$, $P_2H_2S_2$, $P_3H_3S_3$, $P_4H_4S_4$, and $P_5H_1S_1$ —that intersect the bicameral 4-core. Only if the executive ideal point lies on one of these five rays—which is very unlikely—will the resulting bicameral executive-veto core intersect the bicameral 4-core. An executive at P_1 , for example, creates an executive veto core, P_1b , which intersects the bicameral 4-core at b . An executive ideal point not on one of these five bisectors will generate no

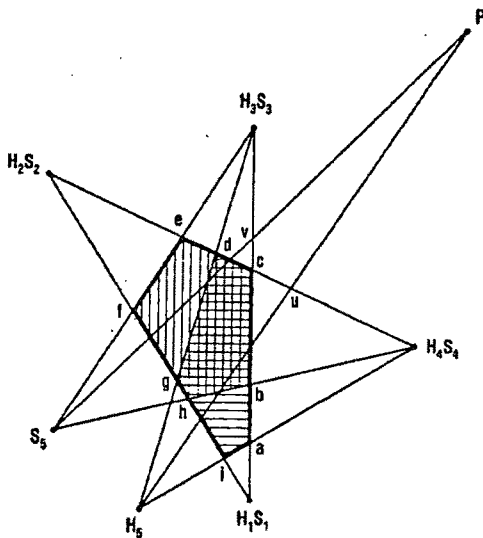
Figure 4. A Bicameral Legislature Facing a Chief Executive at Five Different Locations



override core since the executive-veto core will not intersect the bicameral k -core.

However, if the members of even just one House-Senate pair of legislators do not have identical preferences, then there are numerous prospects for stability that did not previously exist. Consider the two five-person chambers in Figure 5. Members of four of the five pairs of legislators— H_1S_1 , H_2S_2 , H_3S_3 , and H_4S_4 —have identical preferences; only H_5 and S_5 differ. Assume $k = 4$. There is no core to the overall 10-legislator unicameral game because the required symmetry conditions are not met. Nor does either chamber have a core. The bicameral game between the two chambers has no core because there are more than two bicameral bisectors that do not all intersect at the same point (Theorem 3). Furthermore, there is no veto-override core for the president-House veto-override game since the House's executive-veto core, Pu , does not intersect the House 4-core, $acdga$ (horizontal shading). Nor is there a veto-override core for the president-Senate

Figure 5. Intersection of Executive-Veto Core with Bicameral 4-core



veto-override game since the Senate's executive-veto core, Pv , does not intersect the Senate 4-core, $bcdefhb$ (vertical shading). So if there is a core to the bicameral veto-override game here, it must be due to the *interaction* between bicameralism and the executive's veto.

And in fact, there is a core to this bicameral veto-override game. By Theorem 9, a bicameral override core exists if the bicameral 4-core intersects the bicameral executive-veto core. The bicameral executive-veto core is the $PucvP$ area. The bicameral 4-core is, in this case, simply the set union of the chamber 4-cores, producing the *acefia* pentagon, which has the heavy outline. The executive-veto core and the bicameral 4-core do intersect but only at c . Hence c is the core of this bicameral veto-override game. Note that P can be moved around substantially without eliminating c as the override core.

The modest divergence between the House and Senate, involving just H_5 and S_5 , is thus sufficient to create possibilities

for an override core that did not exist in Figure 4. If this divergence between H_5 and S_5 increases, the possibilities of a core increase. While space does not permit the presentation of further examples (see Hammond and Miller 1987, Figs. 21, 22), it appears that as additional cross-chamber pairs of legislators diverge, then the existence of a core becomes less and less sensitive to the location of the executive's ideal point. These increasing differences in legislative ideal points increase the size of both the bicameral k -core and the executive-veto core, making it increasingly likely that the two sets will intersect somewhere around the periphery of the bicameral k -core.

Note that this analysis has presumed the executive has an ideal point lying outside the bicameral k -core. If the executive's ideal point lies inside the bicameral k -core, recall that the bicameral veto-override game is guaranteed to have a core (Theorem 12).

Finally, it is important to note that unlike the simple bicameral game, large overlaps of the Pareto optimal sets for the two chambers do not necessarily diminish the likelihood of a bicameral veto-override core. It appears sufficient that the *individual legislators* are not identically matched across chambers. For this reason we feel safe in making the following conjecture:

CONJECTURE 3. *Bicameral veto-override games usually have a core.*

Summary of the Constitutional Order

Six different sets of constitutional provisions have now been examined: unicameral games (pure majority rule), bicameral games, unicameral executive-veto games, bicameral executive-veto games, unicameral veto-override games, and bicameral veto-override games. Considering the many complex relationships

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among games that have been presented, we now need to organize the results.

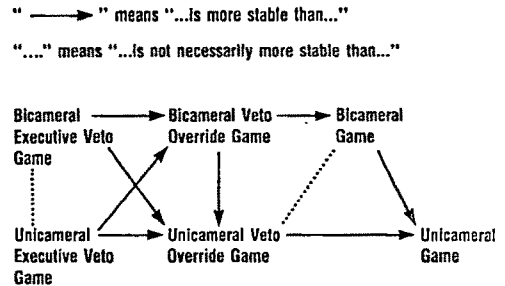
Define a *preference profile* as the set of preference orderings held by a group of legislators. We will say that constitution *A* is more stable than constitution *B* if two conditions hold: (1) if a given profile has a core under *B*, then that profile must have a core under *A*; and (2) there is some profile that has a core under *A* that does not have a core under *B*. Figure 6 summarizes the is-more-stable-than relationships among the six games. If the recipient of an arrow has a core for some preference profile, the originator of the arrow also has a core for that profile as well as for some other profiles. Since the is-more-stable-than relationship is transitive, we can deduce the relationship between each pair of games. For example, the bicameral executive-veto game is more stable than the bicameral veto-override game, the unicameral veto-override game, the bicameral game, and the unicameral game.

A *noncomparability* relationship is shown by a dotted line. Games are non-comparable in terms of stability for either of two reasons. In one kind of case, since both the bicameral and the unicameral executive-veto games are guaranteed to have cores, neither is more stable than the other. In the other kind of case, the nature of the relationship simply depends on the particular preference profile under consideration, as with unicameral veto-override games and bicameral games.

We should emphasize that the results upon which Figure 6 is based depend on our two key assumptions:

First, we have assumed that our two issue dimensions are equally important to every legislator, thereby producing circular indifference curves. The existence of executive-veto cores does not rely on an assumption about the shape of indifference curves. But the extent to which other results can be generalized when legislators and executives have noncircular indif-

Figure 6. Stability Relationships among Six Different Legislative Games



ference curves must be ascertained.

Second, we have presumed the existence of only two issue dimensions. Several of our results, to be sure, can be generalized to higher dimensions. But for a bicameral *k*-core to exist in higher dimensions requires a larger and larger value of *k* (see Lemma 3). Similarly, with a fixed value of *k*, as the number of dimensions increases, it seems reasonable to think that any bicameral *k*-core would get smaller and smaller, and there would be a decreasing likelihood that it would even exist. As a result, the executive-veto core (which will always exist) would be less and less likely to intersect a bicameral *k*-core (even assuming one does exist). This in turn means a decreasing likelihood that a veto-override core would exist.

Conclusions

We have examined the extent to which the Constitution creates a stable political order. To Madison, however, stability was not the only value the political order should have. In *Federalist* number 37, for example, he wrote:

Among the difficulties encountered by the convention, a very important one must have lain in combining the requisite stability and energy in government with the inviolable attention due to liberty and to the republican form. . . . Energy in government is essential to that security against

external and internal danger and to that prompt and salutary execution of the laws which enter into the very definition of good government. *Stability in government* is essential to national character and to the advantages annexed to it, as well as to that repose and confidence in the minds of the people, which are among the chief blessings of civil society. An irregular and mutable legislation is not more an evil in itself than it is odious to the people; and it may be pronounced with assurance that the people of this country, enlightened as they are with regard to the nature, and interested, as the great body of them are, in the effects of good government, will never be satisfied till some remedy be applied to the vicissitudes and uncertainties which characterize the State administrations (pp. 226-27, emphasis added)

From the Madisonian perspective, then, a properly designed Constitution requires not only stability-inducing features but several others as well.

We have little to say about the relationship of *stability in government* to *energy in government*, to the *republican form of government*, or to *liberty*. It is easily demonstrated, however, that any core (if it exists) to one of our games meets one test of "good" public policy, that of Pareto efficiency. A policy is Pareto efficient if there exists no other policy unanimously preferred to it. If there is a core to a bicameral game, for example, this means that for every alternative in the core there is no other policy preferred by a joint majority. And since there is no other policy preferred by a joint majority, it obviously follows that there is no other policy preferred unanimously, by all members of both chambers. From this we can conclude that points in the bicameral core are all Pareto efficient.

Given our focus on stability, one might plausibly raise the question, as have numerous critics of U.S. democracy, as to whether the constitutional system is, in some sense, *too stable*. Is it, for example, *unresponsive* to changes in the locations of the ideal points of legislators and the president?

A well-developed answer to this question requires another paper entirely. But

our results may give some insight into the problem. It appears that the location of any bicameral veto-override core is quite responsive to changes in the location of presidential ideal points. On the other hand, this override core may be relatively resistant to large-scale changes in legislative preferences. In Figure 3, for example, as long as the locations of the president and legislators H_3 and S_1 do not change, many of the other legislators' ideal points can move about in many different ways without much impact on the shape, size, or location of the override core.

This kind of pattern may have been what was intended by those who drafted the Constitution. The (apparent) lack of responsiveness of the core to changes in legislative preferences certainly seems compatible with the concern of Madison and Hamilton about the "mutability in the laws." And it may not be stretching things too much to suggest that the responsiveness of the core to changes in presidential preferences is an essential component of *energy in government*. If these inferences and interpretations prove correct, then our results can be read as a formal demonstration that the Constitution should work, at least in part, as designers like Madison and Hamilton argued it would.

Appendix

Proof of Theorem 1

THEOREM 1. *A point x is in the core if and only if no straight line through x leaves a majority of both chambers to one and the same side of that line.*

Proof. We first show that if x is in the core, then no straight line through x leaves a majority of both chambers to one and the same side of that line. It is a proof by negation, so we assume (see Fig. A-1) that there is some line RS through x that leaves a majority of both chambers to one and the same side of the line.

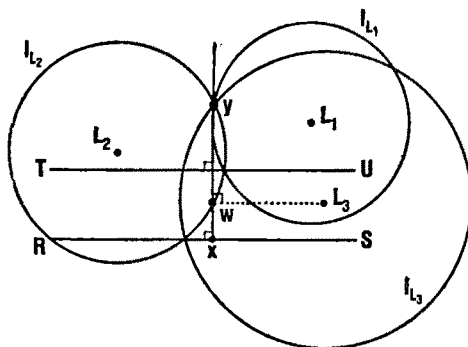
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Consider legislator L_3 who is in the joint majority to one side of RS and who is closest to the RS line. Now construct a point w the same distance to the same side of the RS line, on a line perpendicular to RS . Now w must be closer to every legislator in that joint majority than is x . As a result, every legislator in the joint majority prefers w to x , and x cannot be in the core, contrary to assumption. Hence, the assumption that there is a joint majority to one and the same side of the line must be wrong.

We next demonstrate that if there is an x such that no line through x has a majority of both chambers to one and the same side of that line, then x is in the core. We will assume there is some y preferred by a joint majority to x and establish a contradiction.

Consider some point y , which, by assumption, is preferred by a joint majority to some x . This means that for a majority of the House and a majority of the Senate, x lies strictly outside their indifference curves through y . Now consider the diagram in Figure A-1. Draw a straight line from x to y . Draw two perpendiculars through the xy line, one through x —producing line RS —and one through a point that bisects xy —producing line TU . Consider any legislator, L_i , in this joint majority. The only way for L_i 's indifference curve through y not to contain x is for L_i to lie above the TU perpendicular; see the indifference curves of L_1 , L_2 , and L_3 through y , for example. But if the ideal points of all legislators in this joint majority who prefer y to x (e.g., legislators like L_1 and L_2 but not L_3) lie above the TU line, then majorities of both the House and Senate necessarily also lie above the RS line through x . Hence there is a majority of both chambers whose ideal points are to one and the same side of a line through x , contrary to the assumption. Thus the assumption that x is not a core point must be wrong. QED.

Figure A-1. Establishing General Conditions for the Existence of a Core in a Bicameral Game



Proof of Theorem 2

In proving Theorem 2, two lemmas are useful:

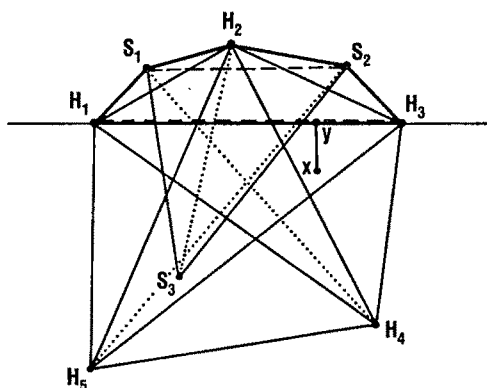
LEMMA A-1. *No point in the Pareto set of a joint majority of minimal size can be upset by some other point in this Pareto set due to the votes of the members of this joint majority.*

This lemma follows directly from the definition of the Pareto set of a group of actors, which is the set of alternatives from which they cannot agree to move.

LEMMA A-2. *Given some joint majority of minimal size, at least one of the contract curves connecting the ideal points of a given chamber's members in the joint majority will be a chamber bisector.*

Proof. Draw the Pareto set of just the House members in the joint majority. Consider some point x outside this House Pareto set but inside the Pareto set of the entire House; see point x in Figure A-2, for example. This point is outside of the Pareto set for coalition H_1 , H_2 , and H_3 . Establish some y on the nearest House

Figure A-2. At Least One Chamber-Member/Chamber-Member Contract Curve Will Be a Chamber Bisector



contract curve in the joint majority's Pareto set by dropping a perpendicular to the nearest House contract curve. Since by assumption the joint majority is of minimal size, only a bare majority of the House will prefer y on or in this House Pareto set to the x lying outside this House Pareto set. If this is not true for at least one House contract curve in this Pareto set, either (1) more than a bare House majority prefers y to x , which violates the assumption that the joint major-

ity is of minimal size, or (2) less than a bare House majority prefers y to x , which violates the assumption that we are considering a joint majority. Since a bare House majority lies on or to one side of the House contract curve, a bare House majority must lie on or to the other side. Hence this House contract curve is a House chamber bisector. The same logic holds for the Senate contract curves in the minimal-size joint majority. QED

We can now prove

THEOREM 2. *If (1) a bicameral legislature has only one bicameral bisector and (2) there is a point x on the bicameral bisector such that the chamber bisectors from one and only one chamber intersect the bicameral bisector in each direction from x , then x is a core point.*

Proof. By negation: we assume Conditions (1) and (2), but assume that some point y beats x .

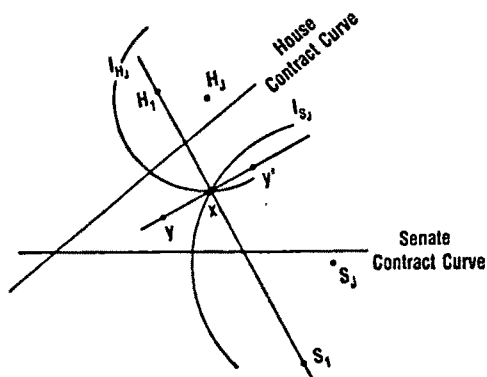
The House and Senate members at either end of (and defining) the bicameral bisector will be called H_1 and S_1 (see Figure A-3). Examining the joint coalition that prefers y to x , there are three mutually exclusive and collectively exhaustive cases to consider, each of which leads to a contradiction.

Case 1. Both H_1 and S_1 are members of the joint coalition preferring y to x .

The first combination is easily ruled out as a minimally sized joint majority that can upset x . Since the interests of H_1 and S_1 are diametrically opposed along H_1S_1 , no minimal joint majority that includes both H_1 and S_1 can prefer some y to x . Some y off the H_1S_1 bicameral bisector would be rejected by both H_1 and S_1 in favor of some point on the bicameral bisector, while movement along the bicameral bisector necessarily leaves one one of them worse off. So the joint majority preferring y to x will necessarily lack either H_1 or S_1 or both.

Case 2. Neither H_1 nor S_1 are members

Figure A-3. Establishing the Existence and Location of a Core in a Bicameral Game



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of the joint coalition preferring y to x .

We next prove that no minimally sized joint majority lacking both H_1 and S_1 can exist that prefers some y to x . Draw a perpendicular to H_1S_1 through x . We will show that the indifference curves through x of a Senate majority cannot all cross the perpendicular from below and that the indifference curves through x of a House majority cannot all cross the perpendicular from above. This will mean that the $H^{-1}(x)$ win set of a House majority cannot possibly intersect the $S^{-1}(x)$ win set of a Senate majority. Recall that H_1S_1 is the only bicameral bisector. Hence, to the right of H_1S_1 there is, by definition, no more than one less than a majority of the Senate, while to the left of H_1S_1 there is no more than one less than a majority of the Senate. Since we have by assumption ruled S_1 (as well as H_1) out of the joint majority upsetting x in favor of y , the Senate members of the joint majority must come from *both* the right *and* the left of H_1S_1 ; no other way of mustering a Senate majority is possible. However, no indifference curve through x of any Senator to the right of H_1S_1 can possibly cross the perpendicular on the left of H_1S_1 (see the indifference curve of S_j , labeled I_{S_j} , for example). Similarly, no indifference curve of any House member to the right of H_1S_1 can possibly cross the perpendicular on the left of H_1S_1 (see the indifference curve of H_j — I_{H_j} —for example). Hence the $H^{-1}(x)$ and $S^{-1}(x)$ win sets cannot intersect to the left of H_1S_1 . As a result, there can exist no joint majority that prefers some y to the left of H_1S_1 . An identical argument demonstrates that there can exist no joint majority that upsets x in favor of some y' to the right of H_1S_1 . Therefore, no joint majority including *neither* H_1 nor S_1 will prefer some y to the x .

Case 3. Either H_1 or S_1 are members of the joint coalition preferring y to x , but not both.

Assume without loss of generality that

it includes only H_1 . Hence a joint majority including H_1 that upsets x in favor of some y must include a majority of Senate members not involving S_1 . Since there is no Senate majority that lies strictly on one side of the bicameral bisector, the joint majority including H_1 must include Senate members from both sides of the bicameral bisector.

But the Pareto set of the minimal-size joint majority just described—containing H_1 and senators (but not S_1) on both sides of the bicameral bisector—will encompass space on both sides of the bicameral bisector. Now consider the Pareto set of just the Senate members of this minimal-size joint majority. The boundaries of the Senate-majority Pareto set will cross the bicameral bisector at two places, at the top of the set (toward the H_1 end) and at the bottom of the set (toward the S_1 end). By Lemma A-2, one of these lines crossing the bicameral bisector will necessarily be a Senate chamber bisector. If it is not a Senate chamber bisector, either (1) the number of senators above the line is less than a majority, in which case x is not in fact upset by the joint majority—contrary to the assumption that x is upset, or else (2) the number of senators above the line is more than a bare majority, in which case we have more than a minimal Senate majority contributing to the upsetting of x —contrary to the assumption that we are dealing with a minimal-size joint majority.

Now, since we have assumed there is no bicameral core, point x must lie outside the Pareto set of the minimal-size joint majority (Lemma A-1), which necessarily means that it lies *below* the Senate chamber bisector just described, which crosses the bicameral bisector. But this means that a Senate chamber bisector crosses the bicameral bisector on the same side as the House majority. This in turn means that there are both House and Senate chamber bisectors intersecting the bicameral bisector on the same side of x . This violates Condition 2. Again, in Case 3, we arrive

at a contradiction.

Since a contradiction is reached in all possible cases, it cannot be the case that x is not a core point. QED

Note

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TOCQUEVILLE'S CONSTITUTIONALISM

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For the ancient philosophers, constitutionalism meant classifying regimes and constructing regimes to form virtuous citizens. In the modern world it generally means checks and balances, institutional mechanisms limiting the power of government and protecting private rights. In *Democracy in America* Tocqueville attempts to combine both views in his interpretation of the U.S. constitutional system. He employs the regime analysis of ancient constitutionalism to understand the new phenomenon of popular sovereignty and its potential for despotic control over the minds and characters of citizens. At the same time, he shows how the constitutional devices found in the United States—such as federalism, judicial review, and the separation of powers—can be adapted to inculcate a kind of moral virtue by teaching citizens to exercise liberty with moral responsibility and to govern themselves. The result is a constitutional theory that weaves ancient and modern principles into an original and coherent whole.

Most U.S. citizens and legal scholars think of constitutional government as a system of legal and institutional restraints on power that protect private rights. To them, *constitutionalism* consists chiefly in knowing the number and kinds of rights that deserve constitutional protection or in examining the institutional mechanisms that limit and balance the powers of government. This commonly accepted view, however, is not the only conception of the term.

A different view is presented by Alexis de Tocqueville in *Democracy in America* (1945). As I shall argue, Tocqueville conceives of constitutionalism as more than a device for limiting power and protecting rights. Appealing to an older tradition, he argues that laws and institutions must be examined in light of the regime or whole social order underlying them. Such an analysis reveals that the modern democratic revolution has created a new kind of despotic control known as "popular

sovereignty," which threatens to destroy the intellectual and moral character of citizens and make them unfit for liberty. Accordingly, constitutional government must go beyond the mere concern with constraining power through institutional mechanisms and take a deliberate interest in the moral education of citizens. This insight, I hope to show, leads Tocqueville to an interpretation of the U.S. Constitution that combines ancient and modern principles in an original theory.

Definitions of Constitutionalism

In the voluminous literature on constitutional government, a great deal of effort is devoted to determining what is meant by *constitutionalism*. The ambiguity of the term is such that most scholars acknowledge a variety of meanings, including some that seem to be mutually contradictory. For the purpose of under-

standing Tocqueville's theory, we shall examine three of the most important definitions and then indicate how Tocqueville's conception may be distinguished from them.

The most general definition can be found in the works of such eminent legal scholars as McIlwain and Corwin. They define *constitutionalism*, in the broadest sense, as "the legal limitation on government . . . [whose] antithesis is arbitrary rule" (McIlwain 1947, 21) or "the belief in a law superior to the will of human governors" (Corwin [1928], 1971, 5). The emphasis here is on the limitation of power by law, specifically by a law that stands above the will of the rulers or the ordinary statute law. Such a fundamental law might be a written code or document; but more commonly it is an unwritten "higher law" that could be discovered, articulated, and enforced by a variety of authorities. Hence, both McIlwain and Corwin include a large number of higher-law theories in this category—classical natural right, Roman law, Christian natural law, English common law, modern natural rights, and the U.S. Constitution. So sweeping is this notion that Corwin claims to unfold an unbroken tradition of such theories from Aristotle and Cicero to John of Salisbury to Sir Edward Coke to Locke and Madison (Corwin [1928], 1971, 9-89; see also Wormuth 1949). This definition is obviously the most inclusive; for it treats every type of higher law as a kind of constitutionalism and views the differences among types of higher law as less significant than their status as norms or rules above the human will.

Though useful as a beginning point, this definition does not do justice to the variety of phenomena claiming the title *constitutionalism*. Hence, many scholars distinguish between *ancient* and *modern* constitutionalism, acknowledging thereby that the ancient Greeks and Romans meant by *constitutionalism* something

quite different from what we think of today. McIlwain, for example, points out that the word often translated as "constitution" in the writings of Plato and Aristotle is *politeia*, a term referring to the entire social order of a city or nation as well as the form of government, "its whole economic and social texture as well as matters governmental in our narrow modern sense" (McIlwain 1947, 24; Wheare 1966, 1). In a recent article that attempts to resurrect the ancient meaning of *constitution*, Maddox refers to Cicero's use of *constitutio*—the Latin noun from which the English *constitution* is derived. It means "the 'shape,' 'composition,' or 'establishment' of a people in their political association" (Maddox 1982, 807). In both cases, the words emphasize the ordering of the whole association, the way its various parts are organized into an identifiable form or structure.

One might add to these observations that the classical philosophers and historians trace the form of a political association to the relation between ruler and ruled, specifically to the way the ruling group or dominant class imposes its ideas of what is just and honorable on the ruled and molds the characters and tastes of citizens. Perhaps the best translation of *politeia*, therefore, is not "constitution" but "polity" or "regime": the whole political and social order as it is shaped by the ruling group and its vision of the good life. Using the notion of a *regime*, classical political science seeks to identify the common patterns and to classify them according to the number and types of people who make up the ruling body—a *monarchy* is a regime ruled by one preeminent individual or family; an *oligarchy* is rule by the wealthy few; a *democracy* is rule by the common people, and so on.

The purpose of such a classification, of course, is not merely to describe structures of power but to guide the legislator in choosing the regime which is best or the best possible in the given circumstances.

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Guided by a "higher-law" standard of natural right that prescribes *virtue* as man's natural end, ancient constitutionalism turns the classification of regimes into an exercise in moral choice, a problem of choosing the regime best suited to make men virtuous—to make them courageous, moderate, just, and prudent. For most of the classical philosophers and historians, this means choosing an aristocracy of wise and virtuous gentlemen in the ideal circumstances, a mixed or middle-class regime in less ideal circumstances, and a democracy tempered by rule of law and public spiritedness as a realistic third choice (Aristotle, *Politics* 3, 4, 7; Polybius, *Histories* 6). In speaking of ancient constitutionalism, then, one is referring to *regime analysis*: the classification of regimes and the prudential choice of which regime or combination of regimes is best suited to mold virtuous citizens.

Neither the general definition nor the ancient view, however, adequately captures the way *constitutionalism* is used today. The typical U.S. citizen, as well as the majority of contemporary legal scholars, use the terms *constitution* and *constitutional government* to mean something more precise than a higher law and more limited than the regime as a whole. They mean institutional restraints on power that protect private rights—such as the separation of powers, electoral accountability, or systems of checks and balances. According to McIlwain, this is the distinctively modern understanding of constitutionalism (McIlwain 1947, 115, 142). Another distinguished legal scholar, Sartori, calls this meaning of constitutionalism the *garantiste* meaning (emphasizing the guarantees for individual liberty) and insists that it is the only correct understanding of the word (Sartori 1962, 863; also, Friedrich 1946, 4, 21; Wheare 1951, 10-11; Wormuth 1949, 3). One should add that this definition expresses the view of James Madison and the other

authors of the *Federalist Papers*. They viewed the U.S. Constitution as a system of checks and balances that limits the powers of the federal government in order to secure the rights of persons and property (*Federalist* nos. 9-10; Madison 1981, 36, 62). According to the modern conception, constitutionalism is grounded in the "higher-law" doctrine of natural or human rights and focuses not on the way regimes mold the characters of citizens but on the institutional mechanisms for limiting, dividing, and balancing the powers of government.

Ancient Constitutionalism and the Modern Phenomenon of Sovereignty

In turning to Tocqueville's analysis of the U.S. Constitution in *Democracy in America*, one senses that it does not fit neatly into any of the previously mentioned definitions of constitutionalism. At first glance, the conceptual framework of the whole of *Democracy in America* seems to follow the lines of ancient constitutionalism. It is a comparison and evaluation of two regimes: democracy and aristocracy (or its European variant, feudalism). Yet, as one scholar has noted, the terms Tocqueville employs to describe the regimes are sometimes confusing because they reflect both "political" and "social" categories (Richter 1970, 90; Richter 1969, 156-59). Thus, Tocqueville defines *democracy* as the rule of the people or popular sovereignty; but he traces the ultimate foundation of democracy to "equality of social condition," which he understands in socioeconomic terms as the absence of hereditary classes or fixed social hierarchies (vol. 1, Chaps. 3, 4). This leads some commentators to the conclusion that Tocqueville is one of the founders of modern sociology (Aron 1968, 237). A more accurate view, however, is that Tocqueville uses some of the

language and categories of modern sociology but does so in the service of an older conception of political science. His emphasis on the role of social classes and their characteristic patterns of behavior is actually an emphasis on the *regime* in the classical sense (Zetterbaum 1967, 52). For the central theme of the book is that all aspects of life in democratic America are shaped by the ruling group, the sovereign people, and by belief in equality, which they establish as just and authoritative. Our first impression of Tocqueville, then, is generally correct: his enterprise is not sociology but the regime analysis of classical political science.

Yet, one cannot deny that Tocqueville continually expresses himself in language that gives primacy to social and cultural phenomena over those that are narrowly political. This tendency is particularly evident in his statements about the organization of *Democracy in America* and his method of analysis. Tocqueville states that his work is divided into two parts: the subject of the first part is the *laws* of the United States; the subject of the second is the *customs* (1:15). A glance at the table of contents of Part 1 indicates that by the *laws* he means more than the legislative acts of the U.S. government; he means the entire constitutional system—the federal and state constitutions, the formal and informal institutions of government, and the conceptions of justice underlying the laws and institutions. Although Tocqueville uses the term *laws* in a comprehensive sense to mean the constitutional system and devotes the first part (18 chapters) to explaining its operation, he clearly states that such matters are only one part of the whole phenomenon of democracy in the United States. He asserts that three causes or factors must be considered in order to understand the democratic phenomenon in America: the accidental circumstances (geography, climate, and historical contingencies), the laws (constitutions and

institutions), and customs (mores or habits, sentiments, and beliefs). Of these three causes, the accidental circumstances are the least important, the laws are of secondary importance, and the customs are most important (vol. 1, Chap. 17). The implication is that laws are subordinate to customs, that political phenomena are shaped primarily by social and cultural phenomena. One might conclude, then, that Tocqueville employs the regime analysis of classical political science but modifies it by making social and cultural factors more important than narrowly political ones in the formation of regimes (Ceaser 1985, 656).

Despite the modern terminology, this approach is not in itself a departure from classical regime analysis because the classical conception of the regime rests on a very broad notion of the political—one that includes the role of social classes and their characteristic customs, mores, culture, or ethos (1:310). Tocqueville departs from the classical approach not in giving primacy to customs but in separating laws from customs. By creating a distinction between the two phenomena, he does something classical political scientists would never do, namely, separate the legal or constitutional system from the regime or whole social order. As a result, Tocqueville makes statements that would sound strange to classical ears: "The political Constitution of the United States appears to me to be one of the forms of government that a democratic people could adopt; but I do not regard the American Constitution as the best, or as the only one, that a democratic people may establish" (1:246). What is unusual about this statement is that Tocqueville equates the constitution with the form of government and distinguishes it from the regime; he then asserts that a democratic regime or democratic people could adopt any one of several forms of government and still remain a democracy. It could create a "democratic republic," as he says

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the American people have done, or it could create a monarchical constitution, as he prefers for France (Tocqueville 1970, 200; Tocqueville 1985, 113–14).

Tocqueville's distinction between law and custom, between constitution and regime, is a significant departure from classical political science because it separates the political realm from the larger social ethos and relegates the political to a matter of secondary importance. Instead of the form of government shaping the social ethos, as the classical philosophers maintain, the social ethos shapes (although it does not completely determine) the form of government. If we search for the reason behind this departure from ancient constitutionalism, it may be found in the modern notion of *sovereignty*. What most impresses Tocqueville about modern democracy is that it resembles the regimes of the past in certain respects but ultimately is based on a new and different kind of authority: it is not merely a regime but a condition of sovereignty. The difference between a regime and a condition of sovereignty, as we shall see, lies in the degree and manner of control exercised by the ruling body.

Tocqueville explains this difference in Chapter four of *Democracy in America*, entitled, "The Principle of the Sovereignty of the People in America." There he asserts that the principle of popular sovereignty actually exists at the bottom of all human institutions, including despotic governments; for the support or acquiescence of the people is required of all established institutions. In the past, however, the sovereignty of the people was concealed or was acknowledged only in empty phrases such as the *will of the nation* or the *commonweal*. By contrast, Tocqueville claims, in modern democracies like the United States "the principle of the sovereignty of the people is neither barren nor concealed, as it is in other nations." This changes the nature of political authority: "In some countries a power ex-

ists which, though it is in a degree foreign to the social body, directs it, and forces it to pursue a certain track. . . . But nothing of the kind is to be seen in the United States; there society governs itself for itself. All power centers in its bosom, and scarcely an individual is to be met with who would venture to conceive, or, still less, to express the idea of seeking it elsewhere" (1:59). In traditional forms of political authority, the ruling group (such as an aristocracy or a clergy) stands outside and above the people, shaping and forming the people to its ends; the state molds society, creating a regime. In modern democratic societies, by contrast, the ruling group does not shape the people, the state does not mold society, but "society governs itself for itself." This kind of authority differs from earlier kinds because it is not based on a distinction between a ruling part and a ruled part; the two parts are identical. No need exists for one part to mold and shape the other (as in a regime) because there is no challenge or resistance. The state does not mold society but is absorbed by society and becomes subordinate to society. This is a condition of sovereignty because there exists an unchallenged, indisputable, and all-absorbing power that dominates without ruling because there is no conceivable alternative to its power.

Obviously, it is from this notion of sovereignty that Tocqueville draws his dire warnings about "the unlimited power of the majority in the United States" (vol. 1, Chap. 15). In expressing his fears about the tyrannical power of the majority, Tocqueville goes well beyond the criticisms of democracy leveled by the classical philosophers or by democratic theoreticians in modern times, like James Madison. Their warnings were focused on the political dangers of majority rule, such as the dispossession of the rich, the ostracism of the great, or the suppression of minority rights (Aristotle, *Politics* 3; *Federalist* no. 10). For Tocqueville (and,

following him, J. S. Mill), the tyranny of the majority is not only a political phenomenon but also an intellectual and moral phenomenon—an enslavement of the mind and a weakening of the will arising from the inability to conceive of any sort of authority except that of the people, or any standard of legitimacy except that of majority opinion. Even the harshest critic of democracy in the ancient world, Plato in the eighth book of *The Republic*, does not single out democracy for being more intellectually and morally despotic than other regimes; he contends that all regimes are restrictive and exclusive in the way they impose their notions of justice and honor on society. In fact, he sees more diversity and openness in democracy (which even tolerates the philosopher, up to a point) than in other regimes. What Tocqueville describes as the sovereignty of the people, the empire of the people, the irresistible pressure of popular taste and culture, is something that is not fully captured by the classical notion of the *regime*. With a sense of profound awe before a newly discovered mystery, Tocqueville says, "When I compare the Greek and Roman republics with these American states . . . When I remember all the attempts that are made to judge the modern republics by the aid of those of antiquity, and to infer what will happen in our time from what took place two thousand years ago, I am tempted to burn my books in order to apply none but novel ideas to so novel a condition of society" (1:327). With a sense of forboding, he also says, "I think, then, that the species of oppression by which democratic nations are menaced is unlike anything that ever existed before in the world" (2:336). The novel condition that Tocqueville witnesses and seeks to understand is the modern phenomenon of democratic sovereignty.

If we search for Tocqueville's explanation of the origin and cause of this new phenomenon, we cannot find it in his

discussion of popular sovereignty or majority tyranny in volume 1 of *Democracy in America*; for the first volume focuses on the laws, the legal or constitutional system as it is shaped by the principle of popular sovereignty. To understand not only the effects but also the cause of popular sovereignty, we must turn to volume 2, where the customs, or mores, of the people are analyzed; for the phenomenon of sovereignty is caused by a way of thinking, a way of conceiving of authority. From his discussion of democratic mores, we learn that attitudes toward authority in the United States are shaped by a mode of thinking and feeling that Tocqueville calls *individualism*. He argues that individualism is largely a product of the modern Enlightenment (as well as the Protestant Reformation), which taught modern men to question all authorities and to look for truth only in their own independent thinking—a mental habit, in other words, of skepticism and individual self-reliance (2:4-5). The effect of this mental habit is a startling and profound paradox in the way that modern democratic men conceive of authority; for it does not liberate the human mind in the way that the Enlightenment thinkers originally hoped and expected. According to Tocqueville, when most men are called upon to free themselves from higher authorities, they become frightened and confused and feel the insignificance of their own thoughts; instead of thinking for themselves, they turn to public opinion or the voice of the majority for guidance (2:11). The result is a paradoxical combination of individualism and conformity—an insistence on the equal right to think for one's self combined with a silent surrender of the mind and bending of the will to a new authority, the sovereign people.

To a certain extent, Tocqueville admits, this is only natural: "men will never cease to entertain some opinions on trust" without questioning or examining them;

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radical and perpetual doubt about first principles leads either to madness or chaos; dogmatic beliefs of some kind are necessary for life. But the new conformity underlying democratic individualism is actually more slavish and dogmatic than, for example, the religious authority of the past precisely because it appears on the surface to be intellectual freedom and is rarely acknowledged as a species of intellectual dogmatism. Its disguises make it difficult to recognize, for it imposes itself not by active assertion but by passivity, complacency, and casual acceptance—by the relaxation of the mind and will that occurs when the demands of higher authority are questioned. Tocqueville suggests that even those “minorities” who might be suspected of disloyalty to democracy, such as the wealthy, the clergy, and the literary or intellectual elite, have difficulty resisting the pressures of popular taste and culture (1:54, 312-20; 2:36-64). For the principle of popular sovereignty, or will of the majority or voice of “the public,” triumphs without argument “through a sort of enormous pressure of the mind of all upon the individual intelligence” (2:9-12).

In short, because modern democracy is founded not merely on the rule of the people but on the popular mind as it is filtered through the modern Enlightenment, the whole conception of authority is revolutionized. The sovereignty of the people comes into being, characterized by free-thinking individualism and an uncontested surrender to the will of the people. The result, according to Tocqueville, is a despotism of the mind and weakening of the character such as the world has never seen before, raising the specter of a new kind of democratic tyranny.

Following this analysis, we can say that Tocqueville's *constitutionalism* is a combination of old ideas and new categories adapted to fit the phenomenon of modern democracy (Richter 1969, 159). When Tocqueville observes the political and

social developments of his day, he thinks broadly in terms of regimes, primarily the contrast between aristocracy and democracy or between “the old regime” (to use Tocqueville's phrase for European feudalism) and the new democratic order. But, the regime analysis of ancient constitutionalism cannot fully grasp the new democratic order because the concept of the *regime* does not adequately describe the kind of domination and potential for despotism of modern *sovereignty*. Moreover, the classical concept of the regime does not account for the way that modern societies distinguish between the sovereign and the government, the customs and the laws, the society and the state, or the sovereign people and the *constitution* (in the narrow and limited sense in which the term henceforth is used). As Tocqueville says in the introduction to his work, “a new science of politics is needed for a new world,” and his adaption of classical regime analysis to fit the phenomena of democratic sovereignty and the limited constitution is the centerpiece of the new science.

Adapting the Regime of Virtue to Democratic Sovereignty

In presenting Tocqueville's modification of ancient constitutionalism, we have concentrated almost exclusively on its descriptive features, on its use as an analytical tool to describe the nature of power and authority. Yet, ancient constitutionalism is also, indeed it is primarily, a normative theory. It is essentially an exercise in political prudence. The classification of regimes and the ideal of a regime of virtue are intended to be general guides for the legislator, whose task is to choose the best possible political order in the given circumstances, the regime most conducive to the formation of virtuous citizens under the existing conditions.

In *Democracy in America*, Tocqueville

adopts the normative orientation of ancient constitutionalism. He accepts the traditional view that politics should be directed to the formation of virtuous characters through moral and civic education. But he recognizes that new techniques or a modification of old ones are necessary to achieve this goal, for it is no longer possible to create a regime of virtue by educating a ruling class and having it impose its morals from the top down, so to speak, through laws and institutions. Modern democracy prohibits such an "authoritarian" imposition of morals; it prohibits a regime of virtue in the strict sense because it rejects in principle the notion of higher authorities or the rule of moral superiors. In place of a regime imposed from the top, modern democracy creates a sovereign, the people, who in turn create a constitution, their state and form of government, whose purpose is to serve their needs and protect their rights. In these circumstances, the inculcation of virtue must follow a different strategy, one that is indirect and even hidden from view. Most importantly, as we shall see, it must speak the language of democracy, which means it must appear not as a form of virtue or moral superiority but as a form of liberty.

In one of his most explicit statements on first principles in *Democracy in America*, Tocqueville proclaims the highest ethical principle to be virtue and asserts that virtue should be the standard and guide for politics: "After the general idea of virtue, I know of no higher principle than that of right; or rather these two ideas are united in one. The idea of right is simply that of virtue introduced into the political world. It was the idea of right that enabled men to be independent without arrogance and to obey without servility. . . . There are no great men without virtue; and there are no great nations—it may almost be added, there would be no society—without respect for right" (1:254). Without actually defining *virtue* in this passage,

Tocqueville asserts that it is the highest ethical principle because it is the source of human greatness for individuals and nations. Virtue is the foundation of individual character, indeed, it is the perfection of human character. In the political world, it is the foundation of political right or notions of legitimate authority that enable men to obey authority freely, through voluntary self-restraint, without being tyrannical or servile. The type of virtue that Tocqueville has in mind here (and that he praises repeatedly in all his writings) appears to be a synthesis of civic and heroic virtue. Following Montesquieu and Rousseau, he defines *civic virtue* as republican virtue: it is patriotism or love of country over love of self, a sentiment that Tocqueville often associates with self-assertion on the one hand and mastery of the selfish passions on the other (1:97; Tocqueville 1955, 17; Schleifer 1980, 242-43). By *heroic virtue*, Tocqueville usually means great feats of courage and daring in war as well as chivalry and great acts of principled statesmanship—ideals he instinctively absorbed from his aristocratic heritage, from his reading of Plutarch, and perhaps from contemporary romantic notions of the Napoleonic hero (2:242-55; Tocqueville 1955, 118-20; Tocqueville 1985, 124-25; Boesche 1981). Whatever his sources of inspiration, Tocqueville holds civic and heroic virtue to be the highest ethical ideal because it produces noble characters, characters whose proud self-assertion and voluntary self-restraint make them true citizens and statesmen.

While appealing to the ancient ideal of virtue, Tocqueville recognizes the need to adapt it to the conditions of modern democracy. For democracy produces many forces that tend to undermine the assertive pride and self-restraint of a virtuous character. We have already seen how the pressure of popular sovereignty wears down the intellectual independence and strength of will of those who claim

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moral superiority to the majority. In addition, Tocqueville argues that democracy unleashes certain feelings or passions—an ethos of individualism and materialism—that endanger virtue. He sees *individualism* not only as a mental habit of questioning traditional authorities but also as a passion that results from the weakening of traditional social bonds; it is a peculiar kind of selfishness that combines egoism with a tendency to isolate one's self from others (2:102). *Materialism* is the love of physical well-being, especially the love of comforts and conveniences that make life softer and easier, a passion most characteristic of the middle classes (2:137). In the democratic age, these two passions are usually joined in an ethic of self-improvement, which drives individuals in a competitive and energetic quest for wealth and success (2:165). Tocqueville's view is that such passions are especially harmful to the moral character of citizens. For they tend to encourage withdrawal from public life by channeling all energy into business and commercial activities; ultimately they may destroy every form of individual initiative and restraint, turning the masses into passive sheep (2:149, 335-37).

Throughout his analysis of individualism and materialism, Tocqueville states explicitly that such tendencies are inimical to virtue: "Selfishness blights the germ of all virtue; individualism, at first, only saps the virtues of public life; but in the long run it attacks and destroys all others. . . . Selfishness is a vice as old as the world, which does not belong to one form of society more than to another; individualism is of democratic origin" (2:104). In a more sweeping statement about the effects of individualism and the love of material well-being, Tocqueville says that "the age of implicit self-sacrifice and instinctive virtues is already flitting far away from us" (2:132). Such statements make abundantly clear that the demise of virtue is Tocqueville's primary concern in

judging the moral character of modern democracy. In one of his notebooks, he states succinctly, "Egoism has replaced virtue"—that is the moral problem of democracy (Schleifer 1980, 241).

While making these judgments, Tocqueville recognizes that traditional civic and heroic virtues can never be resurrected in their pure and original forms. The task of the legislator, therefore, is to find substitutes for virtue compatible with democratic habits and beliefs. In surveying the U.S. scene, Tocqueville sees the greatest hope in the American understanding and practice of liberty. In analyzing this phenomenon, he presents an original and insightful interpretation that shows that American liberty assumes two distinct forms, political liberty and individual rights, and that both kinds of liberty can serve as democratic substitutes for virtue.

Tocqueville's account of political liberty appears at the beginning of *Democracy in America* and is boldly proclaimed as "the germ of all that is to follow and the key to almost the whole work" (1:29). He explains how the tradition of political liberty began in America with the settling of New England by the Puritans. In Tocqueville's eyes, the Puritans were remarkable because they combined strict religious beliefs—which prohibited everything from idleness and drunkenness to kissing in public and long hair—and political freedom. Their laws reflected the severity of Puritan morality; yet, "it must not be forgotten," Tocqueville says, "that these fantastic and oppressive laws were not imposed by authority, but that they were freely voted on by all persons interested in them" (1:40-41). By living according to strict laws that were voluntarily imposed, the Puritans were able to unite two things that other societies have found incompatible: the spirit of religion and the spirit of liberty. Through this unusual synthesis, the Puritans created the ideal of political liberty that permeates the whole of U.S.

society. In New England, it was embodied in townships and town meetings, where citizens participated directly in law making and administered local affairs. Spreading in all directions from its original source, the ideal of political liberty has "interpenetrated the whole American confederation and now constitutes the social theory of the United States" because it is the purest or most direct form of democratic self-government (1:32).

In tracing the origins of political liberty to the Puritan tradition of self-government, Tocqueville also uncovers its moral foundations. It rests, he says, on "a fine definition of liberty," a definition best expressed in a speech by Jonathan Winthrop, the early governor of Massachusetts. The gist of Winthrop's speech (as quoted by Tocqueville) is a distinction between two types of liberty. The first is *natural liberty*, which is the freedom to do what one wants and is no different from animal freedom. The second is *civil* or *moral liberty*, which is the freedom to do only what is good, just, and honest; it is a type of freedom that arises out of a covenant between humanity and God and must be exercised "in subjection to authority." According to Tocqueville, moral liberty is higher and more noble than natural liberty because it involves self-restraint, or mastery of the passions, and is bounded by moral and intellectual limits that prevent it from degenerating into license or chaos. Such liberty requires religion or some sense of sacred awe "before truths which it accepts without discussion" in order to provide a sense of limit and restraint (1:44-45). Such liberty, in turn, is the basis of democratic self-government; for it gives people the discipline and self-restraint to live by self-imposed laws, as well as the respect for authority and sense of civic duty to participate in public life. Thus, the moral liberty of the stern and pious Puritans is the foundation of political liberty or self-government in the United States.

The significance of this account is in providing a conception of liberty that closely resembles the classical notion of civic or republican virtue. The description of moral liberty provided by Jonathan Winthrop and warmly embraced by Tocqueville is closely akin to virtue because it is based on a hierarchy of the soul in which the higher or divine part rules over the lower or animal part in conformity with a higher law. It is also like virtue in that it requires people to subordinate their private interest to the public good. Moral liberty is different from classical republican virtue, however, because of its Puritan foundations. It arises from the Protestant idea of Christian liberty, which requires people to obey divine law from a free and willing choice rather than from sentiment, habituation, or the coercion of external authority. Moral liberty is self-willed obligation or obedience to a self-imposed law. Thus, it is properly classified as a species of freedom rather than of virtue, although the two are very similar.

The great question for Tocqueville is what becomes of moral liberty and the democratic self-government it sustains once the Puritan tradition grows weak and dies out. When this occurs, moral liberty is no longer a free and voluntary obedience to divine law but obedience to merely human law or perhaps to no law. Moral liberty then loses the inner discipline which distinguishes it from natural liberty, and democracy is left without restraints. In a sense, one could read volume 1 of *Democracy in America* as a moral melodrama, a kind of *Paradise Lost* in which democracy falls from the grace of the original Puritans to the despotic excesses of the tyrannical majority. As the story unfolds, about three-fourths of the way through volume 1, Tocqueville reveals that the old religious faith that once nurtured self-government in the United States lost its power and must be replaced by a new (but far less noble) moral principle: "Do you not see that

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religious belief is shaken and the divine notion of right is declining, that morality is debased and the notion of moral right is therefore fading away? Argument is substituted for faith, and calculation for the impulses of sentiment. If, in the midst of this general disruption, you do not succeed in connecting the notion of right with that of private interest, which is the only immutable point in the human heart, what means will you have of governing the world except by fear?" (1:255). Striking a note of pessimism, Tocqueville reveals the fate of the Puritan ideal. The old faith has lost its power because the advancing tide of modern skepticism has weakened the notion of divine right and replaced it with enlightened self-interest. In this age of skepticism, the only secure basis for political right is self-interest or private interest. A doctrine that builds upon self-interest and is capable of turning it into a moral principle—a principle of right that makes obedience to authority a matter of voluntary compliance rather than of coercion and fear—must be found.

At this point, Tocqueville turns, in a spirit of prudent compromise, to the doctrine of individual rights: "I am persuaded that the only means which we possess at the present time of inculcating the idea of right and of rendering it, as it were, palpable to the senses, is to endow all with the peaceful exercise of certain rights." The most important ones are property rights. For an individual may have "no notion of the property of others; but as he gradually learns the value of things and begins to perceive that he may in his turn be despoiled, he becomes more circumspect, and he ends by respecting those rights in others which he wishes to have respected in himself" (1:254). Tocqueville's claim is that in an age when divine right and traditional virtues are declining, the only effective moral principle is the doctrine of individual rights, especially property rights. This defense of

rights, of course, is purely prudential. For rights find powerful support in human selfishness and thus can be seen as nurturing moral vice or social instability. Yet, when properly understood as things that must be respected in others in order to be enjoyed by oneself, they turn self-interest into a principle of right. Through self-interest, rights lead people to self-restraint, respect for fellow citizens, and respect for authority. Rights thereby become another substitute for virtue, although a less satisfactory substitute than political or moral liberty.

The difficulty that remains for Tocqueville is to find a common moral basis for these distinct notions of liberty; for the two are not entirely compatible. Political liberty is a moral or theological conception derived from the Puritan tradition. It requires people to govern themselves in accordance with divine law; it frees them from the enslavement to their base and selfish passions by making them accountable to a higher law and teaches citizens to subordinate their private selves to the public good. While Tocqueville calls this a "fine," or noble, definition of liberty, he recognizes that its religious foundation will probably not survive in the enlightened and secular age of democracy (although he feels every effort must be made to perpetuate it as long as possible in the customs of Christian America [Kessler 1977]). By contrast, the second notion of liberty, individual rights, is perfectly at home in the enlightened and secular age but lacks a notion of moral restraint or accountability to a higher law; it encourages private selfishness and promotes restraint and respect for authority only through enlightened self-interest. Its foundation is more powerful and lasting but less noble.

To resolve this dilemma, Tocqueville looks to a phenomenon that is already present in American customs and political life, although imperfectly perceived and understood. That phenomenon is a sense

of moral responsibility—the belief that human freedom involves moral obligations because human beings are masters of their fate and must have the willingness and capacity to govern themselves. This notion of moral responsibility, Tocqueville suggests, is a secular counterpart or secular legacy of the old moral liberty of the Puritans (1:67, 94-96). Like the Puritan ideal, it requires self-government in the political and personal sense. It requires people to determine their destiny by participating in law making and freely submitting to their self-made laws; and it requires individuals to take control of their lives and be accountable for the consequences of their actions (Lively 1962, 221-28). However, it locates the ground of responsibility not in obedience to divine law but in human pride—in the pride and dignity of free people who seek to improve their condition in this world by mastering their fate, controlling their destiny, and developing the courage, intelligence, and discipline to govern themselves. Moral responsibility in this sense is a wholly secular notion that combines civic duty and self-interest, thereby bridging the gap between the two notions of liberty.

According to Tocqueville, the most admirable features of democratic liberty can be traced to the notion of moral responsibility. It inspires the cooperative endeavors of local government and civic associations, based on the principle of "self-interest rightly understood." Such actions are motivated by the desire of citizens to improve their condition and benefit themselves, while requiring them to develop some of the virtues of self-government (2:127-32). Moral responsibility also inspires the initiative, enterprise, and daring that drives commercial and technological progress; it engenders the pioneering spirit in people who boldly seize the future and make it work for themselves (1:260-61). The same moral

sense can be found in the patriotism and respect for law that is peculiar to modern democratic nations and is especially pronounced in the United States. Such patriotism is different from the sentimental attachment to birthplace and almost religious reverence for the sovereign found in the ancient monarchies of Europe. Democratic patriotism is neither sentimental nor instinctive but rational and reflective; it is the feeling of a citizen who believes the laws and prosperity of the nation are partly the citizens' own creations and are beneficial to them; it is the possessive pride felt by a citizen who consciously says, "This government is mine and works for me" (1:251). This "rational sentiment" also creates respect for the rule of law among democratic citizens because they recognize that laws are voluntarily self-imposed and may be changed if they are harmful (1:258). According to Tocqueville, all of these activities and attitudes arise from the belief that human beings are responsible for their fate and must have the willingness and capability to govern themselves.

In a grand summary statement, Tocqueville asserts that "the greatest advantages of democracy" may be traced to the belief in moral responsibility that underlies democratic liberty. It imparts to "the humblest individual . . . a certain degree of self-respect" and a desire to be "better informed and more active" and to improve his or her condition. It creates "an all-pervading and restless activity, a superabundant force and energy" in the whole society (1:260-61). From such statements, we may infer that Tocqueville praises the notion of moral responsibility because of its effect on the characters of free people: it produces the proud self-assertion and voluntary self-restraint of virtuous citizens. Moral responsibility, in short, raises democratic liberty to the rank of virtue.

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The American Constitution Correctly Understood

Using these insights about the problems of democratic sovereignty and the formation of virtuous characters, Tocqueville develops a distinctive interpretation of the U.S. Constitution. He recognizes that the modern democratic revolution has fundamentally altered the nature of political authority. It has created a distinction between the sovereign and the government or, more precisely, between the sovereign people and their political constitution, which drastically reduces the scope and power of the constitution. In modern democratic societies, the dominant force is the sovereignty of the people or the omnipotent majority; it acts like a *regime*, ordering and shaping the whole of society, while the *constitution* (in the narrower sense) is a limited and artificial device. A modern constitutional system, such as the one found in the United States, is a set of legal and institutional mechanisms designed to serve the needs and protect the rights of the people. As such, it is a kind of epiphenomenon that is largely shaped by the underlying forces of democratic sovereignty and the historical contingencies from which it arises.

In these circumstances, Tocqueville approaches the U.S. Constitution with two objects in mind: to show the debilitating effects of majority tyranny and to show the methods for promoting morally responsible liberty. As a student and admirer of *The Federalist*, he recognizes that the framers of the U.S. Constitution shared these concerns when they created a Republican constitution based on the principle of limited majority rule (1:143, 158-59). Since the founding, however, it has become evident that the constitutional mechanisms created by the U.S. framers—the whole “Madisonian” system of checks and balances—are insufficient to stop, and may even encourage, the pressure of democratic sovereignty and

majority tyranny. Indeed, Tocqueville warns that the U.S. constitutional system is in danger of being completely absorbed by the social forces of the majority, threatening not only the individual rights it is designed to protect but also the intellectual and moral character of citizens (1:60).

Writing in the 1830s (one generation since the founding), Tocqueville observes that the U.S. constitutional system suffers from a decline in the moral quality of leaders and citizens, reflected in a general weakening of character and a relaxation of moral restraints (1:143). He also notes many specific abuses of power. One is the tendency of the legislative branch to dominate all others, an expression of majority tyranny that the framers sought to, but failed to, overcome: “The Americans have not been able to counteract the tendency which legislative assemblies have to get possession of the government” (1:126). Another problem is the extreme weakness of the executive. Tocqueville calls it “an inferior and dependent power” compared to Congress and describes the process of electing and reelecting presidents as “a national crisis” or “a sort of revolution,” which stops the wheels of government and turns the executive into “an easy tool in the hands of the majority” (1:142-43). All of these problems can be seen in the behavior of President Andrew Jackson; Tocqueville views him as a populist demagogue, “a slave of the majority,” who attacks the powers of the federal government and thereby contributes to both the despotism of the majority and the fragmentation of the union (1:431-32). This points to a third problem, the weakness of the federal system itself. While it possesses the virtue of combining the power and glory of a great nation with the liberty and well-being of small communities, it has an inherent weakness: the government of the union is an artificial creation, “a legal fiction,” and therefore commands less

authority and affection than state and local governments; the dissolution of the union, Tocqueville warns, is more than a remote possibility (1:172, 432). Thus, the existing constitutional system in the United States is deficient because it fails to restrain the majority and to contain the centrifugal forces of competing interests. It suffers, one might say, from both the tyranny and the anarchy of the majority.

The reason for these deficiencies, Tocqueville suggests, is that the framers provided legal and mechanical remedies, such as divisions of power or countervailing forces, for what is essentially a moral problem: the tyranny of the majority is ultimately due to a loss of virtue. In its fullest sense, it is a weakening of intellectual and moral character by the whole ethos of modern democracy; it creates despotic tendencies because the people lose the willingness to take responsibility for their lives and the capacity to govern themselves. One way that Tocqueville responds to this problem in *Democracy in America* is by emphasizing the Puritan origins of the U.S. constitutional system (at the beginning of volume 1) and by continuously stressing the primacy of mores to laws. His second solution, which focuses on the laws themselves, is to combine the wisdom of the Puritans with that of the framers in a vision of the constitutional system that stresses the moral education of citizens rather than the mere division of power through institutional mechanisms or checks and balances.

In a passage reflecting both the limitations and the dignity of laws in the democratic age, Tocqueville says, "Laws cannot rekindle an extinguished faith, but men may be interested by the laws in the fate of their country. It depends upon the laws to awaken and direct the vague impulses of patriotism, which never abandons the human heart; and if it be connected with the thoughts, the passions, and the daily habits of life, it may be con-

solidated into a durable and rational sentiment" (1:97). The function of laws, Tocqueville suggests, is to fill the void left by the decay of religion with the inculcation of patriotism. With its echoes of modern nationalism or even totalitarianism, this statement might sound "illiberal" or dangerous. In the context of *Democracy in America*, however, the rational sentiment of patriotism to which Tocqueville alludes is identical to the morally responsible liberty, or genuine self-government, that Tocqueville defends as the modern form of civic virtue; for it requires citizens to take an "interest in the fate of their country," that is, to possess the assertive pride that inspires people to control their destiny and master their fate. At the same time, it requires them to obey their self-imposed laws and to exercise self-restraint. Next to religion, Tocqueville argues, such patriotism is the only force that can voluntarily unite people's wills without creating new forms of despotism (1:97).

In showing how the laws can serve as instruments of moral education, Tocqueville reinterprets and in some cases modifies the constitutional devices created by the framers, such as federalism, judicial review, and the separation of powers. The thrust of his proposals is at first glance contradictory: he recommends greater decentralization of the American federal system as well as greater concentration of power on the national level. The consistency of these proposals, as we shall see, lies in Tocqueville's insight that morally responsible liberty requires the laws to empower the majority by direct involvement at the local level while restraining the majority by independent authority on the national level.

Tocqueville's praise of local government and a decentralized federal system is undoubtedly the most familiar feature of his constitutional analysis. Yet, as Schleifer points out, Tocqueville's sup-

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port for decentralization is actually rather strange; for the exercise of local government would seem to provide new opportunities for majority tyranny and to support local tyranny rather than local liberty (1980, 212ff). Schleifer's inability to resolve this difficulty is due to insufficient clarity about Tocqueville's notions of majority tyranny and responsible liberty. Tocqueville argues that local government gives even the humblest citizen a chance to hold office or to participate in decision making; without this experience, he says, people become "indifferent to their fate" and "sacrifice their free will," that is, they lose the sense of being masters of their fate (1:96). Through direct participation, they realize that "men must walk in freedom and be responsible for their acts, or remain passive spectators . . . in schemes with which they are unacquainted" (1:94). Thus, decentralization may loosen external controls on local majorities; but it mitigates majority tyranny because the essence of such tyranny is not mere conformity but conformity through weakness of will—through passivity, complacency, mediocrity, and resentment of superiority in any form. By contrast, the sense that human beings can control their own destiny through political participation encourages assertive pride and energetic activism on the one hand and obedience to self-imposed laws and voluntary self-restraint on the other (1:98). Such conformity is not tyranny but morally responsible liberty, the liberty of people who are masters of their fate and who govern themselves through laws freely determined and freely obeyed.

Another puzzle in Tocqueville's praise of decentralization is his claim that local government contributes to patriotic unity rather than to political fragmentation. The solution lies in his understanding of democratic patriotism: it springs not from a sentimental attachment to one's birthplace but from a rational attachment to laws and government; it is the feeling of

someone who consciously says, "This government is mine and works for me." Obviously, such rational patriotism arises most naturally from small communities where citizens have the keenest sense that government is their own and contributes to their prosperity. But the attachment to an abstract political entity rather than to a particular plot of land makes it possible for "every citizen of the United States [to] transfer, so to speak, his attachment to his little republic into the common store of American patriotism" (1:170). Thus, a decentralized federal system helps to mitigate the anarchy as well as the tyranny of the majority because the feeling of possessing or being a part of the government fosters a patriotic pride in the nation as whole.

This analysis of decentralization involves a subtle but important shift from the founders' view of federalism because "decentralization is equated with local government and opposed to state and even county government" (Winthrop 1976, 95). Indeed, Tocqueville downplays the states and denigrates the notion of states rights. He regards most of the state constitutions as defective—the terms of office are too short, the legislative bodies dominate the executive, "the despotism of the majority prevails"—and he views the states as the main source of fragmentation and disunity in the nation (1:160, 431). Tocqueville's intention, therefore, is to weaken the states while strengthening local government and the union as a whole.

Another institution that empowers local majorities by requiring direct participation is the trial by jury. Although Tocqueville clearly recognizes this institution as a form of popular sovereignty, he praises it for two reasons. It is a mechanism of a modern constitutional system that acts like a regime, directly imposing the norms of the ruling body on the ruled. The presiding judge is a moral educator, imparting the majesty of the law and legal

notions of equity and proper procedure to ordinary citizens. In addition, the citizens themselves make decisions with fateful consequences for the accused, which provides a lesson in moral responsibility. In Tocqueville's words, "the jury teaches every man not to recoil before the responsibility of his own actions and impresses him with that manly confidence without which no political virtue can exist. It invests each citizen with a kind of magistracy; it makes them all feel the duties which they are bound to discharge towards society. . . . By obliging men to turn their attention to affairs other than their own, it rubs off that private selfishness which is the rust of society" (1:295). This passage is a clear expression of Tocqueville's view that political participation in making decisions ennoble the character of citizens by engendering "confidence" and requiring transcendence of "private selfishness"—an exercise in democratic liberty that shines like political virtue because it gives citizens the pride to take responsibility for their acts and the self-discipline to step outside the narrow circle of their personal lives.

While Tocqueville's praise of local government and jury duty are well known, his calls for greater centralization and concentration of power in the United States are often overlooked—lost, perhaps, in his warnings about the possibilities for new forms of "Caesarism" in volume 1 (p. 341) or the "soft despotism" of a centralized bureaucratic state in volume 2 (p. 335ff). Yet Tocqueville clearly favors greater centralization, of the proper kind, for the United States. His goal is to empower the majority at the local level while seeking greater independence from majority will on the national level by strengthening the federal judiciary and concentrating more power in the executive branch.

These recommendations involve a subtle reinterpretation of the doctrine of separated powers. Most importantly,

Tocqueville does not see the separation of powers as a balancing of three roughly co-equal branches; rather, he gives primacy to the judiciary. He praises the U.S. framers for creating a powerful federal judiciary, especially by granting judges the power to declare laws unconstitutional (judicial review). This power, Tocqueville points out, is a unique feature of U.S. constitutionalism, not to be found in the nations of Europe where judges only decide matters of fact or interpret the law when it is contested in a specific case. The limitations on judicial power in European nations result from their conceptions of the constitution as either an immutable entity beyond the power of judges to alter (the French view) or one that is changeable by the ordinary legislative powers (the English view). Compared to the French and English views, Tocqueville claims, "The political theories of America are more simple and rational. An American constitution is not supposed to be immutable, as in France; nor is it susceptible of modification by the ordinary [legislative] powers of society, as in England. It constitutes a detached whole, which, as it represents the will of the whole people, is no less binding on the legislator than on the private citizens, but which may be altered by the will of the people in predetermined cases according to established rules. In America, the Constitution may vary; but as long as it exists, it is the origin of all authority, and the sole vehicle of the predominating force" (1:105). The U.S. Constitution, in Tocqueville's telling phrase, is a "detached whole"—a fundamental law that stands outside and above the government and the people. Hence, there are two orders of law in the United States, the constitutional and the legislative. The constitutional law is the more fundamental of the two and functions like a "higher law": it cannot be modified by a legislative act and is the standard to which statute or positive law must conform (Pierson 1938,

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603-4). Yet, the Constitution is not really a higher law in the traditional sense. Unlike divine or natural law it does not exist independently of the human will; it is created by the will of the people and may be altered or amended by the people according to preestablished rules. However, as long as it exists, it is absolutely binding on the people and their representatives; the duty of judges is to remind people of this obligation by striking down (or refusing to enforce) laws contrary to the Constitution (1:106). In the process, the people learn the most important lesson of moral responsibility: that they are free to create a fundamental law but must live within the boundaries of a self-created authority. Understood in this way, judicial review functions not only as a check on legislative despotism but as an instrument of moral education.

In addition to teaching citizens and leaders to live responsibly, the judiciary serves an important political purpose. This political purpose, however, is quite different from the contemporary phenomenon of judicial activism—the creation of law or policy by the courts through a flexible interpretation of the Constitution. Tocqueville states that a U.S. judge preserves his moral authority because he does not “attack the legislator openly and directly . . . [but] is brought into the political arena independently of his will” by deciding a case brought by a third party (1:107). He even warns explicitly against judicial activism by saying that “an essential quality of judicial power is never to volunteer its assistance to the oppressed, but always to be at the disposal of the humblest who solicit it” (2:343). The political role of the courts is very different from pronouncing abstract ideals or initiating social change. As Tocqueville says, although the activity of the Supreme Court is “essentially judicial, its prerogatives are almost solely political; its sole object is to enforce the execution of the laws of the Union.” He contends

that without the federal judges “the Constitution would be a dead letter: the executive appeals to them for assistance against encroachments of the legislative power; the legislature demands their protection against the assaults of the executive; they defend the Union from the disobedience of the states, the states from the exaggerated claims of the Union, the public interest against private interests, and the conservative spirit of stability against the fickleness of democracy. Their power is enormous, but it is the power of public opinion” (1:157). The political role assigned to the federal judiciary is the conservative one of preserving the unity and wholeness of the union against the jarring and clashing of competing powers that have been built into the American constitutional system.

In this statement one can discern a simple but powerful criticism of the entire Madisonian conception of checks and balances. When power is divided into many centers—when the three branches of government vie with each other, when the several states compete against each other and against the federal government, when diverse interest groups compete and conflict—what remains to hold the system together? Tocqueville's answer is “public opinion,” but it is the opinion of the public as shaped by the judiciary in “enforcing” the Constitution, the humanly created system of higher law.

In order to be effective, Tocqueville adds, the justices of the Supreme Court must rise to the level of impartial statesmen, exercising a special kind of political prudence: “Not only must the Federal Judges be good citizens and men of information and integrity, which are indispensable to all magistrates, but they must be statesmen, wise to discern the signs of the times, not afraid to brave the obstacles that can be subdued, nor slow to turn away from a current when it threatens to sweep them off . . . [for] if the Supreme Court is ever composed of

imprudent or bad men, the Union may be plunged into anarchy or civil war" (1:157). In the final analysis, then, it is not the separation of powers and its checks and balances that make the constitutional system work. It is a single branch, the federal judiciary, that binds the system together into a whole with the power of public opinion and moderates the clash of competing powers and interests. In a curious way, Tocqueville sees the judiciary as the ultimate "executive" of the laws: its primary task is the enforcement of law and its peculiar skill is to enforce the law through the exercise of moral authority—by prudently guiding public opinion. This vision of judicial power as an enforcement power (which resembles Tocqueville's description of the "justices of the peace" on the county level [1:77]) is more political than the view of the founders and less political than contemporary ideas of judicial activism or administrative law. For it gives the federal judiciary the political mission of preserving the union but does not challenge judges to initiate social change by expanding rights or to enforce laws through creative rule making. On the contrary, Tocqueville's appeal is to a kind of judicial statesmanship whose task is to teach all parties to live responsibly within the limits of their self-imposed law, the Constitution—a lesson in moral restraint.

Tocqueville makes a similar though less conclusive argument in recommending a stronger executive. His primary purpose is to expose the weakness of the U.S. president by comparing him to the constitutional king of France. The latter is a more powerful executive for two reasons. First, in addition to executing the laws, the king plays a dominant role in law making by initiating legislation, exercising an absolute veto, and appointing members in one chamber and dissolving the other, at will. Second, he holds his office by hereditary title and thus does not need to be reelected. Even under such a

powerful executive, the government of France is limited by public opinion and is thus "essentially republican" (1:129). In making this comparison, however, Tocqueville does not indicate precisely how far the United States should or could go in imitating it.

One obvious lesson is that the U.S. president should be allowed to acquire more legislative power, permitting a greater consolidation of powers in the executive (1:127-41). A second seems to be that the presidency, although it could never be transformed into a hereditary office, might be strengthened by extending its tenure beyond four years and eliminating reeligibility (as some presidential reformers of today suggest in supporting a single six- or eight-year term). Such a modification follows from Tocqueville's remark that the founders conceived of the executive as a check on legislative despotism, but "by introducing the principle of re-election they partly destroyed their work." Contrary to their intention, they created a system that "stops the wheels of government" every four years, substitutes "management and intrigue for patriotism," and increases the power of the majority (1:142-43). Thus, Tocqueville's ideal American executive is virtually a constitutional monarch, a kind of "patriot king" (Ketcham 1984), who does not merely check the power of Congress but replaces it as the primary law-making entity.

In addition to modifying the separation of powers, Tocqueville seeks to counteract the Madisonian principle of dividing the majority into a multiplicity of interests. He explains the defect of this principle in a passage that reads like a direct response to *Federalist* no. 10: "All the passions that are most fatal to republican institutions increase with an increasing territory, while the virtues that favor them do not augment in the same proportion. The ambition of private citizens increases with the power of the state; the strength

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of parties with the importance of the ends they have in view; but the love of country, which ought to check these destructive agencies, is not stronger in a large rather than a small republic. . . . [For] the inclinations and interests of the people are more diversified by the increase in population, and the difficulty of forming a compact majority is constantly augmented" (1:167). Tocqueville here affirms Madison's premise that extending the size of a republic multiplies the number of competing interests, thereby decreasing the possibility of a compact majority. But in contrast to Madison he regards this as a bad tendency because it weakens the patriotism or love of country that should restrain the ambition of private individuals and political parties (Schleifer 1980, 112-20). Ironically, then, Tocqueville seeks to strengthen the majority by making it more cohesive or compact. But he does so in order to create an internal restraint, the sentiment of patriotism, rather than to rely on the external control of interest checking interest. What Tocqueville seems to be groping for here is not only a more cohesive union but a notion of the *nation* or the *national will*, recognizing that genuine self-government requires a sense of national responsibility rather than the fragmentation and haphazard play of competing interests.

In historical terms, Tocqueville's view of the American constitutional system stands in opposition to the prevailing ideas of his time—the ideas of Jacksonian democracy featuring states rights and sectional divisions, unlimited territorial expansion, the weakening of the national government through executive veto and contempt for the judiciary, and the glorification of the common man. As an alternative, Tocqueville seeks a combination of the best elements of the anti-Federalist and Federalist traditions (Ceaser 1985, 661)—a view that encourages the virtues of local self-government and strong national leadership in a moderately sized

and cohesive union. This vision, in Tocqueville's judgment, is the best way of transforming the rather weak system of checks and balances in the United States into something that resembles a regime, a coherent moral order that teaches citizens responsible liberty and the discipline of self-government.

Conclusion

Tocqueville's constitutionalism is a novel synthesis of old and new ideas, a prudent adaption of ancient constitutional principles to the conditions of modern democracy. It shows how the classical conception of a *regime of virtue* can be used to critically analyze the effects of democratic sovereignty, as well as to correctly understand and modify the American system of checks and balances. At the foundation of this theory is the insight that democratic citizens who exercise liberty with a sense of moral responsibility attain a nobility of character that shines with the brightness of virtue.

In the last analysis, however, Tocqueville recognizes that moral responsibility is something of an illusion. It rests on the belief that people are the masters of their fate and can find within themselves the courage and discipline to govern themselves by laws of their own making. Yet, as Tocqueville asks in a poignant question, "What can be done with a people who are their own masters if they are not submissive to the Deity?" (1:318). By raising this question, Tocqueville indicates that even the noblest conception of human freedom cannot be limitless. It requires the mind to "bow with respect before truths which it accepts without question"; it requires limits imposed by divine law.

Indeed, Tocqueville ultimately appeals to a "higher law" pointing to a notion of moral perfection beyond human freedom, to an end given by nature or ordained by God. While this "higher law" is loosely

mentioned several times in *Democracy in America* (1:269, 288, 434), it is developed systematically only once, in a remote chapter of volume 2 entitled "Of Honor in the United States." There, Tocqueville claims that morality in most times and places is governed by what the dominant classes honor and dishonor, praise and blame, admire and disdain. Although these codes are almost entirely conventional, people usually accept them without question or hesitation. Yet even while they obey, Tocqueville says, "they feel by a dim but mighty instinct, the existence of a more general, more ancient, and more holy law" (2:242). This intimation of a higher law is "the natural order of conscience" or "the general necessities of mankind revealed by conscience to every man." It stands above all conventional laws and valuations and points to the realm of "mere virtue, which lives upon itself, contented with its own approval" (2:244-55).

This intimation of a higher law, which appears to be a residual form of Christian natural law, is the ultimate foundation of Tocqueville's constitutional theory. It points to a standard above the will of the majority as well as the humanly created but morally binding modern constitution. By grounding human virtue in the divine and natural order, it provides the moral compass for all Tocqueville's prudential judgments in the new world of modern democracy. We thus conclude that Tocqueville's constitutionalism is a grand synthesis of classical regime analysis, modern constitutionalism, and Christian natural law, intricately woven into an original and coherent whole.

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NETWORKS IN CONTEXT: THE SOCIAL FLOW OF POLITICAL INFORMATION

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We examine the effects of individual political preferences and the distribution of such preferences on the social transmission of political information. Our data base combines a 1984 election survey of citizens in South Bend, Indiana with a subsequent survey of people with whom these citizens discuss politics. Several findings emerge from the effort. First, individuals do purposefully construct informational networks corresponding to their own political preferences, and they also selectively misperceive socially supplied political information. More important, both of these individual-level processes are shown to be conditioned by constraints imposed due to the distribution of political preferences in the social context. Thus, individual control over socially supplied political information is partial and incomplete. Finally, these information-transmitting processes interact with the social context in a manner that favors partisan majorities while undermining political minorities.

Politics is a social activity imbedded within structured patterns of social interaction. Political information is conveyed not only through speeches and media reports but also through a variety of informal social mechanisms—political discussions on the job or on the street, campaign buttons on a friend's shirt, even casual remarks. Such political information is processed and integrated not by isolated individuals but rather by interdependent individuals who conduct their day-to-day activities in socially structured ways and who send and receive distinctive interpretations of political events in a repetitive process of social interaction. Thus, political behavior may be understood in terms of individuals tied together by, and located within, networks, groups, and other social formations that largely determine

their opportunities for the exchange of meaningful political information (Eulau 1986). In short, *environment* plays a crucial role in affecting the social flow of political information.

People choose their friends and the content of their conversations, but each of these choices is, in turn, bounded by an environment that, for many purposes, must be taken as given. We explore the ramifications of these two-way streets between individually determined friends and discussion content on the one hand and the socially structured supplies of possible friends and conversation content on the other. Hence our focus is on the content and perception of political information and the influence of the social context within which such information is exchanged. We turn initially to an examination of individual control over the

social flow of political information before considering the role of the environment in limiting such control.

Individual Control Over Information

Political information is conveyed through social interaction, but does political preference determine the supply of information, or does the supply of information determine political preference? The direction of influence undoubtedly runs both ways. People choose information sources subject to their own preferences, but they also take what is available. They consume what social structure and social situations supply albeit guided in acceptance by individual perceptual mechanisms (Jones 1986). Thus, several different individual-level processes might account for the high levels of political homogeneity so frequently reported in studies of associated individuals.

High levels of correspondence between individual preference and the surrounding preferences of associated individuals might arise from discretion in the choice and use of associates. Individuals certainly choose to associate with some individuals and to avoid others, and they talk politics with some associates but avoid discussions with others. To the extent that these choices are motivated by a priori political preference, a high level of homogeneity in the politics of associates is the consequence of preference rather than its cause.

Indeed, the dominance or precedence of political preference over social influence corresponds well with a choice-theoretic perspective on political behavior. In his *Economic Theory of Democracy*, Anthony Downs argues (1957, 229) that it is fully rational for individuals to reduce information costs by obtaining political information from personal contacts. Among the benefits of socially obtained

information is the ability of the receiver to choose the information source and thus to select a set of biases and viewpoints on the part of the source that the receiver finds congenial: "It is often relatively easy," writes Downs, "for a man to find someone he knows who has selection principles like his own."

A second source of reported political homogeneity within informational networks arises from measurement procedures. The level of correspondence between a respondent and associates is often determined on the basis of the respondent's own report. A single respondent provides information not only on his or her own political preferences but also on the political preferences of associates. Thus, if individuals selectively screen the political information they receive through social interaction, they may also systematically misperceive its content (Lauman 1973). Political homogeneity produced in this fashion is, from a purely technical perspective, a measurement artifact. Notice, however, that this is a measurement artifact with significant theoretical consequences. It indicates that disagreeable interactions can be reinterpreted (misperceived) in an agreeable light. Even if individuals are unable to control the objective content of socially transmitted political information, they are still able to control the interpretation of that content in a manner that reduces political dissonance, and thereby minimizes the impact of social influence.

Finally, additional subtleties may operate as well. An individual who is well aware of an associate's political preferences and also knows them to be at variance with his or her own beliefs may very well misrepresent, not fully communicate, or deliberately make ambiguous his or her own preferences precisely in order to reduce dissonance in the relationship (Macoby, Matthews, and Morton 1954). Hence, a discussion partner may be quite prepared to misperceive political informa-

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tion when, simultaneously, the transmitter is prepared to obfuscate the message (Jones 1986). It is not only political elites or candidates for public office (Shepsle 1972) who may have motives to heighten communication ambiguity in democratic politics (Calvert 1985; MacKuen n.d.).

In summary, three individual-level factors undermine the potential impact of socially transmitted information. Citizens choose with whom to discuss politics, they reinterpret dissonance-producing information, and they may deliberately misrepresent their true opinions. These individual-level capabilities bring us full circle. Political information is conveyed through social interaction, but individuals choose with whom to interact, and they also suppress dissonance-producing information if it is not already rendered politically inoffensive by the transmitter. On these premises, socially transmitted political information becomes, wholly and completely, an extension of individually based political preference. The potential influence of socially transmitted information is explained away on the basis of choice, misperception, and misrepresentation: sociology gives way to economics and individual psychology.

This position may be juxtaposed with a view that makes a case for the significance of social structure in determining the social transmission of political information. Not only is it worthwhile to inquire into the structural factors that motivate people to conform or to misrepresent their positions, but one might also examine the manner in which social structure determines interaction opportunities or even imposes interactions.

Social Control Over Information

A case for social influence might be made on either theoretical or empirical

grounds. Anticipating empirical support for social influence we ask, Is there a theoretical basis from which to build an expectation for the influence of socially transmitted information?

First, to argue that the selection of information sources takes precedence over social influence is to assert that political preferences dominate social-influence processes running in the other direction. It is a gross simplification to assume that individually defined interests translate directly into individual preferences (Lewis-Beck 1986, 342-44), for preferences must be informed, and information arrives through socially structured channels. In particular, as McPhee (1963) persuasively argues, political preferences are sustained within particular structural locations, and they are configured by the multiple interactions of the individual within those structural locations. Thus, to the extent that political preferences are dependent on (socially supplied) information, they are inherently dynamic and responsive to social influence. Preference is not only inflicted upon socially transmitted information, but socially transmitted information is inflicted upon preference. Mutual causation between social contexts and individual preferences is inherent in this view of mass political-information processing.

Second, it is often difficult for political scientists to remember that politics is not at the forefront of most choices that most individuals make. Assuming for the moment that individuals do indeed choose their own locations within the social structure, the choices are likely to be predicated on matters other than politics and political preference (Brown 1981). People choose a job because it pays better or because they need work. People choose a neighborhood because they can afford one of the houses. People choose a church because their parents raised them in it. The point to be emphasized is that politics and political preferences are ancillary to

most of the significant life choices people make, the choices that locate individuals within the social structure. But these choices, made largely on nonpolitical grounds, may nevertheless impinge mightily on politics. Particular locations within the social structure expose citizens to particular social contexts and the information biases such contexts willy-nilly provide. These social contexts circumscribe the opportunities for social interaction. Thus, individual choice regarding associational patterns and political discussions may be seen as operating within the opportunities and constraints imposed by a given social context.

These processes are stochastic. It is possible to buy into an upper-class neighborhood and find oneself surrounded by Democrats, but a preponderance of Republican neighbors is more likely. Individuals probably reason over these bundles of probabilities in a similar fashion: If I buy into this neighborhood, the chances are my daughter will date good Catholic boys (and ultimately marry one), the reasoning might run. But recognizing the contingent nature of the processes of living does not make the (stochastically determined) impact of events any less. Indeed, events that are not certain are more effective conditioners than those that occur regularly, as Skinner (1938) demonstrated long ago.

This is not to say that individuals are powerless in deciding with whom to discuss politics. In her study of a predominantly Democratic automobile plant, Finifter (1974) demonstrated that Republican autoworkers became socially introverted, resisting association with the numerically superior Democrats. In his study of a working-class suburb, Berger (1960) demonstrated that the middle-class minority was excluded and withdrew from community life. It is not that choice is absent, but rather that associational choice is contingent—a locationally specific response to a particular social mix.

The role of the mass media may be deceptive in regard to these social processes. The nationalization of political-information sources achieved through the disappearance of multiple-newspaper cities and the dominance of television may very well enhance the coercive power of informationally biased social contexts. If individuals in one-newspaper and three-look-alike-network cities are to hear second opinions, the only recourse is probably socially transmitted new information. But this will be biased by its social location, that is, by the social context. Hence, readily available political information from the modern mass media may, because of its increased uniformity and homogeneity, increase rather than decrease the political influence potential of the social context (Laurily K. Epstein, personal communication, 1986).

Evidence will help in assessing the substantive significance of the various processes we have sketched. We turn now to empirical matters with some analyses taken from a study with a research design intended to cast light on the sorts of issues just raised.

Networks and Contexts in South Bend

As a first step, it is crucial to draw an important, if abstract, theoretical distinction between *contexts* and *networks*: contexts are structurally imposed while networks are individually constructed. Contexts are external to the individual even if the composition of the context depends upon the makeup of individuals contained within it. In contrast, networks represent the product of myriad choices made by people who compose the net, but choices that are circumscribed by the opportunities and constraints imposed by context. To present an extreme example, a Mondale voter who resides in a context where all other individuals support Reagan will be unable to discuss politics

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with another Mondale voter. As a practical matter, this distinction between networks and contexts may blur, but it provides a useful analytic tool with which to consider the issues raised above.

The analyses are based on a study undertaken in South Bend, Indiana during the 1984 presidential election. The South Bend study was designed with a particular motive: to locate mass political behavior—political attitudes, candidate preferences, party preferences, policy positions, and political activities—within the social patterns of everyday life. Two structural ingredients are especially germane here: the informational networks through which individuals send and receive political information and the larger but still very immediate contexts within which individuals reside. Both networks and contexts were observed and measured as part of the South Bend study, and clarity about measurement procedures is important for understanding our results.

A three-wave panel survey of approximately 15 hundred respondents was conducted during the summer and fall of 1984 before, during, and after the presidential election. The respondents lived in 16 different South Bend-area neighborhoods purposely selected to maximize social homogeneity within the neighborhoods and social heterogeneity between the neighborhoods. These neighborhoods constituted the primary sampling units, and a high rate of random sampling was achieved in each neighborhood. The neighborhoods vary from extremely affluent planned suburban developments through respectable middle-class neighborhoods, through well-maintained working-class and ethnic enclaves, to poor and deteriorating central-city neighborhoods. All the neighborhoods are composed of predominantly white populations, and the small number of black respondents are excluded from the analyses reported below. (More detailed

information regarding study design, response rates, and related issues is available upon request from the authors.)

Approximately equal numbers of respondents, in turn, were randomly selected within each of the neighborhoods and interviewed through the computer-assisted telephone interviewing facility of the Indiana University Center for Survey Research. Intensive sampling within neighborhoods produces two distinct advantages. First, any survey item can be aggregated to produce a measure of neighborhood context; thus, we can construct survey-derived estimates of neighborhood support for Reagan in the 1984 election. Second, intensive sampling makes it possible to employ procedures developed by Boyd and Iversen (1979) for the analysis of contextual data; we employ a variation of the Boyd and Iversen centering procedure to produce approximately orthogonal explanatory variables.

Contexts are measured at the level of the neighborhood, using an aggregated survey item to provide the contextual measure, and the relevant individual-contextual property is voting behavior in the 1984 election. Thus the context of political behavior, as distinguished from the network of political discussion, is defined geographically and socially on a priori grounds and measured directly from survey respondents. While our measure of context is the geographically based neighborhood, the social context is clearly a much broader concept that encompasses wider domains, such as religious affiliation, recreational activities, and social organizations. Thus the measurement employed here is narrower (and more conservative) than the theoretical construct it represents. Indeed, more comprehensive measures of social contextual experience would likely demonstrate even larger effects than those reported in our tables.

Networks are measured from two vantage points: first, that of the main respon-

dent to the survey; second, that of the political discussant named by the main respondent. Respondents to the third-wave survey, conducted shortly after the 1984 election, were asked to give the first names of three people with whom they were most likely to have discussions about politics. If respondents could not name anyone with whom they discussed politics, a follow-up question asked for the first names of three people with whom they were likely to have informal conversations. The vast majority of the respondents were able to identify political discussants. The nature of the probe is very important because it was intentionally structured around a clear political reference point. We did not ask our main respondents to give us a list of their best friends or their close associates. Rather, we asked them to tell us with whom they discussed politics—their social sources of political information. Not surprisingly, many of the main respondents named discussants who were spouses and other relatives. However, for the purposes of this paper, all relatives by either blood or marriage are omitted; our focus is entirely upon unrelated discussants.

Notice that our definitions of *contexts* and *networks* do not require that the two overlap territorially. That is, a network may lie wholly outside the context, but this fact would not negate interdependence between these two elements of social structure. A person who lives in a context surrounded by Reagan voters may assume that everyone votes for Reagan, even when encountering individuals outside that context. Furthermore, a person imbedded in a Reagan context probably has a higher likelihood of encountering Reagan voters even when the encounter occurs beyond the context. Contexts create opportunities for social interaction which are not necessarily bounded by the original context.

A series of follow-up questions asked main respondents for information and

perceptions regarding the discussants, including the presidential candidate for whom the discussant voted in 1984. A final question asked for the last name and street address of each discussant so that we could ask the discussants "a few short questions regarding the last election." The main respondents, perhaps surprisingly, were quite forthcoming with this information. Based upon their responses, the staff at the Center for Survey Research constructed a sampling frame of approximately 18 hundred discussants, and our interviewing budget allowed us to conduct a fourth interview with more than nine hundred discussants. In addition to these interviews, fifty-five main respondents reported a discussant who had also been interviewed in the third wave. This was not totally unexpected since the neighborhoods were small in size with a great deal of social integrity, and the sampling rate within the neighborhoods was accordingly relatively high. After the relationships between spouses and other relatives are removed the resulting dyadic data set includes about five hundred conversational relationships. Twenty-two percent of the relationships in our analyses involve repeated use of a main respondent—instances when a main respondent has more than one relationship represented in the data matrix.

Congruence and Dissonance Between Discussion Partners

The resulting data set offers a unique opportunity to examine political choices as they are imbedded within the social flow of political information. As a first step we examine (1) the objective correspondence between the preferences of main respondents and their discussants, (2) the perceptions of the political preferences of discussants held by main respondents, and (3) the accuracy of these

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Table 1. Proportion of Main Respondents Who Correctly Perceive Discussant's Voting Behavior

Main Respondent	Discussant		
	Nonvoter	Reagan Voter	Mondale Voter
Nonvoter	.222 (9)	.790 (19)	.818 (11)
Reagan voter	.400 (20)	.912 (170)	.662 (65)
Mondale voter	.333 (15)	.547 (53)	.922 (90)

Note: Omits relatives. Percentage of Reagan voters' relationships that produce agreement = $170/(20 + 170 + 65) = 67\%$. Percentage of Mondale voters' relationships that produce agreement = $90/(15 + 53 + 90) = 57\%$.

perceptions. Table 1 summarizes this information for the relationships included in this analysis.

Voters with similarly voting discussants show a remarkably high level of accuracy in their perception of discussant behavior: 91% of Reagan voters with Reagan discussants correctly identified discussant behavior, and 92% of Mondale voters with Mondale discussants made similarly correct identifications. Accuracy is reduced somewhat within relationships that produce disagreement. Both Reagan and Mondale voters have considerably more difficulty in correctly identifying nonvoting discussants, but 66% of Reagan voters correctly identify discussants who voted for Mondale, and 55% of Mondale voters correctly identify discussants who voted for Reagan. This general pattern changes among the nonvoting respondents: they do not accurately identify fellow nonvoting discussants (22%), but they are fairly accurate in their identification of Reagan voters (79%) and Mondale voters (82%). Finally, Table 1 shows the general levels of agreement within relationships: 67% of the Reagan voters' relationships involve discussants who voted for Reagan, and 57% of the Mondale voters' relationships involve discussants who voted for Mondale.

Magnitude in political science is, like beauty, in the eye of the beholder, but Table 1 suggests several things to us. First, these relationships are not overwhelmingly homogeneous in terms of voting preference. They include substantial levels of political disagreement measured in the metric of reported votes. Second, while perceptual accuracy generally increases in politically congruent relationships, clear majorities of voters in dissonant relationships recognize the disagreement.

What factors are responsible for objectively defined congruence and dissonance within the relationships? What factors are responsible for accurate and inaccurate perceptions?

The Structural Basis of Political Information

In the analysis that follows we consider the preferences of discussion partners from two different perspectives: the perceptions of those preferences by the main respondents, and the objective reality of those preferences as they are measured by the discussants' own self-reports. The analysis comes in three

question-stages: (1) Are the main respondents' perceptions of discussion partners affected by the political preferences of, and by the political preferences surrounding, the main respondents? (2) Are main respondents likely to have discussion partners whose objectively defined (self-reported) preferences correspond to the political preferences of, and the political preferences surrounding, the main respondents (in this instance the social context becomes important as it affects the supply of particular political preferences and, thus, the opportunities for discussion with people holding particular preferences)? and (3) Do the main respondents systematically *misperceive* discussant preferences as a function of their own preferences and as a function of the preferences surrounding them?

The relationships between networks, contexts, and individual preference are estimated using a logit model for micro data (Hanushek and Jackson 1977). The model is appropriate to these data where the dependent variables are binary—Reagan discussant or not, perceived Reagan discussant or not. An otherwise straightforward analysis is complicated by two factors. First, the choice and perception of discussion partners is one part of an inherently simultaneous social-influence process. A citizen's own political preferences influence the choice of a discussion partner, but this choice has subsequent consequences for the citizen's preference. These two phenomena are inherently dynamic and interdependent. The resulting technical problem is simultaneity leading to identification problems in the statistical models to be estimated. Discussant choice may be a function of individual preference and individual preference may be a function of discussant choice. Although we do not carry out the analysis for the implicit full simultaneous system, it is appropriate to take the simultaneity into account in estimation.

The simultaneity problem is addressed

in the analysis that follows through the construction of two instruments that measure the likelihood that any individual (1) voted for Reagan or (2) did not vote. Each instrument is constructed from the logistic regressions shown in Table 2. Thus, rather than using dummy variables to represent the main respondents' voting behavior, we employ the probabilities estimated by the instruments (Maddala 1983). The instruments avoid the measurement bias due to simultaneity. They are constructed to correlate highly with reported behavior but to be (relatively) independent of discussant behavior (Wonnacott and Wonnacott 1979).

The second estimation problem arises due to the danger of potentially omitted explanatory factors. Beginning at least with the work of Hauser (1974), critics of contextual research have demanded that alternative, individual-level explanations be taken into account before asserting a contextual effect. In principle, this is laudable, but in practice it has often meant the inclusion of long lists of correlated individual-level control variables (Kelley and McAllister 1985). It should come as no surprise that the inclusion of highly correlated explanatory variables often weakens statistical purchase. That is, additional individual-level controls run the very real danger of producing excessive colinearity and thus a misplaced willingness to accept null hypotheses.

The research design for the South Bend study provides at least a partial solution to this second problem. By centering the individual-level variable around its mean within contexts and by centering contextual means around the mean for the sample as a whole, orthogonality is produced between the centered individual measure, the centered neighborhood mean, and their multiplicative interaction. This does not guarantee orthogonality between these variables and the additional individual-level controls, but it decreases colinearity to a degree sufficient for the

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Table 2. Construction of Instruments for Main Respondents' Voting Behavior

Independent Variable	Probability of Reagan Vote	Probability of No Vote
Constant	-4.46 (4.66)	.92 (.67)
Age ^a	.01 (1.38)	-.05 (3.89)
Education ^b	.13 (2.37)	-.32 (3.72)
Party identification ^c	.83 (12.12)	—
Partisan loyalist ^d	—	.49 (2.68)
Interest ^e	—	1.50 (6.32)
Number of cases	506	506

Note: Logit model. *T*-values are shown in parentheses.

^aMain respondent's age in years.

^bEducation of main respondent (years of schooling).

^cSeven-point party-identification scale (0 = *strong Democrat*).

^d0 = *strong partisan*; 1 = *weak partisan*; 2 = *independent leaning toward a party*; 3 = *independent*.

^eInterest in election: 1 = *a great deal*; 2 = *some*; 3 = *only a little*; 4 = *none at all*.

present analyses. (See the Appendix for technical details.)

To summarize, the main respondent's voting behavior is measured through the use of two instruments that measure the likelihood of voting for Reagan and the likelihood of not voting. This removes the taint, technically, of causation from the discussion partner's behavior in statistical estimates of coefficients. A mean likelihood of voting for Reagan is then calculated within the neighborhood and for the sample as a whole. The individual-level Reagan vote instrument is centered around the respective neighborhood mean; the mean Reagan vote likelihood within neighborhoods is centered around the overall sample mean; and an interaction variable is formed from the product of these two variables. Finally, two demographic controls for income and education are included in each model as well.

These procedures are all well-understood and straightforward applica-

tions of statistical technique. They have the unfortunate side effect of appearing as a great deal of hocus-pocus; thus, we include a second model in each analysis that uses simple and straightforward procedures: no instruments, no centering, no interaction, no individual-level controls, where the main respondent's right-hand-side explanatory voting behavior is measured through the use of dummy variables. This simplified model corresponds to Alwin's formulation for estimating the effects of social context (Alwin 1976). We are not surprised (and the reader should be reassured) to find that the results are much the same for both models.

The centering procedure is best accomplished by calculating neighborhood means from the sample that is employed for a particular analysis. This means that, on average, the neighborhood means are calculated from approximately 24 interviews within each neighborhood. The reliability of the neighborhood means is

supported by their very high correlation ($r = .95$) with the proportion of the entire third-wave sample in each neighborhood voting for Reagan. And in this latter case, the average sample size in each neighborhood is more than 85. The general reliability of the survey-derived contextual measures is further supported by the correlation of third-wave neighborhood demographic means with demographic measures for the neighborhoods taken from the 1980 census. Once again, the survey measures do quite well, correlating with the census measures at the .9 level.

A final measurement issue involves response bias in the survey. Our respondents report a level of voting turnout that is in excess of the true population value. This is due in part to common overreport problems, but it is also due to the panel design. People who agree to participate repeatedly in a panel study concerned with politics are more likely to be upper-status people, and they are also more likely to be voters. Thus, the question arises whether using neighborhood means provides a reliable measure of Reagan support within the neighborhoods. If the measure is intended to provide a precise estimate of the neighborhood Reagan support to the base of all eligible voters, then it certainly fails the validity test because turnout is undoubtedly overestimated. But this bias does not render the measure useless for present purposes. What is central to the analysis is variation in Reagan support across neighborhoods, and the measure is appropriate for such use. As we showed above, survey-derived measures of neighborhood social demographics compare very well with the census-derived measures of social demographics, and there is no reason to expect that survey-derived measures of political demographics would compare any worse. Thus, in spite of the overreport bias, there is strong reason to believe that it provides a good relative measure of Reagan support across neighborhoods.

Perhaps more importantly, the measure of neighborhood Reagan support has an additional virtue: it provides an estimate of Reagan support to the base of the politically active neighborhood population—the population that should matter both substantively and theoretically. That is, the survey ends up, *de facto*, being a survey of politically engaged citizens, and politically active citizens tend to be surrounded by other active citizens. Thus, from a substantive standpoint, the bias in the neighborhood means corresponds well to the bias in the sample. In terms of anticipated effects from the social context, this is a desirable design feature. The politically active population is the theoretically correct population to sample.

The Perception of Information

Does the perception of a discussant's preference continue to vary as a function of the main respondent's own preference when the social context is taken into account? Does the perception vary as a function of Reagan support in the main respondent's context?

Table 3 displays the results for the logit model that estimates the main respondent's perception of the discussant's vote as a function of both the main respondent's voting behavior and the level of Reagan support in the main respondent's neighborhood. Reagan support at both the individual and contextual levels generates coefficients that lie in the expected direction and possess respectable *t*-values. Respondents who support Reagan and (to a lesser degree) respondents who are less likely to vote, are more likely to perceive their discussants as Reagan voters than the respondents who support Mondale. Furthermore, respondents who live among Reagan voters are more likely to perceive their discussants as Reagan voters.

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The magnitude of these effects are characterized in Table 4, where the probability of perceiving a discussant as a Reagan voter is computed from the model estimates reported in Table 3, across main respondent preferences and across the observed range of neighborhood Reagan support. In order to arrive at main respondent preferences, the mean value of both instruments is calculated within the three possible behavior categories of Reagan voter, Mondale voter, and non-voter. The two demographic controls are

held constant at the sample means. (See the Appendix for details.)

Table 4 shows that regardless of context, Reagan voters are most likely to perceive their discussants as Reagan voters and Mondale voters are least likely to do so. The table also shows that main respondents who reside among Reagan voters are more likely to perceive their discussants as Reagan voters, regardless of individual preference. This latter effect is substantial, and it compares quite favorably to the effect of individual

Table 3. Projected Reagan Vote for Discussant by Vote of Main Respondent and Reagan Support in Main Respondent's Neighborhood

Independent Variable	Instruments with Controls ^a	Original Data
Constant	-.40 (.52)	-3.15 (5.96)
Main-respondent Reagan support ^b	2.79 (7.97)	1.82 (7.78)
Non-voting main respondent ^c	.80 (1.08)	1.31 (3.51)
Neighborhood Reagan support ^d	4.58 (4.69)	4.04 (4.10)
Individual-context interaction ^e	3.88 (1.48)	—
Education of main respondent ^f	.003 (.06)	—
Income of main respondent ^g	.08 (1.06)	—
Number of cases	444	468

Note: Includes only discussants who are not relatives. Logit model. Dependent variable is projected Reagan vote. T-values are shown in parentheses. Projected Reagan vote = 1 if main respondent *thinks* discussant voted for Reagan; 0 otherwise.

^aIn this column, main-respondent Reagan support, neighborhood Reagan support, and their interaction are centered variables.

^bThe original variable equals 1 if main respondent voted for Reagan; 0 otherwise. The instrument varies from .05 to .98.

^cThe original variable equals 1 if main respondent did not vote for president; 0 otherwise. The instrument varies from .001 to .92.

^dMean level of Reagan support in main respondent's neighborhood, calculated from survey (mean instrument in left column; proportion Reagan voters in right column).

^eMain respondent Reagan support multiplied by neighborhood Reagan support.

^fYears of school completed by main respondent (possible range = 0-17; approximate mean = 13).

^gFamily income (possible range = 1-8; approximate mean = 5, or 20-30 thousand dollars per year).

Table 4. Probability That Main Respondent Perceives Discussant as a Reagan Voter by Main Respondent's Vote and the Proportional Reagan Vote in the Main Respondent's Neighborhood

Main Respondent's Vote	Neighborhood Reagan Support ^a	
	Low (.31)	High (.78)
Reagan	.38	.87
Nonvoter	.32	.76
Mondale	.21	.57

^aThese levels represent the range, on the neighborhood proportions, who reported voting for Reagan, for 1,390 nonmissing third-wave respondents. The corresponding centered neighborhood means for the Reagan support instrument, upon which the estimates are based, were -.21 and .24. Data from Table 3, col. 1.

preference. Differences of 17 and 30 points are shown between the perceptions of Mondale and Reagan voters, and the difference between main respondents living in Reagan and Mondale neighborhoods ranges between 36 and 49 points.

It is important to realize that the table displays something other than marginal effects. In an 80% Reagan neighborhood, if all that was involved was the increased supply of Reagan voters, the expected value should be the same—.8 for all three classes of voters. It is not. Indeed, the table shows either a discussant-selection bias based upon individual party preference—a consequence of personal preference imposed on social structure (Finifter 1974), or some form of misperception that is perhaps complex (Jones 1986), or both.

A common question looms behind both sets of effects. Do individual preferences and contextual properties affect the perception or the reality of the socially transmitted information? Put another way, Are these perceptions rooted in an accurate assessment of discussant preferences or in an interpretation that distorts discussants' actual (self-reported) preferences? We divide this problem into two components by (1) investigating the discussants' self-reported preferences before (2) examining the main respondents' perceptions in light of discussants' self-reported preferences.

The Content of Information

Is the objective content of socially transmitted information affected by individual preference? Is it affected by the social context? These questions are attacked indirectly by considering the discussant's self-reported vote (interpreted here as the objective political content of socially transmitted information) as a function of the main respondent's vote and the level of Reagan support in the main respondent's neighborhood. Our concern is not with the conditions that affect perceptions of discussant preferences but rather with the conditions that affect the reality from which these perceptions are developed.

The logit model of Table 5 estimates the choice of a Reagan voting discussant as a function of the main respondent's voting preference and the main respondent's social context. Essentially, the table answers the question, How powerful are your preferences and the preferences that surround you in determining the likelihood that you will have a discussion partner who reports a particular preference? The coefficients of central interest lie in the expected direction and each possesses a satisfactory *t*-value. Reagan supporters are more likely to have discussants who voted for Reagan, and main respondents living among Reagan voters are more

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Table 5. Self-reported Reagan Vote for Discussant by Vote of Main Respondent, and Reagan Support in Main Respondent's Neighborhood

Independent Variable	Instruments with Controls ^a	Original Data
Constant	.28 (.41)	-1.69 (3.72)
Main-respondent Reagan support	2.11 (6.43)	1.03 (4.91)
Nonvoting main respondent	1.03 (1.48)	.52 (1.56)
Neighborhood Reagan support	2.72 (3.04)	2.34 (2.68)
Individual/context interaction	2.84 (1.13)	—
Education of main respondent	-.08 (1.46)	—
Income of main respondent	.16 (2.19)	—
Number of cases	466	491

Note: Includes only discussants who are not relatives. Logit model. Dependent variable is discussant Reagan vote. *T*-values are shown in parentheses. Discussant Reagan vote = 1 if discussant reports voting for Reagan; 0 otherwise.

^aIn this column, main-respondent Reagan support, neighborhood Reagan support, and their interaction are centered variables.

likely to have discussants who voted for Reagan. The table provides evidence for the selective choice of discussants based on the preferences of main respondents and also evidence for social coercion arising from the partisan supply available in the social context of the main respondents. It is not only the perception of discussant preferences that is affected by context but the reality underlying these perceptions as well. Not only are citizens who live in contexts dominated by Reagan supporters more likely to perceive their discussion partners as Reagan voters, but these citizens are also more likely to have discussion partners who *are* Reagan voters.

The magnitudes of these effects upon the probability of having a self-reported Reagan voter for a discussant are displayed in Table 6. As before, the effect of context compares favorably to the effect

of individual preference, but the effects demonstrated in Table 4 are somewhat more pronounced. Reagan voters are 15 and 26 points more likely than Mondale voters to have a discussant who voted for Reagan. And in comparison to main respondents living among Mondale voters, main respondents who live among Reagan voters are between 22 and 33 points more likely to have a discussant who voted for Reagan. Thus, the objective political content of socially transmitted information (the true partisanship of the discussant) depends both upon associational choice on the part of the receiver and upon the constraints on supply arising from the political composition of the social context that circumscribes that choice.

Note that there is no reason to expect the partisan behavior of discussion partners to reflect the contextual distribution

Table 6. Probability That Discussant Voted for Reagan by Main Respondent's Vote and the Proportional Reagan Vote in the Main Respondent's Neighborhood

Main Respondent's Vote	Neighborhood Reagan Support	
	Low (.31)	High (.78)
Reagan	.46	.79
Nonvoter	.44	.71
Mondale	.31	.53

Note: Data from Table 5, col. 1.

of partisanship if discussion partners are chosen only on the basis of the selector's individual partisanship. The latter condition predicts the same cell entries in both columns of a given row in Table 6, which manifestly is not the case. In short, then, the objective content of the partisan signal in the political discussion process is a function of both the individual's preferences and the preferences in the individual's surroundings. But does this make any difference? If misperception is sufficiently systematic and sufficiently strong, then objective content may not matter. We now turn to an investigation of this final issue.

The Perception and Misperception of Information

Is the misperception of socially transmitted information affected by the political preferences of the receiver? Is it affected by the social context? More importantly, does misperception reinforce individual political preference in the choice of discussion partners and thereby offset the potential of the social context to expose individuals to sources of dissonant information?

To address these issues, it is necessary to take account of the objective (self-reported) voting behavior of both the discussant and the main respondent when examining the main respondent's percep-

tions of the discussant's vote. The logic of the analysis is to investigate the main respondent's perception of the discussant's vote while controlling for the discussant's objectively defined (self-reported) vote. With objective content controlled, it is possible to assess whether individual misperception overwhelms the effect arising from information exposure to partisan distributions in the individual's social context.

Table 7 displays the logit model that is obtained when the main respondent's perception is treated as the left-hand-side dependent variable, and the self-reported voting behaviors of both the main respondent and the discussant, as well as Reagan support in the neighborhood, are treated as right-hand-side explanatory variables. The explanatory variables of central interest produce coefficients that lie in the expected direction with crisp *t*-values. In general, main respondents are more likely to perceive a discussant as a Reagan voter if the discussant voted for Reagan (objective reality), the main respondent resides among Reagan supporters (social reality), and the main respondent supported Reagan (subjective reality). The table shows that controlling for the discussant's objective partisanship does not eliminate either individual-preference effects or social-context effects on perception.

These results become more provocative when the magnitudes of effects are considered in Table 8. This table is striking

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Table 7. Projected Reagan Vote for Discussant by Vote of Main Respondent, Self-reported Vote of Discussant, and Reagan Support in Main Respondent's Neighborhood

Independent Variable	Instruments with Controls ^a	Original Data
Constant	-2.33 (2.40)	-4.39 (6.52)
Discussant Reagan vote	2.81 (9.52)	2.94 (10.17)
Nonvoting discussant ^b	.49 (1.11)	.46 (1.08)
Main-respondent Reagan support	2.10 (4.95)	1.77 (6.00)
Nonvoting main respondent	.62 (.66)	1.39 (2.97)
Neighborhood Reagan support	4.20 (3.59)	3.45 (2.93)
Individual-context interaction	3.26 (1.02)	—
Education of main respondent	.07 (1.01)	—
Income of main respondent	-.01 (.11)	—
Number of cases	428	450

Note: Includes only discussants who are not relatives. Logit model. Dependent variable is projected Reagan vote. *T*-values are shown in parentheses.

^aIn this column main-respondent Reagan support, neighborhood Reagan support, and their interaction are centered variables.

^b1 if discussant did not vote; 0 otherwise.

Table 8. Probability That Main Respondent Perceives Discussant as a Reagan Voter by Main Respondent's Vote, and the Proportional Reagan Vote in the Main Respondent's Neighborhood

Vote of the Main Respondent and Discussant	Neighborhood Reagan Support	
	Low (.31)	High (.78)
Reagan-Reagan	.68	.95
Reagan-nonvoter	.18	.65
Reagan-Mondale	.12	.53
Nonvoter-Reagan	.64	.91
Nonvoter-nonvoter	.15	.51
Nonvoter-Mondale	.10	.39
Mondale-Reagan	.54	.84
Mondale-nonvoter	.10	.34
Mondale-Mondale	.07	.24

Note: Data from Table 7, col. 1.

Table 9. Probability That Main Respondent Correctly Perceives Whether or Not the Discussant Is a Reagan Voter by Main Respondent's Vote, the Discussant's Vote, and the Proportional Reagan Vote in the Main Respondent's Neighborhood

Vote of the Main Respondent and Discussant	Neighborhood Reagan Support	
	Low (.31)	High (.78)
Reagan-Reagan	.68	.95
Reagan-Mondale	.88	.47
Mondale-Reagan	.54	.84
Mondale-Mondale	.93	.76

Note: For main respondents and discussants who voted for Reagan or Mondale. Data from Table 7, col. 1.

for the probability differences it reveals between objectively homogeneous versus objectively dissonant dyads and, similarly, between politically supportive and nonsupportive social contexts. These results can be made even more vivid by focusing on only the dyads of central theoretical interest and by transforming the dependent variable. In Table 9 the dependent variable has been transformed into the probability of correctly identifying whether the discussant is a Reagan voter. Thus, a main respondent who perceives a Mondale voter as a Reagan voter is incorrect, as is a main respondent who perceives a Reagan voter as a non-voter, and so forth.

Table 9 is worth some study. It shows, first, that voters tend to be accurate in their perceptions of discussion partners who agree with their own preferences: Reagan voters tend to recognize other Reagan voters, and Mondale voters tend to recognize other Mondale voters. This is especially the case when surrounding preferences are supportive of the voter's preference. In both instances the social context has at least a modest effect upon perceptual accuracy. But now examine disagreeing dyads.

Clearly, the greatest opportunity for contextual effects upon perceptual accuracy arises in discussion dyads that involve heterogeneous preferences. This is

consistent with theory about the dynamics of the political discussion process (Huckfeldt 1986; MacKuen and Brown 1987; McPhee 1963; Sprague 1982). Reagan voters are more likely to perceive Mondale discussants accurately in Mondale contexts and less likely to perceive them accurately in Reagan contexts. Similarly, Mondale voters are more likely to perceive Reagan discussants accurately in Reagan contexts and less likely to perceive them accurately in Mondale contexts. Thus, majority versus minority status for a particular political preference in the social context emerges as a crucial consideration in cases where there is objective political disagreement between discussion partners (Noelle-Neumann 1984).

Finally, notice that the holders of a *minority* preference are more likely to perceive the *majority* preference accurately: they are more likely to perceive their discussion partner's political preference correctly if *disagreement* is present within the dyad. Thus, the social context transforms the nature of discussion dyads even between discussion partners who share the same preference. Members of the minority evidently come to expect political dissonance during social encounters and frequently fail to recognize political agreement even when it is present.

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Interpretation

Two interpretive comments are central to understanding these results. First, a politically supportive social context sustains the receiver's misperception of dissonance-producing, socially transmitted political information. Second, members of the political minority accurately perceive members of the majority, while members of the majority do not accurately perceive members of the minority.

More analysis is necessary to sort out the nature of these contextual effects, but the substantive political consequences seem clear. Minorities are debilitated (Miller 1956) because they are often acutely aware of their own minority status. They are likely to recognize the variance between their own preferences and those of their surroundings. In contrast, and for whatever reason, dissonant information is frequently ignored by members of the majority.

In short, the majority enjoys a double benefit while the minority suffers a double liability. Not only are members of the minority more likely to encounter dissonance-producing information, they are also more likely to recognize it as such. Not only are members of the majority less likely to encounter dissonance-producing information, they are also less likely to recognize it as such.

Conclusion

The empirical analyses above emphasize the interdependence of private preference and politically relevant distributions in the individual's social context. The interesting conditions, as expected on the basis of theory, are those of dissonance—either between the main respondent and the discussant, or between the main respondent and the preponderance of political opinion in the respondent's social context. It is clear,

especially from Table 9, that these conditions interact.

It would appear that individuals do purposefully attempt to construct informational networks corresponding to their own political preferences. This exercise of choice, however, is not independent from socially determined conditions of supply—from the contextually imposed opportunity (or lack of opportunity) for social interaction with people who hold various political preferences. Similarly, selective misperception is apparent in the analyses, and it, too, is socially conditioned. (These issues are addressed in Huckfeldt and Sprague, n.d.)

The most interesting feature of the empirical work, perhaps the most surprising result, and certainly the most significant for the operation of democratic politics, is the asymmetry in choice and perception arising from majority or minority political status in one's social context. Political majorities are able to ignore dissonant information. Political minorities, in contrast, suffer from a heightened level of vulnerability for the simple reason that their members accurately perceive the incoming flow of dissonant information. This analysis reveals, then, a process of informational coercion of political minorities achieved through mechanisms of social interaction.

Appendix

The analysis conducted here benefits greatly from the statistical framework for contextual analysis set forth by Boyd and Iversen (1979), but it also marks a significant departure from their model. (For another application of the Boyd-Iversen model see Esser 1982.) They develop their argument within the general framework of the linear model, but we adapt part of their centering procedure to this analysis using the nonlinear logit model. The differences are sufficient to warrant a brief explanation.

First, a great virtue of the Boyd and Iversen approach is that variance may be partitioned between individual effects, contextual effects, interaction effects, and error. Since the logit model has no error term, this is impossible in the current application.

Second, Boyd and Iversen develop some useful techniques to check for appropriate model specification that cannot be used in this instance. The root cause of this inability is, once again, the lack of an error term. In particular, we are unable to check for homogeneity of error across contexts, or for correlation between explanatory variables and error across contexts. An especially crucial test in the Boyd and Iversen model is the ability to recover equivalent coefficients for the two-equation and the single-equation specifications, but the two-equation specification cannot be estimated using the logit model. Thus, while we cannot carry out this check using the logit model, we have carried it out with the South Bend data for other specifications of the linear model with different dependent variables. These tests have been uniformly successful.

Our inability to carry out the Boyd and Iversen tests for misspecification is unfortunate, but it is not crippling, at least by contemporary standards. The vast majority of contextual studies, and indeed every empirical study, make assumptions regarding the behavior of error terms. The utility of Boyd and Iversen's approach is that it offers a way to check many of these assumptions, but few empirical studies are able to employ such a technology.

Finally, the Boyd and Iversen centering procedure not only transforms the explanatory variables, but the dependent variable as well, in order to maintain the original metric on the estimated coefficients. This cannot be done in the current instance; thus, the metric for coefficients is transformed. This is not a problem, however, because the metric for coeffi-

cients in a nonlinear model, such as the logit model, offers little interpretive insight. Instead of interpreting the magnitudes of coefficients, we use the logit model to calculate change in probabilities across values of one explanatory variable while other explanatory and control variables are held constant. As was stated in the text, social demographics are controlled at their sample means, and the instruments are controlled at their mean levels for the respective reported vote categories. The range on centered neighborhood means for Reagan support is calculated on the basis of the entire third-wave survey in order to take advantage of the larger sample size. The following outline shows the values for the controlled variables, as well as providing the central formulas for the centering procedure:

A. Centering procedure

Centered Reagan support at the individual level = $R_{ij} - \bar{R}_j$

Centered Reagan support within neighborhoods = $\bar{R}_j - \bar{R}$

where

R_{ij} = the score of the i th respondent in the j th neighborhood on the Reagan support instrument

\bar{R}_j = the mean score on the Reagan support instrument in the j th context

\bar{R} = the mean score on the Reagan support instrument for the sample

B. Control values used to generate estimated probabilities in Tables 4, 6, 8, and 9

1. Demographic means

Education = 13

Income = 5

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2. Means for instruments across reported vote categories

Instruments	Reported Vote		
	Reagan	No Vote	Mondale
Centered Reagan vote probability	.18	-.08	-.26
Nonvote probability (not centered)	.06	.36	.09

3. The range on centered neighborhood Reagan support (calculated from the entire third wave)

highest neighborhood level of Reagan support = .24

lowest neighborhood level of Reagan support = -.21

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WHO WORKS WITH WHOM? INTEREST GROUP ALLIANCES AND OPPOSITION

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Interest-group interactions may be examined in ways comparable to the analysis of conflict and coalition in other areas of political science. We seek to measure and compare the structure of interest-group participation and conflict in four domains of U.S. domestic policy: agriculture, energy, health, and labor. Data are drawn from a survey of 806 representatives of organizations with interests in federal policy, supplemented by interviews with 301 government officials in the same four domains. Several types of data are adduced regarding the intensity and partisanship of group conflict in each domain and the range and variety of group participation. Coalitional patterns are described and the mutual positioning of different kinds of organization—peak-association groups versus more specialized trade, professional, or commodity groups, for example—are examined.

Some fields of intellectual endeavor enjoy the luxury of exploring well-defined problems whose difficulties, however intractable, are themselves part of a self-conscious culture of inquiry. Other fields of academic effort may well have substantial common ground, but its contours and possibilities have not yet been fully recognized by those committed to its exploration. One such field is interest-group politics. For two decades now we have gradually been coming to appreciate that the questions and concepts we had thought were central to this area of investigation were substantially misspecified and that quite

another tack was needed if we were to make sense, theoretical or descriptive, of the burgeoning mass of activity to which we attach the label of *interest groups*.

Since Mancur Olson (1965) turned us in a different direction, we have been the beneficiaries of important work on the origins and maintenance of voluntary associations (inter alia, Berry 1977; Moe 1980; Salisbury 1969; Walker 1983). Concepts have been reformulated in order to encompass institutions within the interest-group frame of reference (Salisbury 1984). We know much more than we did before about lobbying tactics and strategy (Berry 1977; Schlozman and Tierney 1985). We

have accumulated sophisticated treatments of both individual group and sector-level development and change over time (Browne and Salisbury 1972; Hanson 1985; Laumann and Knoke 1987; Starr 1982; Walker 1983). And we have useful cross-national comparisons of the structural ties among business, labor, and the state (Schmitter and Lehmbruch 1979; Wilson 1985). Here we propose to extend the range of our attention to a topic central to many fields of political science but not yet addressed by students of interest groups—the structure of conflict.

Whenever a more or less well-defined system of interactions can be observed or inferred—in legislatures, electoral politics, or international relations, to take three good examples—we move quickly to characterize that system in terms of the conflicts over scarce resources that have so long been held to be the defining feature of politics. We may focus on particular episodes—the struggle over a bill in Congress, a specific presidential contest, or the Falklands War. We may select out some feature of the overall structure for attention—the committee system, third-party challenges, or coalition patterns in the United Nations. We have developed quite a few fragments of theory regarding both generic conflict patterns and behavior conditioned thereby and the operation of specific institutional systems. It may be that the linkages between the more abstract and the more particular are sometimes weakly forged, but it seems plain that substantial progress has been registered by growth in cumulative understanding, theoretical and empirical.

We propose to argue that interest-group politics in the United States constitutes a system, or set of systems, defined by interaction among organized groups and between those groups and public officials. The structure of this system, and of subsystems within it, may usefully be characterized in terms of the extent and shape of conflict among par-

ticipants. This structure is not evident in every instance of policy dispute, nor is it necessarily revealed by any particular case example of interest-group activity. But, as with voting studies or congressional roll calls, the interest-group system or systems can be discerned at more aggregated levels of observation.

The Research Design

It is ironic that it should have taken so long to get at the structure of interest-group conflict. Since the conceptualizations of Arthur Bentley (1908) began to work their yeasty way into political science, it has been commonplace to put group conflict at the very core of our thinking. Having done so, however, we have been so far unable to go beyond examples to reach more comprehensive formulations of how group interactions are structured. Is there much conflict or little? Are the main group players aggregated into two opposing camps or fragmented among several "parties"? Does the structure vary from one policy area to another? We cannot speak to these questions from existing literature. Recent work has greatly improved our understanding of how groups are formed and what they do, but it has not addressed the structure of conflict (for a partial exception, see Schlozman and Tierney 1985, 283-88). Part of the reason may be the absence of any readily observed defining behavior. Party systems can be examined by looking at the distribution of votes. So can legislatures. So can the Supreme Court. The executive branch is more difficult because criterion actions that could be the basis for characterizing their relationships are not visibly undertaken by all participants. Interest groups presented the same difficulty. No set of observations of group activity with enough scope of coverage or iterations of behavior has been made to allow reasonable inferences

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to be drawn regarding the existence and shape of systemic structure. Our data overcome these difficulties.

The design of our research began with the construction of lists of organizational participants in each of four policy areas—agriculture, energy, health, and labor. It was our firm belief that unless we linked our analysis of interest representation to the substantive policy concerns of the groups involved we would have a bloodless set of responses, interest-group activity without interests. In order to secure enough respondents in a given policy area to enable our analysis effectively to incorporate these substantive considerations, it was necessary to limit ourselves to a few policy domains. Our selection was bound to be somewhat arbitrary, but we have sought to tap some of the important dimensions of interest representation and to capture a reasonable portion of the range and diversity of representational activities.

The policy domains we examine exhibit very different patterns of institutional and policy change over recent decades. In agriculture and labor there has been a relatively stable set of executive agencies and congressional committees and subcommittees; and the programs in these fields, while not without controversy, have displayed substantial continuity in general design. Health and (especially) energy, by contrast, have undergone much greater changes. The past decade has seen each come under the jurisdiction of a new or redesigned cabinet-level department. Congress has developed new mechanisms to deal with energy issues, and the issue agendas of these two fields have been in great flux.

Both level and form of conflict were expected to vary. Labor policy typically pits two broad constellations of interests—business and labor—against each other, and the struggles are sometimes of epic proportions. Health matters, by contrast, were once the focus of great struggle, but,

although health-care-cost containment continues to inspire sharp disagreement, most health issues in recent years have attracted quite muted political voices. Apart from the labor field, we expect to find a general tendency toward interest proliferation and fragmentation as the interests of various types of specialized producers—doctors, insurance companies, medical schools, and so on in health; oil and gas, coal, nuclear, and other fuel interests in energy; various crop and commodity interests in agriculture—grow increasingly differentiated. To be sure, there are peak associations in some domains—agriculture and health, as well as labor—but no organization can claim even this status in energy, and in the other fields the hegemony of the American Medical Association or the American Farm Bureau Federation has been badly eroded in recent years.

We expect our domains to display diversity, therefore, in the patterns of interaction among organizational actors, and we will be able to investigate to what extent this diversity reflects differences in the substantive policy and organizational composition of the domains. Alternatively, we can estimate how much of what individual-interest representatives *do* reflects their personal backgrounds and present job circumstances and how much is, in effect, imposed by the contemporary context of political and institutional forces within which interest representation must operate.

Our selection of policy domains is skewed toward domestic policy. Issues of foreign trade and other international concerns do arise, but they are not of central concern in any of the domains in our study. Interest representation on domestic matters may be larger in volume and differently structured than on matters of national defense, foreign aid, and other issues involving international considerations. Even those differences are hardly total, however, and we would hope that

our findings sufficiently reflect the bulk of the policy process to be of some relevance to the understanding of private-interest representation well beyond the four domains of policy examined.

The procedures employed to identify organizations active in a particular policy domain required substantial documentary investigation and preliminary interviewing. No existing list adequately defines the population of interested participants. Each policy area includes several subsystems that attract somewhat different types of participants, and any single method of locating participating organizations has a systematic bias toward identifying certain kinds of groups and neglecting others (Salisbury 1984). We used four different listings of organizations for each policy domain, and the resulting rosters are broadly inclusive not only of the highly active groups but also of the marginal and intermittent participants. The sampling methods are more fully described in the Appendix.

Having defined the universe of group participation (and welcoming the natural weighting that resulted from multiple nominations), we drew a sample of 311 organizations, equally distributed across the four domains. By telephone we interviewed an appropriate official from each, asking, among other things, who represented their interests in Washington. Each group could name up to four individuals within the organization and up to four people from outside. These nominations (totaling 1,716 individuals) then became the basis for sampling respondents. A total of 774 were interviewed in person, a response rate of 77%. Interviews averaged about 75 minutes in duration. These data form the heart of our research. We asked each respondent to name three government officials with whom he or she regularly interacted and then interviewed 301 of those officials. Finally, we added 32 persons who had been among 72

nominated by informants as notable participants in the four policy domains but who, unlike the other 40, had not appeared in our sample.

Some of the analysis that follows treats respondents as individuals constrained by operating within a policy domain and other factors as well but giving their personal reactions and estimates. In some of the analysis, however, we let the individuals stand for the organizations that employ them and treat the organizations as the actors. This is appropriate for purposes of examining the patterns of organizational conflict, but it generates one unanticipated and unfortunate side effect. Approximately 20% of our respondents are external lawyers and consultants. Each was nominated by an organization, but in a fair number of instances they work also for other clients. We found that when asked about allies and adversaries or about actions and attitudes relating to specific events, these respondents gave replies that could not dependably be assigned to the nominating organization. Too often we could not tell what interests were reflected. Consequently, we determined to treat external lawyers and consultants as employees of their respective firms and not try to include them in the ally-adversary structures.

Quite a few of the larger organizations are represented by more than one respondent in our sample; indeed, there are as many as four from the AFL-CIO, the chamber of commerce, and a few others of comparable significance. There are also some state and local chapters or branches of national organizations, which we treat as part of the same category as the parent group.

We turn now to our empirical findings regarding the structure of group conflict in the four policy domains. We have three distinct sets of data bearing on the general question. First, for both Washington representatives and government officials,

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we report perceptions of conflict in each domain. We compare domains to see what differences appear in these perceptions and also examine some likely indicators of individual variation in perception. Second, we look at the patterning of alliance and opposition as revealed by responses to questions asking interest representatives to nominate other groups they worked with or considered as adversaries. A third set of data, to which only brief reference will be made in this paper, is drawn from responses to questions regarding actual participation in each of some 20 very specific events that occurred during the five years prior to our interviews in 1983-84. If a group had been active and taken a particular position, it could then be located, through smallest-space analysis, in relation to all the other groups active in that domain. From these quite different data sets we derive reasonably consistent pictures of the extent and shape of group conflict in each domain. In turn, we can compare those structures and draw inferences regarding both policy-domain characteristics and, more generally, the character of interest-group conflict in the United States.

The Perception of Issues

As we attempt to assess the nature of interest conflict in our four policy domains, it will be helpful to have some idea of how the interest representatives themselves characterize the issues they deal with. In this section we examine three dimensions: *stability*, *scope of participation*, and *intensity of conflict*. Our data are drawn from responses to pairs of opposing statements. Respondents placed themselves on a five-point scale, but in the presentation that follows, rather than treat them as interval data, we simply subtract the percentage of *disagree* responses from the percentage of *agree* choices. In no instance was there any discernible bimodality of responses; thus

we are satisfied we have not lost or obscured anything substantial in opting for this simple procedure. The data are presented in Table 1.

We present two measures of issue stability, one on the long-livedness of issues in the domain and one on the stability of group coalitions. A large majority in each policy area sees the issues as long lasting, and both government officials and interest representatives share this view. Indeed, the agreement is sufficiently great that it might be accepted as settled that most of the business of policy-making entails working and reworking issues with a long history. Not many "new" items get on the agenda, and few matters are ever truly disposed of with dispatch.

There is far less agreement regarding the stability of issue coalitions, though in each domain a modest majority perceives stability rather than flux. The size of that majority is reasonably consistent across policy domains, though higher in labor than elsewhere. Except in the health field, government officials perceive substantially less stability than do interest representatives. We will explore the possible meaning of these differences after we have reviewed the other two sets of issue characteristics.

To assess the scope of interest-group activity in each domain, we asked respondents to estimate the amount of attention given by the general public to domain issues and the relative number of groups actively interested in them. Two points can be made. First, the energy field is seen as considerably broader in participation and attention than the others. Second, agriculture is seen as the narrowest. Doubtless this reflects the excitement surrounding energy issues in the period of our inquiry, but it may also be true that there is a particularly wide array of substantive concerns in this domain. It is interesting that energy representatives offer such high scope estimates. In the health field the representatives give much

Table 1. Perceptions of Issue Characteristics (in Percentages)

Domain Issue Characteristics	Agriculture		Energy		Health		Labor		Total	
	Representatives	Government Officials	Representatives	Government Officials	Representatives	Government Officials	Representatives	Government Officials	Representatives	Government Officials
Issue stability										
Issues on long-lived	68	52	78	88	71	59	68	56	72	64
Coalitions are stable	26	5	21	17	28	28	35	19	27	22
Scope of domain participation										
High public visibility	38	47	78	65	43	65	54	63	53	60
Large number of actors	17	35	52	53	33	61	34	47	34	49
Level of domain conflict										
High intensity of conflict	49	6	70	88	44	45	70	69	48	67
Strong partisanship	-18	-11	11	31	-7	3	37	46	4	15
Number of respondents	189	74	181	80	214	73	190	74	774	301

Note: Figures are the percentage of positive responses minus the percentage of negative responses.

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lower figures than the government officials. This suggests that the health representatives are more substantively specialized than the officials, and hence encounter a smaller slice of the domain's business. In energy, it appears, this distinction is not present. Energy politics have been in such turmoil that it is not surprising that both officials and representatives have been exposed to a broad scope of activity.

A third dimension of domain-issue characteristics involves the degree of group conflict. We asked about the intensity of conflict in general and also about the extent to which domain issues were contested in partisan terms. The two questions draw sharply different responses—conflict is generally said to be high and partisanship to be low. Government officials rather consistently perceive somewhat greater conflict than do private-interest representatives. As one moves from one domain to the next, however, responses on the two questions move more or less together and display rather startling domain differences. Agriculture and health are less conflict-dominated and much less partisan than energy and labor. The labor domain appears to be intensely partisan in comparison with the other domains, and this characteristic ought to appear in the data on allies and adversaries as well. Partisan conflict should be expressed as essentially bipolar opposition whereas the ubiquitous but less partisan disputes of the energy realm might be expected to take a more dispersed, pluralistic form. When we examine the ally-adversary data, we can see whether this expectation is confirmed.

In an effort to probe more deeply into the sources of variation in the issue perceptions of individual representatives, we undertook an extensive probit analysis, employing a considerable number of independent variables. The results were not sufficiently robust to warrant their full

presentation here, but they did generally support one important conclusion. We employed several different indicators of individual characteristics, including organizational position, prior government experience, present degree of policy representation effort, and personal values. A contrasting set of considerations consisted of the policy domain involved and the substantive type of organization represented, including labor unions, farm groups, and so on. In virtually every case the latter considerations had much more substantial effects. Agriculture is quiet and nonpartisan; labor is noisy, conflictual, and partisan. Energy and health are highly visible with many participants, while agriculture is perceived to be otherwise. Peak-association representatives see themselves more embroiled in controversies of broader scope and attention (apart from the actual range of their issue involvement) than do those who serve more specialized interests. These differences make reasonable sense and give support to our hope of capturing some diversity by our choices of policy domains.

Allies and Adversaries: Who Works with (and against) Whom?

In this section we examine responses to two questions. First, we asked whether the organization employing the respondent regularly encountered other organizations, as either allies or adversaries, in the particular policy domain. If they answered affirmatively, we asked the respondent to name up to three of each. The responses allow us to chart in considerable detail the patterning of inter-organizational relationships. We will distinguish between organizations that are deeply embroiled in group conflict and those that remain on relatively friendly terms with others in the domain. We will

Table 2. Nominations of Interest-Group Allies and Adversaries

Domain	Allies	Adversaries
Agriculture		
Total nominations	422	287
Organizations named	186	116
Nominators	161	133
Energy		
Total nominations	449	321
Organizations named	143	95
Nominators	165	142
Health		
Total nominations	497	244
Organizations named	215	96
Nominators	185	125
Labor		
Total nominations	446	349
Organizations named	184	99
Nominations	167	151

also examine how domains differ in their conflict structures and the extent to which those differences are embedded in the substantive differences among interests in different domains.

Overwhelming majorities in each domain are able to identify both allies (89.9%) and adversaries (74.8%). Labor is most completely defined in ally-adversary terms, health and agriculture somewhat less so. Responses do not differ much by organizational position, though government-affairs specialists are a bit more likely than others to name adversaries.

Probit analysis of the same kind referred to in the previous section indicates that indeed the labor domain is especially likely to generate adversaries. Moreover, labor groups in whatever domain they participate, are significantly more likely to identify adversaries than are business peaks, business corporations, citizen or externality groups, and energy producers. Time spent by the representative in the domain also makes it likely that adversaries will be named. The full equation produces an estimated r-square of .27 and correctly predicts 77.5% of the cases.

That seems a reasonably robust result and one in which most of the predictive power is found in the categories of organizations rather than in individual-level variables or in the domain. As we examine the interconnections among these organizational types, we will thus have good reason to regard them as meaningful, apart from other considerations.

When we turn to the actual nominations, we find a very sizable array of organizations named. Table 2 presents the total numbers for each domain. We really cannot say whether the domain differences in Table 2 are large or small because we lack any comparative standard, but it is clear from the ratio of nominators to nominee organizations that in the energy domain the players agree somewhat more fully about both friends and enemies. In the labor area, the adversary list is relatively concentrated, while in both agriculture and health allies and adversaries are more diffuse.

In order to comprehend the specific alliances and antagonisms in each domain, we have sorted both the nominating organizations and their nominees into groups of similar organizational types.

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Table 3. Allies and Adversaries in Agriculture

Nominee Organization	Nominating Organizations			
	Farm Peaks	Commodity Groups	Trade Associations	Externality Groups
Ally				
Farm peaks	30	26	9	6
Commodity groups	27	60	13	5
Trade associations	8	10	41	4
Externality groups	5	0	6	38
Number of respondents	26	35	27	24
Adversary				
Farm peaks	20	18	3	9
Commodity groups	12	3	2	5
Trade associations	2	6	1	15
Externality groups	6	25	41	9
Number of respondents	22	28	19	20

We wished to make the process as parsimonious as possible, but we also felt it necessary to adjust our categories somewhat to take account of elements specific to each policy domain. Many organizations defy easy categorization. Energy firms are often involved in the production or sale of more than one kind of fuel, for example, and a classification of organizations based on fuel type, of critical importance in energy politics (Chubb 1983), encounters difficulties. In agriculture some commodity groups are composed entirely of farmer-producers while others include processors, shippers, commodity brokers, and so on (see Guither 1980). We do not claim to have escaped error, but we believe that most of the more than one thousand organizations named were sensibly classified, and that the findings, accordingly, are credible.

The agriculture domain presents the simplest array of categories; though not the plainest structure (cf. Bonnen 1980; Browne 1986; Hanson 1985). The four categories in Table 3 are *farm peak organizations*, the general organizations of farmers and their subsidiaries; *commodity groups*, producers or producer-dominated organizations specializing in a

single crop or farm commodity; *trade associations*, organizations of corporations not directly involved in agricultural production; and, borrowing Hadwiger's (1982) term, *externality groups*, including environmental, welfare, labor, and consumer groups concerned about the externalities of farm policies.

Table 3 presents the number of nominations received by organizations in each category (omitting the considerable number scattered across other kinds of groups). It shows several things of importance. First, the groups in each category tend very strongly to find their allies within their own organizational category. As we will see, this is true also in every other domain. Secondly, farm-peak organizations are internally divided. Both friends and enemies are found there, by the peak organizations and by each of the other sectors. To a lesser extent this is true also of the externality set. Neither commodity groups nor trade associations, on the other hand, often find adversaries internally or in the other. Despite the great diversity within each category and the real potential for conflict between, say, feed producers and livestock interests, the reported organizational alignments do not

Table 4. Allies and Adversaries in Energy

Nominee Organization	Nominating Organizations				
	Business Peaks	Oil and Gas	Nuclear and Electric	Trade Associations	Environmental Groups
Ally					
Business peaks	3	10	1	15	3
Oil and gas	11	57	4	23	0
Nuclear and electric	4	3	38	11	0
Trade associations	6	7	5	24	0
Environmental groups	0	1	8	1	18
Number of respondents	9	32	27	31	8
Adversary					
Business peaks	0	0	0	0	0
Oil and gas	1	14	0	5	4
Nuclear and electric	0	1	5	1	5
Trade associations	0	2	3	1	4
Environmental groups	14	37	51	46	3
Number of respondents	8	27	26	29	8

suggest such conflict. The primary object of adversarial concern among these two sets of specialized interests, commodity groups and trade associations, is the externality category. Apart from the externality groups themselves, few organizations choose them as allies, and they loom very large as opponents. As we will see, a comparable tendency can be observed in other policy domains as well.

The energy field reflects a pattern much like that of agriculture, with one principal exception. In energy politics the role of peak associations—those that transcend particular industries, like the chamber of commerce or the National Association of Manufacturers (NAM)—is quite modest. Here again, however, we see specialized-producer interests working closely with one another and with relatively few adversaries within the industry. Trade associations are less differentiated from producers in energy than they are from those in agriculture, though in both domains they seem to avoid making enemies among producer groups. Hostility from the producer interests and peak business organizations is visited on the environmentalists. Three-fourths of the adver-

saries named by the energy groups included in Table 4 were environmental groups. Rarely were they said to be allies. Thus they occupy the same functional position in energy as externality groups in agriculture, a category that in that domain also includes environmentalists.

The health-policy domain presents some features resembling energy and agriculture. Here, too, we find that interests select their allies primarily from within their own organizational categories. The externality groups—citizens organizations of various sorts including the Nader-related groups, and labor unions—are often the targets of adversarial relations. More often than in other domains we have examined, however, these groups are also chosen as allies. We take this to mean that the health domain is somewhat less rancorous than others. We noted earlier that a smaller proportion of health respondents had indicated that they regularly encountered opposing groups. Moreover, health representatives perceived their issues to be less conflict ridden and less partisan than did participants in other domains. The diversity of relationships with the *citizen and labor*

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Table 5. Allies and Adversaries in Health

Nominee Organization	Nominating Organizations					
	Medical Associations	Academic Groups	Hospitals	Disease Groups and Nonfederal Officials	Trade Associations	Citizens and Labor
Ally						
Medical associations	41	10	6	3	0	6
Academic groups	15	39	8	0	0	0
Hospitals	14	10	42	3	2	4
Disease groups and nonfederal officials	6	1	0	29	0	10
Trade associations	1	0	0	0	46	1
Citizens and labor	5	5	6	8	6	50
Number of respondents	31	25	23	19	20	25
Adversary						
Medical associations	19	1	0	0	0	7
Academic groups	1	0	0	0	0	0
Hospitals	3	2	1	5	0	11
Disease groups and nonfederal officials	0	0	0	3	2	0
Trade associations	2	1	2	4	3	8
Citizens and labor	17	9	11	6	21	16
Number of respondents	21	13	14	10	16	19

category is further evidence on the same point.

In the health domain there are no genuine peak associations. The American Medical Association might be so regarded, but only for doctors, and it is obvious that doctors are only one among several sets of interest-group participants, albeit a highly significant set. No health group successfully transcends the several major categories of interested parties and lays a persuasive claim to speak for the health-policy community. It may well be that this very fact helps account for the somewhat lower level of acrimony characterizing health politics. Indeed, in an earlier era, when the AMA did largely dominate the full agenda of health issues, the domain seems to have been considerably more conflict ridden (Starr 1982). Analogies to superpowers in international politics may be farfetched, but it is not implausible to argue that the existence of

superpowers tends to polarize conflict and that polarization tends to lead to intensification.

Other points of interest in Table 5 include the fact that academic and hospital groups are especially reluctant to name any adversaries. Veterans, a group category not included in the table, carry this strategy to perfection; they claim to have no organized group opponents at all. The congressional politics of health issues is often strenuously contentious, of course, but what these data indicate is that the conflict is not so much among competing interests represented by adversary organizations as with the more amorphous forces of budget constraints and administration priorities. Group politics is an important, sometimes even decisive, part of the overall policy-making process; but it is seldom, if ever, all one needs to know to explain policy outcomes.

We turn now to the labor-policy do-

Table 6. Allies and Adversaries in Labor

Nominee Organization	Nominating Organizations				
	Labor Peaks	Unions	Citizens Groups	Trade Associations	Business Peaks
Ally					
Labor peaks	2	32	8	2	0
Unions	9	37	11	11	2
Citizens groups	15	17	30	3	4
Trade associations	0	1	5	54	15
Business peaks	0	0	5	30	42
Number of respondents	10	35	26	43	23
Adversary					
Labor peaks	0	0	6	20	22
Unions	1	3	3	20	12
Citizens groups	4	24	24	2	4
Trade associations	1	11	5	7	0
Business peaks	15	41	17	4	2
Number of respondents	10	33	24	33	21

main. Here it is plain that we encounter a bipolar conflict structure. The peak associations of labor and business dominate the poles, and there is almost no trafficking with the other side. Individual unions and trade associations are slightly less implacable foes. The latter, especially, have worked out some alliances with the unions. Citizens groups in this domain are a diverse lot, primarily aligned with labor but including a significant number of anti-labor groups, producing substantial intra-category conflict. One revealing feature of Table 6 is that in the conflict-charged climate of labor policy, unlike the other three domains, nearly every respondent was willing to name adversaries as well as allies.

These findings confirm our data regarding the characteristics of issues, where the labor domain evidenced the highest levels of conflict and partisanship. The ally-adversary structure mapped in Table 6 is congruent with the perceptions of labor issues. It is also confirmed by the patterns of event-specific activity reported by our respondents. Groups' relationships to one another in respect to what positions, if

any, they had taken on particular policy issues revealed a clear bipolar structure on labor issues. In the other domains, the event-triggered patterns were segmented along lines essentially similar to the organizational groupings identified in Tables 3-6. (For further discussion of these events-based data see Laumann et al. 1986.)

Conclusions

We began by suggesting that the interest-group universe could be regarded as a system of structured conflict (and cooperation). Our data suggest that this is certainly valid at the subsystem, or policy-domain, level (though we are really not able as yet to portray or characterize in any detail the complete "pressure system," to employ Schattschneider's [1960] convenient, if misleading, term). Domain subsystems have been seen to have relatively stable patterns of interaction, quite sharply ideological and bipolar in the labor-policy domain but fragmented, primarily among sets of

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specialized producer organizations, in the other areas.

Specialized producers, with relatively narrow policy agendas, tend to avoid becoming embroiled in adversarial encounters. As becomes the protagonists in a system of distributive politics, they try instead to confine their efforts to building whatever support they can for their primary policy goals. These live-and-let-live efforts are complicated in some domains by the presence of peak associations, organizations that seek to transcend the limited membership potential of specialized groups like trade associations and to formulate broader policy agendas as they try to speak persuasively on behalf of an entire economic sector. Peak associations tend to express their policy goals in the more encompassing language of doctrine and ideology, using abstraction to rise above the tangible differences of interest among more specialized producers.

The more prominent and unified the peak associations in a policy domain, the more polarized its group structure and policy struggles become. The limiting case in our data is labor, where the peak associations of business and labor do battle and pull the more specialized groups into one orbit or the other. In agriculture, by contrast, the peak associations, once the basis of cohesive bloc politics, are themselves divided. Farm policy-making tends now to be dominated by narrower and often shifting coalitions and to be articulated with less partisan or doctrinal fervor than was the case, say, in the 1950s.

In each of these policy domains, the role of externality groups—environmentalists, consumers, and other “citizens” aggregations—is a kind of mirror image of the peak associations. Where, as in labor, the latter are powerful, the externality groups are diverse, divided, and often serve as satellites to the “superpowers.” Where peak associations are divided or nonexistent, the externality in-

terests provide the principal focus of opposition, perceived as getting in the way of the realization of specialized producer interests (see also Schlozman and Tierney 1985, 285-86). Externality groups often like to think of themselves as the guardians of the “public interest” against the diverse claims of multifarious “special interests.” With or without that normative language, we find that this structure does indeed characterize three of our four policy domains.

Our interpretation of the role of peak associations in structuring interest-group conflict moves in a very different direction from the principal lines of argument regarding societal corporatism and from the widely discussed contention of Mancur Olson (1982) regarding the significance of encompassing groups. In the corporatist model, effective peak associations of labor and business are necessary elements along with the state in a tripartite structure of cooperative negotiation (Schmitter and Lehmbruch 1979). The singularity of the United States in failing to develop along these lines has been examined elsewhere (Salisbury 1979; Wilson 1982). Our data suggest that even when we do find reasonably effective peak associations, group relations are more rancorous, not more cooperative.

Olson’s argument also would lead one to expect that the more encompassing the group (i.e., the more effective the peak associations), the more efficient (i.e., less dominated by narrowly focused producer interests) the policy decisions. The finding that intense conflict develops when the group politics moves from a fragmented, “special” interest-dominated pattern to one in which more broadly encompassing groups are the main players, does not seem compatible with most notions of efficiency. There may, of course, be a stage beyond bipolar group politics in which a single encompassing “group” is able to accomplish fully Pareto-optimal results, but at that point it would appear that we had

passed beyond the sphere of democratic politics.

We believe that our findings are relevant to the ongoing debate concerning the validity of pluralist models of U.S. politics. It is obvious that our design has been premised on the assumption that a multiplicity of groups compete in meaningful ways for the rewards at stake in public decisions. But the patterns of group action we have reported go well beyond that. In his classic study of New Haven, Robert Dahl (1961) rests much of his pluralist interpretation on the discovery that business interests (and other interests also) pursued agendas that stopped far short of the full range of public issues and policy decisions. Consequently, their resources, though imposing, were often not brought into play. In his more recent statements, characterized by a considerable degree of recantation, Dahl (1985) and his colleague Lindblom (1977, 1983) place less emphasis on the theme of selective participation as the crucial factor limiting power and protecting pluralism. They stress instead the structural distinctiveness of business interests and the inherent political advantage derived therefrom.

Our data probably cannot illuminate the debate regarding the more abstract properties of business power in a capitalist order. They do show, however, that there is great variety in the forms of politically active business interests (as well as of labor, farmers, professional groups, and others), and that the variety of form is systematically related to the agendas and strategies of action. Trade associations and specialized producers are not simply smaller, narrower segments of a larger class or sector. They often have different goals and operate in different ways, not as anomalies or deviations from some class-defined normalcy but through rational strategy calculated to advance genuine interests.

If, as we have found, the substantive

policy domain is a dominant factor in determining the way interest representatives perceive the political context in which they operate and if, further, the structures of domain conflict and cooperation are built from the particular configurations of interest organizations that participate in domain politics, it follows that an adequate understanding not only of interest-group politics but of the full policy process requires us to keep a careful descriptive watch on the group composition of a domain. Unless we know who the organizational players are, how they relate to one another, and what dynamics of organizational development and change are at work, we are unlikely to secure an adequate analytical grasp of the situation. Neither research monographs nor beginning texts should continue to talk about "the farm bloc," the dominant role of the American Medical Association, or the unity of "business" interests when events have long since rendered those empirically invalid. Our effort to describe the structures of conflict and cooperation among organized groups may thus be taken as a contribution to an ongoing obligation to attend systematically to the *substance* of interest-group politics.

In the end, of course, we must be concerned not only with the structures of interest-group relations but with their policy outcomes as well. What is the reflection in substantive decisions of the fragmented politics of agriculture or the bipolar conflicts over labor issues? Does a "play-it-safe" trade association accomplish more of its objectives than a contentious citizens group? In the next phases of our research, we hope to be able to speak to these questions. Meanwhile, however, showing that interest-group interactions display patterns interpretable in their own terms and comparable in structure to those in other realms of political analysis provides an avenue of potential linkage to the broader bodies of theory on

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which a coherent discipline of political science must rest.

Appendix: Methods of Sampling Client Organizations

We arrived at our sample of client organizations by drawing on four sources of nomination that could measure levels of policy-making activity (Shapiro 1984). Our first method for locating interested actors in a policy domain was especially attentive to issues attracting mass-media attention because of their controversiality and broad popular interest. Such issues are especially likely to appear in the congressional arena. Organizations taking partisan public stands on popular issues are especially likely to be identified by this technique. We conducted a computerized search of stories in national and regional newspapers and magazines dealing with federal policy-making in each domain from January 1977 to June 1982, noting the number of stories mentioning each organization. The data regarding newspaper coverage of organizational participation were compiled from the Information Bank, a data base of the New York Times Information Service (NYTIS). To compensate for regional effects, the data base included the *Chicago Tribune*, *Houston Chronicle*, *Los Angeles Times*, *New York Times*, *San Francisco Chronicle*, *Seattle Times*, *Washington Post*, and *Time* magazine. The product of the computer search was an extensive set of news-story abstracts from the source newspapers. These abstracts were then searched and tagged for the names of organizations, and lists of organizations in each domain were compiled. The number of mentions for each organization was recorded, a mention being defined as an appearance in one newspaper abstract. Duplicate abstracts of stories reported in two or more news sources were eliminated.¹

Second, we searched the abstracts of congressional hearings held by committees and subcommittees with jurisdiction in each of the four domains during the 95th-97th congresses, noting the number of hearings at which organizations testified. Less publicly partisan and more specialized organizations are revealed by this procedure. In light of the enormous number of hearings covered in the Congressional Information Service (CIS) database, we restricted the search to the first sessions of the 95th, 96th, and 97th congresses and to select major committees active in each domain.

Third, during July 1982, we interviewed 20-23 government officials in various policy-making positions in each domain in the Congress and the executive agencies, noting the number of officials mentioning an organization as one that frequently contacted them or which was representative of organizations that contacted them episodically. This method is especially likely to identify organizations having direct dealings and concerns with particular executive agencies and their regulatory initiatives. Two criteria were used in selecting individuals to be interviewed: (1) the position of the individual in the unit and (2) his or her tenure in the unit. An attempt was made to avoid relying exclusively on politically appointed officials or on those with less than two years' tenure in office.

Finally, for each domain we compiled a list of organizations appearing under the industry headings raised to the domain in *Washington Representatives* (1981), an annual publication that canvases various public sources and surveys organizations in an effort to list organizations represented before the federal government. Organizations that are self-professed lobbyists before Congress and the executive agencies are especially likely to be identified.

Each source method of nominating organizations for inclusion in the popula-

tion of claimant organizations has distinctive features that sets it apart from the others (Salisbury 1984). If we had relied only on the listing produced by the *Washington Representatives*, we would have underestimated the population of interested organizations by more than half in each domain. (The percentage of organizations not contained in the *Washington Representatives* directory was 70.4% in agriculture, 52.8% in energy, 67.1% in health, and 80.3% in labor.) Domains differ substantially among themselves with respect to the amount of newspaper attention given them, the raw numbers of organizations identified as active, and the number of organizations engaged in repeated efforts to influence policy outcomes in their respective domains. The energy domain seems to have the largest number of organizations attracted to its concerns, with agriculture and health roughly tied in scale of participation with almost a third fewer organizational participants, and labor placing a rather distant fourth. While the CIS ratios of the number of mentions to the number of organizations mentioned are remarkably stable across the four domains, the NYTIS ratios vary considerably across domains. There is a high ratio in energy, for example, suggesting the relative prevalence of "repeat players" in the domain.

For each domain we drew a random sample of one hundred organizations, with each of the four sources contributing equally, but with each organization in the first three sources weighted by the number of mentions in that source. Because, for all but the listing from *Washington Representatives*, the probability of selection increased with the number of mentions, our sampling procedure reflected each organization's level of activity in the domain. We had strong reasons to adopt this weighting procedure. Knoke and Laumann (1983) had demonstrated that there was a high corre-

lation (.75 and .72 for the energy and health domains, respectively) between the number of organizations active in a domain that named an organization as among the most influential actors in the domain and the number of mentions in newspaper stories, appearances in congressional hearings, and mentions by government officials. Hence, there was solid evidence for treating the number of mentions in the various sources as a measure of perceived organizational influence. The sampling procedure thus generated a list of organizations to be interviewed that disproportionately included the more influential and active organizations in the domain, while also allowing the selection of less influential and active organizations. Simple inspection of the samples reveals that we were quite successful in including many of the most prominent and influential organizations, with an admixture of less visible, more peripheral organizations. This mixture of organizations will permit us to investigate what, if any, systematic differences are to be observed in modes of participation in policy deliberations across different types of organizational actors.

Having sampled the client organizations, we then identified the representatives they employed or retained by conducting telephone interviews with client informants in a minimum of 75 organizations in each domain, 316 organizations in all.² These informants were identified by using published listings of organization officers, supplemented with direct inquiries of the organizations to determine who had operating responsibility for the organization's involvement in federal policy in the appropriate domain. The informant was asked to name up to four individuals inside the organization (employees, officers, member volunteers) and up to four individuals external to the organizations (employed in outside firms, trade associations, and so forth but not falling within the definition of an internal

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representative) who acted as key representatives for the organization in the policy area. It is this listing of nominated individuals that constituted the population of representatives.

The client interviews generated between 400 and 450 names of representatives in each domain, for a total of 1,716 individuals. (Representatives could appear on the lists as many times as they were mentioned; about 5% appeared in more than one domain.) Random selection from these lists produced samples of 257-61 representatives per domain, with a realized sample ranging from 192 to 216 across the four domains. The overall response rate of representatives was 77%. About 10% of the representatives contacted declined interviews; the rest could not be scheduled for various reasons. Slightly over two-thirds of the respondents were based in Washington and were personally interviewed there. Another 15%, based in other major cities, also were interviewed personally. The remainder of the sample was interviewed by telephone using an adapted format.

The final set of actors to be identified were the government officials most often the targets of representational activity in a given domain. These target government officials (including those holding elected, appointed, and career positions) were identified by asking the representatives we sampled to give the names and positions of "the five government officials or staff members you contact most often in the course of your work" in the appropriate policy domain. From these lists we sampled from 101 to 108 names in each domain, and successfully interviewed about 72%. About 8% of the government officials declined to be interviewed.

Notes

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1. For a presentation of the specific data see Nelson et al. 1987.

2. The response rate was 78% with 10% of the organizations refusing interviews. Another 12% of the organizations could not be located, were located overseas, or had ceased to exist. Refusals did not follow any discernible pattern and were not regarded as a significant source of bias.

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NATIONALIZATION AND PARTISAN REALIGNMENT IN CONGRESSIONAL ELECTIONS

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The relationship between nationalization of the U.S. electorate and partisan realignment is explored. Concepts and measurement of nationalization are examined, as well as definitions of electoral change. The British concept of swing is utilized as an appropriate measure of electoral change. Examination of long-term trends in the variances of the congressional vote and swing from 1842 to 1980 shows they are related to the electoral dynamics of realignment. Analysis utilizing a variance-components model shows there has never been a nationalization in terms of configuration of the electorate. But nationalization in the movement of the electorate has taken place cyclically, corresponding to the partisan realignments of the 1890s and the 1930s, rather than monotonically, as suggested by previous research.

One of the major problems of U.S. political science has been to identify the various forces working to shape partisan votes in the electorate and to chronicle their change over time. Among the most important efforts has been Donald Stokes's model, which partitioned the variance of the congressional vote into national, state, and district components (Stokes 1965). Stokes found that since the Civil War era there has been a growing nationalization of the electorate in the United States (Stokes 1967). He attributed this nationalization to the changing structure of mass communication.

During the two decades since its appearance, only two works have directly assessed the adequacy of Stokes's method (Claggett, Flanigan, and Zingale 1984; Katz 1973). Claggett, Flanigan and Zingale questioned the validity of Stokes's conclusion by showing that the estimated "national component," which Stokes did

not present in his paper, has not grown for a century (1984, 79, n. 1 and Fig. 2). Reanalyzing the question, they claimed to use "basically the same methodological approach to the problem" as Stokes (p. 84). However, they changed handling procedures originally set by Stokes, using county-level election returns instead of district-level data. They used regions instead of states as an intermediary aggregate level and extended the period of analysis to include the decades from the 1840s to the 1960s. In this reanalysis they found no evidence of growing nationalization.

That Claggett, Flanigan, and Zingale reached different conclusions from Stokes is intriguing because they felt sure their measure of the national effect was "strictly comparable to the 'national effect' in Stokes's analysis" (p. 84). Yet the results associated with the second measure in their paper are quite different from those associated with the first. (Compare

Stokes's estimated national effect with their national effect in Claggett, Flanigan, and Zingale 1984, Figs. 2, 4.) Claggett, Flanigan, and Zingale did not refer to this discrepancy at any point. In light of such discrepancies, their statement that the "differing conclusions rest on examination of additional and more appropriate data, not on any variations in method that may have been introduced" (p. 89), is not persuasive. It seems that Claggett, Flanigan, and Zingale cast serious doubt on the validity not only of Stokes's argument but of their own as well, for valid methodology applied to similar data should produce similar results.

Not only has some doubt been cast on the reliability of Stokes's conclusions, but his findings do not mesh very well with other literature on the U.S. party systems (see Niemi and Weisberg 1976 for a similar observation). There were two partisan realignments in the period Stokes analyzed: the realignment following the election of 1896 and the realignment of the New Deal era. Yet these political cataclysms do not seem to have affected the constant process of nationalization that Stokes identified. A natural question is, What has been the relationship between the degree of nationalization of the U.S. electorate and partisan realignments?

Electoral analysis of realignment has focused mainly on presidential elections and has tried to identify the breakpoints in partisan support in a series of elections (Key 1955). Therefore, the national partisan vote has been the center of attention. This approach has led to the refinement of critical realignment theory by identifying distinct stages in party systems (for example, see the t-score analysis in Burnham 1970). But there are some problems. To focus on the national partisan vote deflects our attention from the diverse nature of U.S. politics. The national partisan vote is a summary measure of votes cast in various districts within states. Therefore the national "cutting points"

marking transitions between party systems were the outcomes of complicated processes at the subnational levels (see, for example, a study of realignment in New York state in Benson, Silbey, and Field 1978). The nationalization approach Stokes initiated can be useful in analyzing this aspect of realignment.

A recent article by David Brady (1985) suggests one clue for the application of the nationalization approach to the analysis of realignment. In an effort to redirect our attention to a policy-oriented approach to realignment, Brady has proposed a theory of policy change. One of his propositions is of particular interest here because it posits a link between nationalization and realignment. In periods of realignment, he has written, "the election results were more national, less local" (p. 30). In order to test this proposition, he checked the coefficients of variation of the aggregate congressional-vote swings. He found that coefficients of variation tended to be smaller during a realignment period than at other times, indicating that across-the-board movement of the electorate was more prevalent at such times. This analysis builds upon Brady's insight and approach, showing that the degree of nationalization in elections is related to the dynamics of realignment, and presenting a model appropriate to analyzing this relationship.

Concepts of Nationalization

Previous research on the nationalization of the electorate has sometimes been ambiguous conceptually or methodologically about exactly what has been nationalized. For example, Stokes (1965, 1967) was concerned with the political levels at which forces moving the electorate arise. Stokes (1965) initially partitioned the total variance of the *change of voting* into between-state and within-state (district-level) variances (Stokes 1965,

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63-65). Yet when he extended his model to incorporate the national term, he replaced the *change of voting* with the *vote itself*, over a series of elections. Stokes argued as if the transition from the first model to the second were a simple extension of his methodology. Yet it was in fact a transformation of his model and perspectives, for the two variables Stokes employed in the two models are conceptually distinct.

Claggett, Flanigan, and Zingale (1984) clarified the two concepts of nationalization. They identified one type of nationalization as convergence in the levels of partisan support. This concept is related to the partisan vote at each district. If reaction to the national party plays the most important role in an election and the composition of the electorate in most districts is similar, then it is probable that every district will show a similar level of partisan support. The fortune of a party candidate in one district will be closely connected to that of the party's other candidates across the country. Nationalization as convergence refers to the homogeneity of the electorate. I will use the term *configuration* to indicate this attribute of the electorate.

The electorate with a nationalized configuration is one that shows few regional and district differences in partisan support. But one of the important characteristics of U.S. electoral politics is that local factors have always played a significant role. It cannot be hypothesized that the U.S. electorate was truly nationalized at any time in history. The traditional existence of regional difference in partisan support, clearly seen in presidential elections, militates against the existence of a nationalized electorate (Burnham 1981a, 1981b). Moreover, the decline of competition during the last 20 years in districts where congressional incumbents are seeking reelection suggests that there has been, if anything, *denationalization* in recent decades (Mayhew 1974).

The second concept of nationalization is that of a uniform response to political forces. This concept considers the *movement* of the electorate as distinguished from its basic configuration. Claggett, Flanigan, and Zingale described this aspect of nationalization as a "situation in which all subunits are responding with a uniform surge toward (or away from) a particular party" (1984, p. 82). Thus districts may show concordant movement even though they are not homogeneous in configuration. If there is the same direction and amount of electoral change in every district, then the electorate is perfectly nationalized in movement. Stokes was only concerned with nationalization as a uniform political response, not as a uniform configuration.

Measurement of Electoral Change

In order to assess the degree to which the nation's vote is nationalized in configuration, I will analyze how the partisan vote is dispersed at different levels of aggregation. As to nationalization in the movement of the electorate, one must turn to some indexes of electoral change.

One of the influential definitions of aggregate electoral change in the U.S. electoral-research literature is that of Flanigan and Zingale (1974).¹ They elaborated on types of electoral change, paying attention to the uniformity and the persistence of change. But their basic idea was to define *aggregate electoral change* as "movement away from normal voting patterns" (p. 56). The normal voting pattern was operationalized as the grand mean of the array of elections. It was assumed that "the fluctuations in the actual vote over a period average out to an approximation of the underlying partisanship of the electorate" (Clubb, Flanigan, and Zingale 1980, 89). The vote was considered to move away from the underlying grand mean at each election.

Defining *change* as movement away from the normal voting patterns creates some difficulties. First, the normal vote pattern should be estimated somehow, which is hard to operationalize in electorally turbulent years. The second difficulty arises with the use of this electoral change in the analysis of variance techniques; that is, if the amount of electoral change in one year is measured by the deviation of the vote from its over-time mean, then it follows that the amount of change in the entire set of elections analyzed may be measured by the over-time variation of the vote (see Flanigan and Zingale 1974, 76-77, n. 5). Thus, Claggett, Flanigan, and Zingale (1984) employed the over-time variation of the national mean (normalized election sum of squares) to measure national effect as a uniform response. Stokes's (1965) national component is basically the over-time variance of the national mean vote. I do not think this is an appropriate way to measure electoral change.

Let me illustrate the problem by an example, where a series of elections yields 40%, 40%, 40%, 50%, and 55% for a certain party. The support for this party is the same for the first three elections, and then it increases monotonically. The over-time mean of the vote is 45%. The amount of change in the period is measured by the over-time variation of the vote, that is, 3.5 according to Claggett, Flanigan, and Zingale's method. Consider another series of elections where one party gets 40%, 50%, 40%, 55%, and 40%. The support for this party is quite unstable in the period. A significant portion of the electorate changes partisan allegiance at each election. However, this set of elections has the same amount of over-time variation as that of the previous one, for only the sequence of the values of the vote is different between the two sets. The above examples clearly show that the sequence of elections does not affect the over-time variation of the vote. But

change is a time-related concept. I think that the sequence of elections should be taken into account when measuring change.

I suggest defining *change* as the difference between two electoral results. My preference is grounded both on the simplicity of the definition and the similarity of this definition to the British concept of *swing*. British scholars define *swing* as the average of the Conservative percentage gain and the Labour percentage loss (Butler 1951; see also Steed 1965). If there is no strong third party to be counted, then *swing* will simply be the Conservative gain, for the gain of the one party is the loss of the other. Thus *swing* is equivalent to my definition of electoral change for one party when there is no third party. *Swing* captures net aggregate movement of the vote from one election to a second one. Stokes (1967) and Brady (1985) also utilized swing in their analyses. Hereafter, I will use the expressions *my measure of electoral change* and *swing* interchangeably because of their similarity. They mean the partisan vote percentage in each district in one election minus the partisan vote percentage in the previous election.

By using swing, one can measure the amount of change for the period in the above examples more appropriately. Two points should be made clear beforehand. First, variance is a measure of dispersion. It measures the degree to which values differ from each other. Thus, the over-time variance of the vote does not measure the amount of change. It rather measures the *configuration* of the vote within the period analyzed, that is, how each election is distinct from others in the period. Second, in order to capture the electoral change for the period, I will look at both the mean and the variance of swing over time. The over-time variance of swing measures the dispersion of swing in the period analyzed.

For the first series of elections in the above example, the swing is 0%, 0%,

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Table 1. Variance of the Democratic Vote across Districts

Year	Variance	Number of Cases	Year	Variance	Number of Cases	Year	Variance	Number of Cases
1842	94.4	181	1888	155.1	304	1934	204.1	339
1844	94.8	147	1890	205.4	314	1936	281.7	373
1846	121.0	180	1892	157.8	329	1938	260.9	341
1848	242.9	201	1894	223.2	334	1940	271.1	351
1850	127.8	175	1896	210.5	257	1942	227.7	327
1852	185.6	206	1898	281.8	294	1944	280.8	367
1854	161.1	153	1900	286.5	317	1946	240.9	354
1856	218.0	209	1902	315.9	327	1948	238.0	353
1858	212.6	221	1904	367.1	342	1950	213.4	340
1860	118.4	145	1906	345.0	324	1952	194.5	343
1862	159.3	163	1908	265.5	348	1954	194.9	350
1864	96.4	164	1910	331.6	327	1956	180.5	358
1866	150.7	172	1912	329.8	353	1958	204.9	344
1868	163.0	217	1914	385.0	351	1960	191.0	360
1870	144.6	225	1916	309.4	347	1962	211.6	380
1872	136.6	269	1918	239.0	308	1964	199.2	394
1874	139.0	242	1920	375.3	364	1966	257.2	386
1876	135.5	285	1922	361.9	357	1968	262.4	390
1878	291.9	271	1924	412.3	355	1970	303.5	386
1880	152.1	278	1926	431.6	338	1972	302.8	389
1882	189.0	292	1928	274.2	360	1974	278.4	382
1884	133.2	298	1930	350.3	328	1976	315.1	401
1886	204.8	286	1932	287.4	344	1978	337.0	359
						1980	380.1	369

Note: Districts where Democratic candidates earned more than 98% or less than 2% of the total vote were excluded from the analysis.

10%, and 5%. The over-time mean swing is 3.75%, and the variance of swing is 17.2. The second series has greater swings: 10%, -10%, 15%, and -15%. The over-time mean swing is 0% and the variance is 162.5. The over-time mean indicates that there is an increase in the party support in the first series but not in the second. But the variance of swing indicates there is more change in the second than in the first.

Variance of the Vote across Districts and Variance over the Decade

The first step in my strategy for analyzing configuration and movement in U.S. congressional elections is to show the variances of the Democratic vote and

swing. Table 1 shows the variance of the Democratic vote across districts in each congressional election since 1842. Every contested result available was used for the calculation.² The variance shows the degree to which there was diversity in partisan support. A small amount of variance means that the nation was relatively homogeneous in the level of support the Democratic party received. The larger the variance, the more diverse the level of partisan support in the districts.

There are two types of change in the variance. First, there is short-term fluctuation lasting only several years. For example, the amounts of variance went up and down slightly through the 1940s. One reason for this fluctuation was the change in the number of contested districts. When there have been many contested districts, variance has tended to be

large; smaller variance has often been associated with a smaller number of contested districts. Generally speaking, mid-term elections tend to have smaller numbers of contested districts because congressional aspirants do not expect to benefit from the coattail effects of presidential candidates. Thus the decisions of congressional aspirants seem to affect the amount of variance.

Second, there are long-term trends in the amount of variance. Table 1 shows that variance decreased around the Civil War and then increased, reaching its highest point in the 1920s. Variance declined in the 1930s for the first time since the Civil War. It hit bottom in the 1950s and again increased over the last 20 years.

One might posit that the long-term trend can be explained by changes in the number of contested districts too. For example, the growth in the amount of variance from the 1860s to the 1920s might have been caused by the growth of the nation and the consequent increase in the number of contested districts. But the trend after the 1930s militates against this explanation, for the decrease in the amount of variance did not accompany a decrease in the number of contested districts.

Thus the long-term trend probably really reflects changing diversity in Democratic support across districts and states. There was a trend toward greater variance through the Reconstruction era, and the realignment of the 1890s accelerated it. The regional cleavage that ran across the nation at that time was between the northeast metropolitan industrial area and the South and West. The Northeast was growing progressively more Republican, while the South and West were becoming more Democratic, producing ever higher variance. One should expect the realignment of the 1930s to have had a strong impact on the configuration of the electorate, because to some degree U.S. citizens divided politically along class

lines. This greater class polarization weakened regional bases of political parties, producing smaller variance.

The trend from the 1950s to the present needs special attention, for congressional elections have become increasingly insulated from presidential elections (Burnham 1975b; Mayhew 1974). In his analysis of presidential elections, Kleppner has shown that the variance across regions of Democratic longitudinal means for 1894-1930 was 136.08, while that for 1932-72 was 39.93 (Kleppner 1981, 19, Tbl. 1.2). Moreover, the variance for 1932-60 was 73.59, while variance for 1960-72 was only 10.73. Judging by Kleppner's analysis, presidential elections have become increasingly nationalized in recent decades. In light of this, the growing variance in congressional elections over the past 20 years indicates an insulation of congressional votes from the national political scene.

To further analyze the degree of nationalization in elections over time, I have calculated *decadal variance* to show the configuration in each decade. Decadal variance is calculated by using the partisan vote in each district over five consecutive elections after redistricting. It measures the degree to which the vote is dispersed from the grand mean of the whole decade. This differs from the variance across districts just displayed. That variation is the deviation of the district vote from the national mean in each election year. In decadal variance, the variation stems both from the deviation of the national mean in each election year from the grand mean of the decade and from the deviation of the district vote from the national mean. Thus by using decadal variance I bring the national variation into consideration of configuration. But although it is possible to distinguish national variation from sub-national variation conceptually, it is not so easy to operationalize, since the national partisan vote is the outcome of

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Table 2. Indicators of the Democratic Vote and Swing

Decade	Decadal Variance of the Vote	Range of Variance of Swing	Mean of Variance of Swing	Decadal Variance of Swing
1840s	141.7	156.5	134.2	151.1
1850s	185.8	27.9	115.7	127.9
1860s	147.8	106.5	64.1	74.4
1870s	179.1	110.4	124.2	141.2
1880s	180.3	51.4	98.5	101.3
1890s	242.5	14.6	61.6	87.7
1902-10	326.5	27.6	56.2	67.6
1912-20	335.3	29.0	73.6	87.4
1920s	370.9	52.4	77.8	89.9
1930s	271.1	24.6	46.7	53.4
1940s	250.1	40.4	46.1	67.6
1950s	199.7	15.9	33.8	50.6
1960s	252.1	21.0	65.4	85.0
1970s	326.5	69.2	123.6	137.3

various votes counted at district level. It is simply an aggregated value, and it reflects more than a uniform or "national" element. Thus, variance of the national mean from the grand mean of the decade reflects both the national variation and the subnational variation. In order to measure the amount of national variation, one has to remove the part that reflects the subnational variation.

In Table 2 the decadal variance of the Democratic vote since the 1840s is shown. One can find almost the same secular trend as that shown in Table 1: the increase in variance from the 1860s till the 1920s, followed by the decrease for the next three decades and then the recent increase.

Variance of the Swing across Districts and Variance over the Decade

Brady (1985) calculated the mean swing and the variance of swing to analyze electoral change in prerealignment, realignment, and postrealignment periods. Brady's choice of elections for the calculation of swing seems arbitrary, in that dif-

ferent time periods went into the calculation. In order to avoid this arbitrariness, I decided to use a fixed time period of two years for calculating variance of my measure of electoral change, in the manner of Stokes (1967), and I expanded his analysis to include 1842-1980.

Table 3 shows variance of the Democratic swing across districts from 1844. The values for the first years after each reapportionment are not shown because the change in district boundaries makes the calculation unreliable. Every contested pair of district election results was utilized for the calculation. This procedure is almost the same as the one Stokes (1967) used in his analysis, and I obtained similar results to his for the 1950s.

A pattern is not so easily discernible as it was with variance of the vote itself. But again there are two types of change. One is the short-term fluctuation over a few years' time. The movement of the electorate was in no way uniform across districts, and the degree to which local swing diverged from the national mean swing fluctuated. This fluctuation was the result of various local responses that sometimes coincided by chance to produce a rather

uniform response and thus small variance. But these usually did not amount to a nationally uniform movement toward or away from one party. Examples of this type of fluctuation are abundant in Table 3 where no two adjacent figures are the same.

The second change involved longer time periods. Variances of the swing in one decade fall within some range, and thus the decades show certain levels in the size of variances. From Table 3, I calculated the *range* of values in each decade (see Table 2). In the 1840s, the 1860s, and

the 1870s the range was larger than 100. But in the rest of the decades, the range was rarely larger than 50. Interestingly, one finds smaller range in the 1850s, the 1890s, the 1930s, the 1950s, and the 1960s than in other decades.

Then I calculated a *mean* of the variances of the swing over each decade (see Table 2). The mean hit bottom three times in 140 years. The first was in the 1860s, just after the realignment of the 1850s. The second time was in 1902-10, just after the realignment of 1896. The third was in the period from the 1930s through

Table 3. Variance of the Democratic Swing across Districts

Period	Mean Swing	Variance	Number of Cases	Period	Mean Swing	Variance	Number of Cases
1842-44	.4	61.3	125	1912-14	-.9	78.5	314
1844-46	-3.0	64.2	129	1914-16	1.4	62.2	312
1846-48	-2.7	193.4	151	1916-18	-.5	62.4	266
1848-50	5.0	217.8	154	1918-20	-8.4	91.2	288
1852-54	-4.3	127.7	129	1922-24	-5.6	65.8	324
1854-56	5.6	101.0	139	1924-26	2.4	49.5	316
1856-58	.7	128.9	200	1926-28	-.2	93.8	316
1858-60	-1.1	105.2	140	1928-30	3.6	101.9	308
1862-64	-4.2	57.0	141	1932-34	.3	62.6	294
1864-66	.7	45.3	156	1934-36	.9	43.8	332
1866-68	2.7	23.7	171	1936-38	-5.4	38.0	335
1868-70	1.1	130.2	213	1938-40	1.0	42.4	328
1872-74	5.0	115.8	215	1942-44	2.1	73.5	316
1874-76	-.8	67.0	232	1944-46	-4.9	40.7	337
1876-78	-5.7	177.4	256	1946-48	7.2	36.9	329
1878-80	2.2	136.7	251	1948-50	-2.5	33.1	319
1882-84	.1	120.2	269	1952-54	4.4	27.1	323
1884-86	.6	115.0	264	1954-56	-2.1	27.5	335
1886-88	-.8	90.0	267	1956-58	6.8	37.5	328
1888-90	4.1	68.8	287	1958-60	-2.9	43.0	329
1892-94	-7.9	55.4	317	1962-64	4.6	54.4	361
1894-96	1.7	56.7	252	1964-66	-6.6	65.6	360
1896-98	6.1	70.0	240	1966-68	-.5	66.1	361
1898-1900	-.6	64.2	284	1968-70	3.6	75.5	368
1902-04	-5.5	68.3	311	1972-74	6.1	166.2	354
1904-06	3.2	45.2	302	1974-76	-1.2	97.0	361
1906-08	-.1	41.9	317	1976-78	-1.3	124.7	337
1908-10	2.2	69.5	318	1978-80	-3.8	106.5	335

Note: Districts where there was no competition in either of a pair of elections were excluded from analysis for these years.

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the 1950s, when the New Deal realignment and its aftershocks spread over the country. Students of U.S. party systems will easily arrive at a hypothesis about the relationship between these changes in variance and the electoral dynamics of realignment.

Next I will turn to the decadal variance, as I did in the analysis of configuration of the vote. As mentioned earlier, decadal variance incorporates national variation and subnational variation. If the decadal variance of swing is small, then it is likely that the movement of partisan support is uniform across districts and over a decade, suggesting the existence of nationalization.

Table 2 shows the decadal variance of swing for the Democratic party. It shows similar trends to range and mean of the variance of swing in each decade. The decadal variance of swing hit bottom in the first decade following a realignment (the 1860s, 1902–10, and the 1930s). And the decadal variance was small in the 1940s and the 1950s. With the reservation that decadal variance incorporates both the deviation of district swing from the national mean in each year and the deviation of the national mean from the grand mean of the decade, findings are neatly summarized by decadal variances.

A Model for the Assessment of Electoral Forces

The previous sections introduced decadal variances of the Democratic vote and swing, indicating that decadal variances are closely connected to the dynamics of electoral realignment. I would like to establish a model for the assessment of electoral forces. As explained above, the decadal variance is the mean of squares of the deviations from the grand mean. The vote and my measure of electoral change upon which decadal variance is based have three levels of aggregation:

district, state, and national. It is possible to partition the decadal variance into these components by a nested model of analysis-of-variance technique. By doing so, one may estimate how much of the variance is attributable to national, state, and district components.

My model can be described verbally as follows: *decadal variance* = *national variance* + *state variance* + *district variance*. *National variance* is the variance of the national mean from the grand mean, while *state variance* is variance of the state mean from the national mean, and *district variance* is variance of the district value from the state mean. But because the model is a nested one, means at each higher level of aggregation must be adjusted so that they do not reflect the variation at lower levels. Thus, one can estimate national means and state means only through the model.

The structure of my model resembles those of Stokes (1965) and Claggett, Flanigan, and Zingale (1984). But there are major differences: While Stokes used the difference between the vote percentage and its mean over the decade in each district in his calculation of decadal variance, I use the vote percentage itself to analyze configuration, and swing to analyze movement (see the Appendix for a further discussion and criticism of Stokes's model). Stokes's data corresponds to what Flanigan and Zingale (1974) called *electoral change*, because it is movement away from the normal voting patterns. For the reasons discussed above, I did not use Flanigan and Zingale's definition of *electoral change* in the analysis. Since the decadal variances of the vote and the swing are consistent with the understanding of realignment and electoral-history analysis, it seems appropriate to utilize them as the data for the nationalization analysis. The results indicate that this approach is fruitful.

A reader who is not interested in the statistical detail may skip the following

formal description of the model and proceed to the next section.³

To put the model formally, I first assume that the vote or the swing in the j th district of the i th state in the k th year Y_{ijk} can be described as a sum of four elements:

$$Y_{ijk} = M + A_k + B_{ik} + C_{ijk} \quad (1)$$

In the equation, M is a constant, which is the grand mean $Y_{...}$, and A_k , B_{ik} , and C_{ijk} are national, state, and district effects. It is also assumed that there are J_i districts in the i th state, that I states make up the nation, and that the results of K years are in the analysis. For the sake of explanatory convenience, I assume here that every district is contested in each year analyzed. A_k , B_{ik} , and C_{ijk} are assumed to have a mean of zero. Also, they are assumed to be uncorrelated.

Because the three random variables are independent,

$$\begin{aligned} \text{var}(Y_{ijk}) &= \text{var}(A_k) + \text{var}(B_{ik}) \\ &+ \text{var}(C_{ijk}) \end{aligned}$$

The left-hand-side expression is the decadal variance of the vote or the swing. The purpose is to estimate the three variances in the right-hand side of the equation.

By the method of analysis of variance, total SS of Y_{ijk} is partitioned into three SSs in the following way:

$$\begin{aligned} \Sigma_i \Sigma_j \Sigma_k (Y_{ijk} - Y_{...})^2 &= \Sigma_i \Sigma_j \Sigma_k (Y_{..k} - Y_{...})^2 \\ &+ \Sigma_i \Sigma_j \Sigma_k (Y_{i.k} - Y_{..k})^2 \\ &+ \Sigma_i \Sigma_j \Sigma_k (Y_{ijk} - Y_{i.k})^2 \end{aligned} \quad (2)$$

(A dot in the subscript of Y_{ijk} means that Y_{ijk} is to be averaged over the subscript that is replaced by the dot.)

Degrees of freedom associated with the terms in Equation 2 are $(K\Sigma_i J_i - 1)$, $(K - 1)$, $K(I - 1)$, and $K\Sigma_i (J_i - 1)$, respectively. Thus, the mean squares of

the three effects are described as follows:

$$MSA = \Sigma_i \Sigma_j \Sigma_k (Y_{..k} - Y_{...})^2 / (K - 1) \quad (3)$$

$$MSB = \Sigma_i \Sigma_j \Sigma_k (Y_{i.k} - Y_{..k})^2 / K(I - 1) \quad (4)$$

$$MSC = \Sigma_i \Sigma_j \Sigma_k (Y_{ijk} - Y_{i.k})^2 / K\Sigma_i (J_i - 1) \quad (5)$$

Next, I will express the expectations of the three mean squares by linear combinations of the variances of the random variables. They are obtained by substituting the right-hand-side expressions of Equation 1 for terms in Equations 3-5. For example, $Y_{..k}$ can be written as

$$\begin{aligned} Y_{..k} &= \Sigma_i \Sigma_j (M + A_k + B_{ik} \\ &+ C_{ijk}) / \Sigma_i J_i \end{aligned}$$

Hence,

$$\begin{aligned} E(MSA) &= \Sigma_i J_i \text{var}(A_k) \\ &+ (\Sigma_i J_i^2 / \Sigma_i J_i) \text{var}(B_{ik}) \\ &+ \text{var}(C_{ijk}) \end{aligned} \quad (6)$$

$$\begin{aligned} E(MSB) &= [1/(I - 1)] (\Sigma_i J_i \\ &- \Sigma_i J_i^2 / \Sigma_i J_i) \text{var}(B_{ik}) \\ &+ \text{var}(C_{ijk}) \end{aligned} \quad (7)$$

$$E(MSC) = \text{var}(C_{ijk}) \quad (8)$$

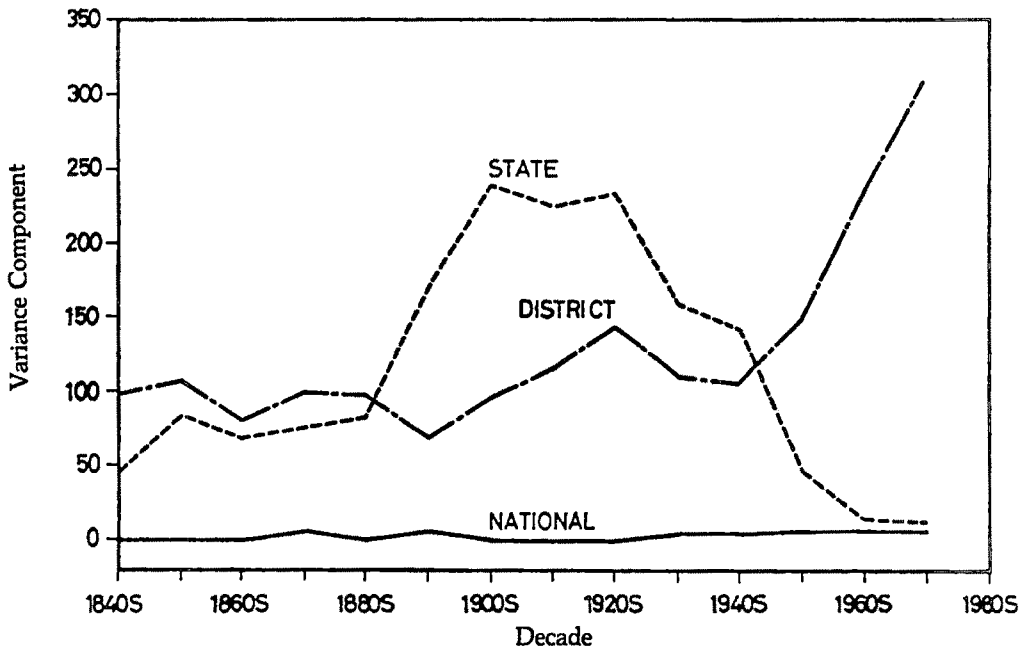
By equating the mean squares obtained by Equations 3-5 to the expected mean squares of Equations 6-8, we get the estimates of the three variances.⁴

Results—Configuration of the Electorate

Figure 1 shows the results of the analysis of the configuration of the Democratic vote from the 1840s to the 1970s. The lines represent the amounts of variance attributable to national, state, and district components. The sum of the three com-

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Figure 1. Configuration of the Democratic Vote



ponents is the decadal variance of the vote.

There has never been a nationalization of the U.S. electorate in terms of convergence. The national component explains very little, if any, of the total variance of any decade. This means that the national mean in one year was not much different from the mean in other years, after adjusting for differences at the district and state levels. But at the sub-national level, the configuration looks quite different. What emerges here is the changing shape of the U.S. electorate during the past century.

The United States had a fairly homogeneous electorate before the Civil War. In this period the total variance of the vote was evenly divided into state and district components. It seems that the Republican

realignment did not much affect the configuration of the electorate. Another analysis was conducted using the data that included uncontested districts, since the effects of a growing number of uncontested districts in this period were anticipated. The results showed an increase in the state component in the 1850s and the 1860s, indicating that the regional difference between the South and the North affected the contestation status of many districts.

The realignment of 1896 led to a further strengthening of the Republican party along a clear regional division: Northeast versus South and West. Figure 1 shows the impact of the realignment as the sharp increase in the state component beginning in the 1890s. A large majority of states ceased to be competitive, with one party

or the other establishing predominance around the turn of the century (Burnham 1981b; David 1972).

The New Deal realignment began to undermine the difference in the partisan vote across states from the 1930s. But it was not until the 1950s that the relative importance of state and district components was reversed. After the New Deal, states that had been predominantly Republican gradually became competitive, although predominantly Democratic states did not change much (David 1972). Beginning with the 1950s, Figure 1 indicates a dramatic increase in the district component along with a decay of states as a meaningful level of aggregation. This was caused by the convergence of party strength among states from the 1950s through the 1970s (Sundquist 1983, 344-48). Erosion of the Democratic predominance in the southern states, illustrated by a growing number of successful Republican candidates in statewide and national elections, was especially noteworthy (Ladd and Hadley 1978, 147).

The increase in the district component reflects another important change in the competitive structure of congressional elections. Mayhew (1974) showed that the shape of the distribution of partisan vote in the districts where incumbents were running changed from unimodality centering around 50% Democratic in the 1950s to bimodality centering around 35% and 65% in the 1970s.⁵ The bimodal distribution produced greater variance than the unimodal distribution. My analysis shows that the district component was virtually the only factor to shape partisan votes in the 1970s.⁶

Results—Movement of the Electorate

Figure 2 displays the results of analysis of the movement of the Democratic vote. This is the decadal variance of the swing,

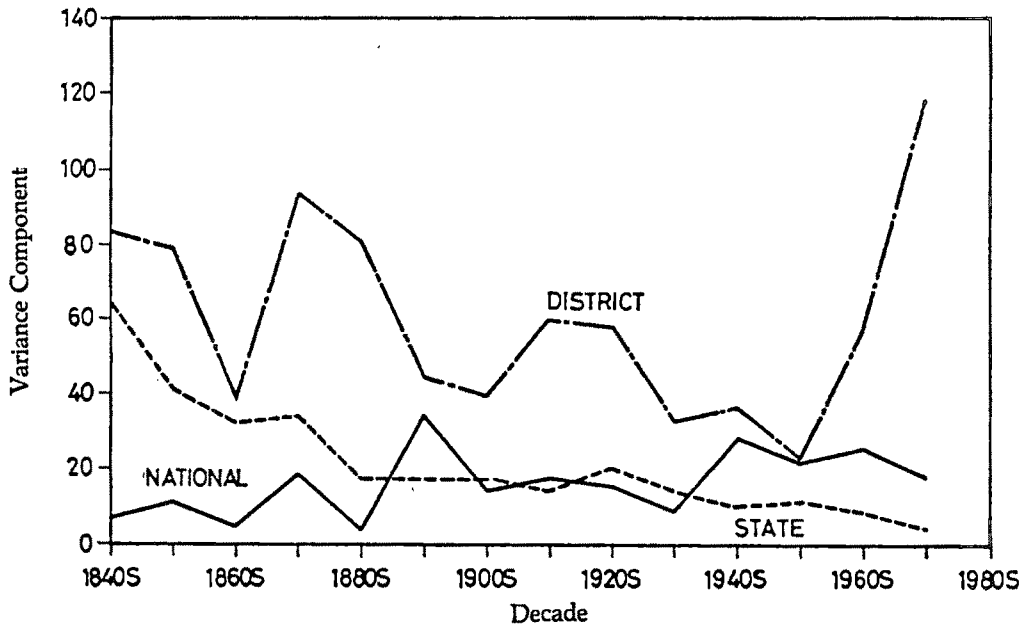
partitioned into national, state, and district components. Some proportion of the decadal variance of swing could be attributed to the national component in every decade. But this proportion increased during critical realignments, as shown by the peaks in the 1890s and the 1940s. As mentioned earlier, the decadal variance of swing was smaller in realignment decades. Thus, during realignments there was less variation in swing across the country, and more of that variation was attributable to the national component. These results validate Brady's proposition that in realignment periods "the election results were more national, less local" (1985, 30). Although the configuration of the electorate was never nationalized, the dynamic forces of realignment brought about a degree of uniform, national swing.

Districts responded very sensitively to realignment. In decades of realignment, the district component of swing shrank to almost half what it was at other times. A small district variance suggests that a state moved as a unit toward or away from a major party during realignment. In some states majority parties were overthrown. Although the state component did not change conspicuously over time, states always played a role in a realignment. For example, during the realignment of the 1860s, more than two-fifths of the variance was attributable to the state component.⁷

Thus I find a cyclical pattern to "nationalization as a uniform response," rather than the monotonic nationalization found in previous research. This suggests a reconsideration of the causes of nationalization. The stimulus promoting nationalization cannot be the changing structure of mass communication, as asserted by Stokes (1967). Instead the occasional movement of the vote uniformly across the nation seems to be considered genuinely political. Voters are mobilized nationally when they receive certain cues

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Figure 2. Movement of the Democratic Vote



from the national political scene. These affect the subsequent fortunes of the major parties. After the heat of a realignment, each congressman once again tries to secure his or her seat by establishing a personal appeal in his or her district. Consequently the district component grows, reflecting the various degrees of success with which congressmen insulate their districts from the national political scene.

The explosive growth in the district component of swing from the 1950s onward poses an interesting question. My analysis of configuration indicated that districts have become more and more diverse in their partisan outlooks. Now the districts show similar diversity in swing. Burnham (1978) has offered an explanation. He argues that incumbents are given "an increasing, personalized protection" (p. 14). In his "incumbent-insulation model," he predicted how the vote would swing in the three types of dis-

tricts: in districts where incumbents have served two terms or more, the swing will parallel the national swing but have little effect on the outcome; in districts where freshmen are up for reelection, strong countercyclic swing will favor those of both parties; and in districts where there is no incumbent, strong convergent swing toward 50% will take place (p. 15). The mixture of these swings naturally leads to the maximum heterogeneity of swing across districts. The increased district component my model has found since the 1950s supports Burnham's model.

Conclusion

I have tried to fill the lacuna between electoral analysis of nationalization and realignment. I hope that these results will provoke some further questions about the nature of U.S. party politics. Particularly,

the results from the 1950s through the 1970s suggest the need of an in-depth analysis of the electoral dynamics currently at work, which may provide an insight into how they will act in the near future.

I have not undertaken an analysis of turnout. But the criticism I have made of previous conceptualizations clearly suggests the unreliability of the conclusion that turnout is highly nationalized (Claggett, Flanigan, and Zingale 1984; Stokes 1965). Further analysis of the relationship between presidential and congressional elections should also be conducted in light of the finding that critical realignments affect the district, state, and national contributions to voting patterns in elections.

The United States is an extremely diverse nation and this diversity must be captured in an appropriate model of electoral behavior. In a system where the basic voting configuration is determined district by district, the movement of the vote has been affected by both local and national influences. The understanding of voting behavior must reflect this fact.

Appendix: Stokes's Variance-Components Model

Stokes's model is described by the following equation:

$$Y_{ijk} = \alpha + \beta_i + \gamma_{ij} + a_k + b_{ik} + c_{ijk} \quad (\text{A-1})$$

where Y_{ijk} is the vote or turnout in the j th district of the i th state in the k th year, α is the fixed national effect, β_i is the fixed effect of the i th state, γ_{ij} is the fixed effect of the j th district of the i th state, a_k is the effect of national forces in the k th year, b_{ik} is the effect of forces in the i th state in the k th year, and c_{ijk} is the effect of forces in the j th district of the i th state in the k th year (Stokes 1965, 66).

Stokes calls this a nested, mixed model. It is nested, because districts are grouped within states and states are grouped

within the nation. It is mixed, because some effects within the model are fixed, others random. He stipulates the three random effects as follows:

$$a_k = Y_{..k} - Y_{...} \quad (\text{A-2})$$

$$b_{ik} = (Y_{i..k} - Y_{i..}) - (Y_{..k} - Y_{...}) \quad (\text{A-3})$$

$$c_{ijk} = (Y_{ijk} - Y_{i..k}) - (Y_{ij.} - Y_{i..}) \quad (\text{A-4})$$

Substituting the terms in Equation A-1 by Equations A-2-A-4, we get

$$Y_{ij.} = \alpha + \beta_i + \gamma_{ij}$$

Thus, the model may be described as

$$Y_{ijk} = Y_{ij.} + a_k + b_{ik} + c_{ijk} \quad (\text{A-5})$$

I am not interested in following Stokes's precise formulation for the calculation of the national, state, and district components. Instead, I would like to point to some implications of Equation A-5. First, Stokes's model as stated in Equation A-5 is quite close to my model. The difference is that while Stokes's model contains the term $Y_{ij.}$, my model contains M , which is the grand mean of the vote $Y_{...}$. If we put $X_{ijk} = Y_{ijk} - Y_{ij.}$, then Equation A-5 will be $X_{ijk} = a_k + b_{ik} + c_{ijk}$. It follows immediately that $\text{var}(X_{ijk}) = \text{var}(a_k) + \text{var}(b_{ik}) + \text{var}(c_{ijk}) + 2[\text{cov}(a_k, b_{ik}) + \text{cov}(a_k, c_{ijk}) + \text{cov}(b_{ik}, c_{ijk})]$. But because Stokes argues that covariance terms are of negligible importance, $\text{var}(X_{ijk}) = \text{var}(a_k) + \text{var}(b_{ik}) + \text{var}(c_{ijk})$. This is exactly the same as my model except that the left-hand-side expression is the variance of X_{ijk} and not that of Y_{ijk} . Thus, the difference between Stokes's model and my model centers on which variance to partition into national, state, and district components. In my model, the variance of the vote or the swing is to be partitioned. In Stokes's model, the variance of $X_{ijk} = Y_{ijk} - Y_{ij.}$ is to be partitioned. X_{ijk} is the deviation of the vote at a district from the mean over a series of elections. This corresponds to Flanigan and Zingale's (1974)

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definition of electoral change as movement away from normal voting patterns. Therefore the question is whether to use swing or Flanigan and Zingale's measure to estimate variance components of movement in the electorate. My choice is based on the conviction that swing is the better measure of electoral change, while the vote itself is the best indicator of the configuration of the electorate. Thus, the variance-components model serves to measure the two types of nationalization.

Notes

I would like to thank Michael Les Benedict for always encouraging me and commenting on my work.

1. Clubb, Flanigan, and Zingale (1980) utilized this definition of electoral change extensively. Brady (1985) also depends on a technique based on this definition.

2. The data utilized hereafter is Inter-University Consortium for Political and Social Research (ICPSR) study no. 7757, Candidate and Constituency Statistics of Elections in the United States, 1788-1982 supplied by the ICPSR. The ICPSR bears no responsibility for the analysis or interpretations presented here. A few words about handling procedures are in order. Scholars are divided on whether uncontested districts should be included. Therefore, I did another set of analysis using the data that included uncontested districts. The results showed that there was an overall increase in the level of variance, but over-time trend was not much affected.

3. My model, as well as Stokes's, is sometimes called a *variance-components model*, which is referred to as Model 2 by Kendall and Stuart (1966) and as Model 5 by Graybill (1961).

4. I chose to handle the data in a slightly different way from Stokes. First, I chose the *total vote cast* as the basis for calculating percentages and examined the configuration and the movement of both major parties, because one party is not exactly the mirror image of the other. Second, every contested data available in each decade was used for the calculation of the variance components. This differs from previous approach (Katz 1973; Stokes 1965), which excluded one-district states and included districts only where there were contests throughout the decade being analyzed. Thus, in my analysis the number of cases in the cells relating to year and state is different from one year to another. This is called "complete unbalancedness of the data." Most of

analysis of variance programs will handle this kind of data.

5. For arguments related to Mayhew's findings and the behavioral changes of congressmen, see Burnham 1975b; Cain, Ferejohn, and Fiorina 1984; Ferejohn 1977; Fiorina 1977a, 1977b; Hinckley 1981; Jacobson 1983; and Tufte 1973. For arguments from a broader perspective on the transformation of the party system, see Burnham 1975a, Ladd and Hadley 1978; and Sundquist 1983.

6. Configuration of the Republican vote, not shown here, was similar to that of the Democratic vote. However, one point is of interest. There was a relatively large national component in 1912-20. This was probably caused by the Theodore Roosevelt Progressive revolt. In that decade, the national component was 129.41 while the state and district components were 101.81 and 196.20.

7. The results of analysis on the movement of the Republican vote were only slightly different. Thus the movement of the Democratic vote tells most of the story. But there was one major difference. In 1912-20, the state and district components jumped in the Republican analysis, while the district component did and the state component did not increase in the Democratic analysis. Burnham, Clubb, and Flanigan (1978) found a similar increase in an analysis of state averages of absolute values of first differences of the Republican proportion of the vote for congressmen. Their explanation seems pertinent to my result. That is, high scores in the 1912-20 reflect the Bull Moose schism in the party, and they will become smaller by combining vote returns of the old guards with those of the Progressives.

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DEMOCRATIC REPRESENTATION AND PARTISAN BIAS IN CONGRESSIONAL ELECTIONS

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The translation of citizen votes into legislative seats is of central importance in democratic electoral systems. It has been a longstanding concern among scholars in political science and in numerous other disciplines. Throughout this literature, two fundamental tenets of democratic theory, partisan bias and democratic representation, have often been confused. We develop a general statistical model of the relationship between votes and seats and separate these two important concepts theoretically and empirically. In so doing, we also solve several methodological problems with the study of seats, votes, and the cube law. An application to U.S. congressional districts provides estimates of bias and representation for each state and demonstrates the model's utility. Results of this application show distinct types of representation coexisting in U.S. states. Although most states have small partisan biases, there are some with a substantial degree of bias.

The relationship between legislative seats and citizen votes is a longstanding concern in democratic theory (e.g., Balinski and Young 1982; Dahl 1956, 147-49; Farrand 1911; Locke 1965, 419-20; Rae 1967; Schattschneider 1942). Through this relationship, legislative majorities are formed and minorities protected. Constitutionally mandated reapportionment and shifting patterns of partisanship have created opportunities for state legislatures and partisan gerrymanders to alter the congressional seats-votes relationship (Cain 1984; Grofman et al. 1982; Polsby 1971). Over the last century, scholars in political science, sociology, economics, mathematics, statistics, and political geography have studied these normative theoretical questions and sought empirical estimates of bias and unfairness (Hay and Rumley 1984; Ken-

dall and Stuart 1950; March 1957-58; Theil 1970; Tufté 1973). Furthermore, the recent spate of court challenges, the courts' willingness to hear political gerrymandering cases, and the Supreme Court's interest in a threshold of political discrimination, have rekindled the seats-votes controversy (Karcher v. Daggett, 462 U.S. 725 [1983]; Davis v. Bandemer, 478 U.S. [1986]; Brown v. Thomson, 462 U.S. 835 [1983]).

Concern with and, we believe, confusion over two fundamental tenets of democratic theory, partisan *bias* and democratic *representation*, dominate this literature. Partisan bias introduces asymmetry into the seats-votes relationship, resulting in an unfair partisan differential in the ability to win legislative seats: the advantaged party will be able to receive a larger number of seats for a fixed number

of votes than will the disadvantaged party. Although *bias* is easily defined, it is not always as apparent or as easily measured (Grofman 1983). Even in the absence of partisan bias, several forms of democratic representation are possible: strict proportional representation—in which the percentage of seats equals the percentage of votes—and winner-take-all elections are the pure forms, with many other possibilities in between.¹ Whereas the extent of bias is a separate problem, the precise effect of partisan bias depends on the specific form of democratic representation.

Much of the literature either treats partisan bias and democratic representation as one concept or mistakenly confuses different democratically legitimate forms of representation with clearly invidious partisan bias. Because bias and representation are related, the separate estimations in most previous research can be shown to be statistically inconsistent. By expressing each of these concepts as a separate parameter in a unified model, we show that it is possible and useful to emphasize the analytical and empirical distinctions by developing a model that jointly estimates both of these parameters. In a single and conceptually simple equation, we incorporate the full range of possible values for bias and representation. The result is a general form that is useful in understanding both the fairness of legislative reapportionment and the democratic character of legislative representation. The validity and utility of this model is then demonstrated with an application to state level congressional seats and votes for the period 1950–84.

The essence of bias and representation in democratic regimes is realized in the translation of votes into seats.² Assume initially that there are only two parties, Democratic and Republican, and that the legislature is composed of a set of single-member, winner-take-all districts. We begin with a few standard definitions. Let

- v = the number of votes cast for Democratic party candidates
- T = the total number of votes cast for candidates of both parties
- $v_R = T - v$ = the number of votes cast for Republican party candidates
- $V = v/T$ = the proportion of votes cast for Democratic candidates
- $V_R = 1 - V$ = the proportion of votes cast for Republican candidates
- s = the number of seats allocated to the Democratic party candidates
- s_R = the number of seats allocated to the Republican party candidates
- D = the total number of single-member legislative districts
- $S = s/D$ = the proportion of seats allocated to the Democratic candidates
- $S_R = 1 - S$ = the proportion of seats allocated to the Republican candidates

We express the absence of partisan bias as partisan symmetry. In general, this means that in an election system where $x\%$ of the Democratic votes produces an allocation of $y\%$ of the seats to the Democrats, then in another election under the same system $x\%$ of the Republican votes would yield the same $y\%$ Republican allocation of seats. This is the situation that Grofman (1983) calls "completely unbiased."

Stated more formally, if $V = x \Rightarrow S = y$, then $V_R = x \Rightarrow S_R = y$, for all x and y . This completely unbiased system requires only one point at which the percentage of votes equals the percentage of seats: when each party receives 50% of the votes, the seats must be divided equally between them. The partisan fairness expressed by this symmetry does not restrict x to equal y at any but this point. When it is true that $x = y$, for all x and y , we have the situation of unbiased proportional representation. However, there are many other interesting types of unbiased

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representation systems, and our model explicitly incorporates the full range of these.³

Bias, formalized as partisan asymmetry, makes it possible for one party to receive 50% of the votes but not necessarily 50% of the seats. This situation is modeled similarly: If $V = x \Rightarrow S = y$, then $V_R = x \Rightarrow S_R = z$, where y is not necessarily equal to z . However, in the United States, even a biased system would not allocate any seats to a party without any votes. This means that both biased and unbiased systems are restricted to pass through the (0%, 0%) and (100%, 100%) points on the votes-to-seats curve; it is near the middle range of votes and seats that the potential for bias is greatest.

Our model of representation and bias will now be developed and explained in more detail. We introduce the mathematical form and explain the substantive significance of the bias and representation parameters of each. Those preferring a nonmathematical exposition are referred to Figures 1-3.

Modeling Representation

There are a number of plausible functional forms that could be used to model the full range of representation while still restricting the system to be unbiased. Most of these forms lead to nearly identical conclusions, if not to the same models. We believe our model best matches the definitions above and has the additional advantage of being a generalized form of the best-known model of the votes-seats relationship, the "cube law" of electoral politics. Our generalization also extracts the hidden features of this formal "law," known at least since 1909 (see Kendall and Stuart 1950), and expresses them in a more interpretable form. We show how this form can realistically and flexibly model concepts and relationships of fundamental importance to democratic theorists, political scientists, and the courts.

Equation 1, with ρ set equal to 3, is the classic cube law:

$$\left(\frac{S}{1-S}\right) = \left(\frac{V}{1-V}\right)^\rho \quad (1)$$

Some time after its inception, investigation with actual election results indicated that values for ρ other than 3 were better descriptions of many electoral systems (see Taagepera 1973; Tufte 1973; and the citations in Grofman 1983, 317). While this mathematical relationship is straightforward, it is difficult to interpret in a theoretically meaningful way without either knowledge of analytical geometry or specific applications.

As an alternative, consider the algebraic characterization that follows. First, by taking natural logs, we rewrite Equation 1 as

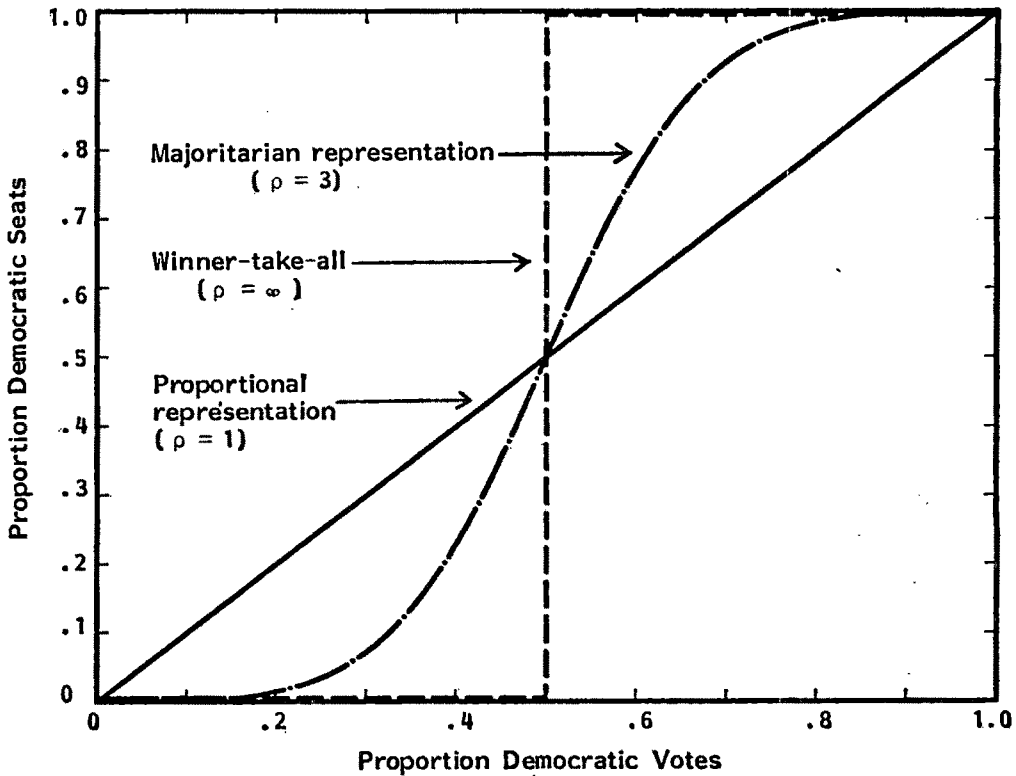
$$\ln\left(\frac{S}{1-S}\right) = \rho \ln\left(\frac{V}{1-V}\right) \quad (2)$$

Tufte (1973, 545), making unrealistic assumptions about the disturbance term (see Linehan and Schrodtt 1978), estimated ρ by running a linear regression of $\ln[S/(1-S)]$ on $\ln[V/(1-V)]$ and including a constant term in the equation.⁴ With some additional algebraic manipulation, Equation 2 can be expressed as a modification of the dichotomous logit model (see King 1986a). Thus, from the perspective of models common in political science, Equation 3 should be more directly interpretable:

$$s = D \left\{ 1 + \exp \left[-\rho \ln\left(\frac{V}{1-V}\right) \right] \right\}^{-1} \quad (3)$$

There are two differences between Equation 3 and the logit model commonly used to analyze dichotomous dependent variables: $\ln(V/1-V)$ is a log-odds function of V ; and there is no constant term.

Figure 1. Forms of Unbiased Representation (Based on Equation 3)



However, the inverse of the term in braces still ranges between 0 and 1, and, when multiplied by D , the entire right-hand side is restricted to vary between 0 and D , the number of districts. Since a log-odds transformation is the inverse of a logit, we call Equation 3 the *bilogit* functional form. Thus, we have in Equation 3 a model that generates the forms of representation depicted in Figure 1.⁵

Figure 1 demonstrates the range of functional forms that can emerge from Equation 3 and depend on the value of ρ (rho), the representation parameter. We discuss winner-take-all representation ($\rho = \infty$), majoritarian representation ($1 < \rho < \infty$), and proportional representation

($\rho = 1$). Equation 3 can also be used to model antimajoritarian or unresponsive representation ($0 < \rho < 1$), not discussed here.

When $\rho = 1$, the translation of votes into seats is by *proportional representation*. As the figure indicates, some proportion of votes will yield exactly that proportion of seats for the Democratic party. Exact proportional representation is unlikely in actual U.S. district-based elections, if only because there are fewer seats than votes: it would require a proportional increase in seats ($1/T$, to be precise) for even one additional vote; this is impossible, unless there were one seat for each voter. Other more realistic condi-

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tions, such as incumbency and party competition, can also lead to results that are not proportional.

Majoritarian representation is the situation where $1 < \rho < \infty$. The further ρ is from 1, the further the electoral system is from proportional representation. A common example of this is the cube law ($\rho = 3$), portrayed as the curve in Figure 1 in the shape of an escalator. This relationship between seats and votes helps majorities to form. When a party approaches 50% of the votes, each additional increment of voters increases the proportion of seats by a larger amount, as evidenced by the steep slope at $V = .5$; thus, parties are encouraged in their search for majorities. There is also an incentive for more partisan competition, since the marginal benefit, in terms of seats, of an additional increment of votes is greatest as both parties approach 50% of the votes. But one aspect of this relationship has been overlooked: although this form of representation favors majority and near-majority parties, there is a sense in which it protects minorities. This can be seen by looking near the top and bottom of the graph, where the slope of the line becomes progressively flatter. After a party gets 50% of the vote (and a majority of seats), each additional increment of votes yields a smaller incremental proportion of seats. The increase in the proportion of majority-party voters it would take to eliminate the last percentage point of minority seats (i.e., from 99% to 100% for the majority party) is far greater than the increase in voters it would take to reduce minority representation by one percentage point near the middle of the curve (say, from 55% to 56% for the majority party). This majoritarian electoral system thus encourages majorities to form but simultaneously makes it more difficult for minority representation to be eliminated. Although only one type of majoritarian representation is pictured in Figure 1, there are an infinite number of

possibilities—from just beyond proportional representation ($\rho > 1$) to just before winner-take-all ($r < \infty$).

A third situation is *winner-take-all*, which occurs when $\rho = \infty$. This is also portrayed in Figure 1. In this case, 50% plus one vote translates into 100% of the seats. Although this situation exists for each congressional district, for example, it does not usually apply to aggregates of them.⁶

Although proportional representation is most often proffered as the standard of fairness, we see no a priori reason to believe that one form of representation is inherently more fair than the others, provided that there is partisan symmetry. Convincing arguments can be made in favor of each of these types of democratic representation. At first glance proportional representation seems fair, since the translation process *reflects* underlying voter preferences most directly. But representation systems need not only reflect to be fair and meaningful (King and Ragdale 1987; Pitkin 1967). Winner-take-all systems, for example, have some elements of reflection but also recognize that only one party can, and assume that only one party should, govern. These electoral systems thus emphasize ability to govern *and* reflection in the method of translating citizen votes into legislative seats. In general, there is a trade-off between these two criteria, but since winner-take-all systems do not favor one political party over the other, there is no real reason to consider it unfair. In fact, one can argue that majoritarian representation, falling between proportional and winner-take-all representation, best describes many popular notions of U.S. democracy: majorities are encouraged, but small minorities are protected and thus represented. An "optimal" value of ρ is therefore a matter for political or judicial decision. Thus, there appears to be no a priori or axiomatic basis on which to choose one system over another.⁷

Modeling Bias

Although there are many types of "fair" democratic systems of representation, partisan bias is usually condemned. Unfortunately, although partisan bias is often discussed, it is seldom estimated. Indeed, it is not even included as part of the cube law. In order to incorporate the possibility of bias into this model, we must choose a form that still restricts the votes-seats curve to pass through the (0,0) and (1,1) points; this means that a party with no votes will receive no seats. At the same time, this model must still allow for the full range of forms of representation already explicated.

Our solution is to augment Equation 1 (reinterpreted as Equation 3) with a bias parameter; this allows the joint estimation of both partisan bias and democratic representation. Letting β (beta) be the bias parameter, the new generalized cube law can be written as⁹

$$\left(\frac{S}{1-S}\right) = \beta \left(\frac{V}{1-V}\right)^{\rho} \quad (4)$$

This form allows the curves drawn in Figure 1, from Equation 3, to be asymmetric, our definition of partisan bias.⁹

Almost every empirical study of the cube law has implicitly assumed that $\beta = 1$, the situation of no bias. When there is in fact no bias, the $\beta = 1$ constraint causes no problem. However, when there is bias toward one of the political parties, the constraint will result in statistically inconsistent estimates. The model in Equation 4 allows for bias without the possibility of statistical inconsistency. Tufte (1973) was probably the first to recognize that bias and representation could be modeled in one equation, and his was a linear approximation to Equation 4. We believe our nonlinear model is a more realistic version than Tufte's in that we allow for every possible degree of partisan bias and every possible form of demo-

cratic representation. Unlike the linear model, even systems with widely varying and quite extreme values of S and V can be incorporated in this model.

Equation 4 can also be written in an algebraically equivalent, but more substantively interpretable, bilogit form:

$$s = D \left\{ 1 + \exp \left[- \ln(\beta) - \rho \ln \left(\frac{V}{1-V} \right) \right] \right\}^{-1} \quad (5)$$

We incorporate the bias parameter in this model because of widespread concern about the fairness of the congressional reapportionment process. We therefore provide a more realistic model of both bias and representation, allowing for the exact form of the bias to depend upon the specific type of electoral representation. Consider now what happens when bias is added to the unbiased forms of representation pictured in Figure 1.

Generically, bias refers to asymmetry in the seats-votes relationship for the two parties. The easiest type of bias to understand is for winner-take-all systems ($\rho = \infty$). A bias in favor of the Republicans is the case where the discontinuity in the curve (the vertical part of the line in Fig. 1, $\rho = \infty$) is moved to the right: the proportion of the vote that the Democrats would have to win in order to take all the seats would be greater than half. Similarly, if the discontinuity moved to the left, the bias would help the Democrats. Bias in winner-take-all systems is so apparent that it rarely occurs in U.S. elections, except in the presence of fraud or other abuses.¹⁰ The particular forms of bias associated with the other two types of representation are quite distinct and more complex. The empirical results presented below indicate that bias coexists with many forms of representation in a number of U.S. states.

Table 1 summarizes the possible ranges

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of the bias parameter and their respective interpretations. Since β is log-symmetric (ranging from 0 to ∞ with 1 at the center), it will be convenient to express the coefficient in terms of its natural log, that is $\ln\beta$ (ranging from $-\infty$ to ∞ with 0 at the center).

Figure 2 plots three types of bias under a proportional representation system.¹¹ $\ln\beta = 0$ is obviously the case of no bias: .5 Democratic votes yields .5 Democratic seats; .6 Democratic votes yields .6 Democratic seats. This is proportional representation because the proportion of seats equals the proportion of votes; it is unbiased because these same figures also hold for the Republican party. The 45°

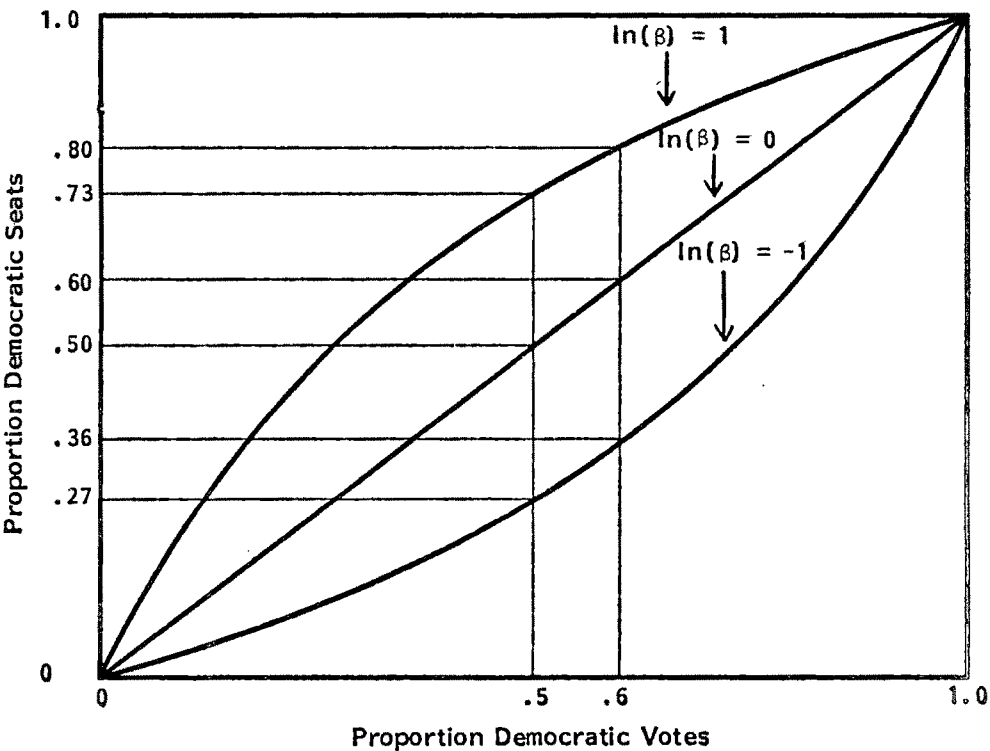
Table 1. Bias Coefficient Values

Coefficient Value	Direction of Bias
$\ln\beta > 0 \Leftrightarrow \beta > 1$	bias towards Democrats
$\ln\beta = 0 \Leftrightarrow \beta = 1$	unbiased
$\ln\beta < 0 \Leftrightarrow \beta < 1$	bias toward Republicans

line embodies this relationship; a given percentage change in seats yields an equal percentage change in votes throughout. In evaluating the other two biased lines, it is useful to consider this unbiased plot as the baseline.

For example, under proportional representation, .5 Democratic votes should

Figure 2. Bias and Proportional Representation (Based on Equation 5)



yield .5 Democratic seats (because this is where the vertical line meets the unbiased line), but the line marked $\ln\beta = -1$ only allocates .27 Democratic seats and the line marked $\ln\beta = 1$ allocates .73. A similar situation exists at all other points on this graph. At .6 Democratic votes, the fair outcome is .6 Democratic seats, but the upper line, $\ln\beta = 1$, is biased toward the Democrats, yielding .8 of the Democratic seats. The lower line, $\ln\beta = -1$, is biased toward the Republicans, yielding only .36 Democratic seats. The asymmetry defines our theoretical notion of bias.

Note that for each point on the horizontal axis, there is a different absolute bias for different parts of any biased line. For $\ln\beta = 1$, the absolute bias is $|.5 - .73| = .23$ at .5 of the Democratic votes, but it is only $|.6 - .8| = .20$ at .6 votes Democratic. In our model, the maximum absolute bias occurs at or near the .5 mark because the curves converge as they approach 0 or 1. This provides some justification for formal statistical models (Quandt 1974) and more intuitive analyses (Tufte 1973) based on bias measured only at .5. However, since there are many electoral systems where the percentage Democratic is rarely near 50%, it also suggests that we should look past this point to incorporate the full range of bias. For proportional representation systems, a measure based on the Gini index of the area between the biased and unbiased curves is possible, but this does not generalize as easily to the majoritarian representation or winner-take-all cases. Grofman's (1975, 1983) "normalized measure" of bias could be utilized here, but for present purposes the most natural way to measure the range of bias existing in the system is to use β or, equivalently, $\ln\beta$.

Figure 3 expresses bias for majoritarian representation systems of the specific type $\rho = 3$. The line in the middle, marked $\ln\beta = 0$, is the unbiased line included for reference. At .6 votes Democratic, the fair proportion of Democratic seats is .77.

This is fair under the $\rho = 3$ majoritarian system, but not necessarily under other representational schemes. For the $\ln\beta = -1$ line, there is a bias favoring the Republicans, so that the same .6 votes Democratic yields only a .55 proportion of Democratic seats. The $\ln\beta = 1$ line, biased toward the Democrats, yields .9 of the Democratic seats. For this figure, the absolute bias is also different for each point on the horizontal axis. For the line marked $\ln\beta = 1$, the absolute bias at .5 votes Democratic is $|.5 - .73| = .23$, but the absolute bias at .6 is only $|.77 - .90| = .13$.¹²

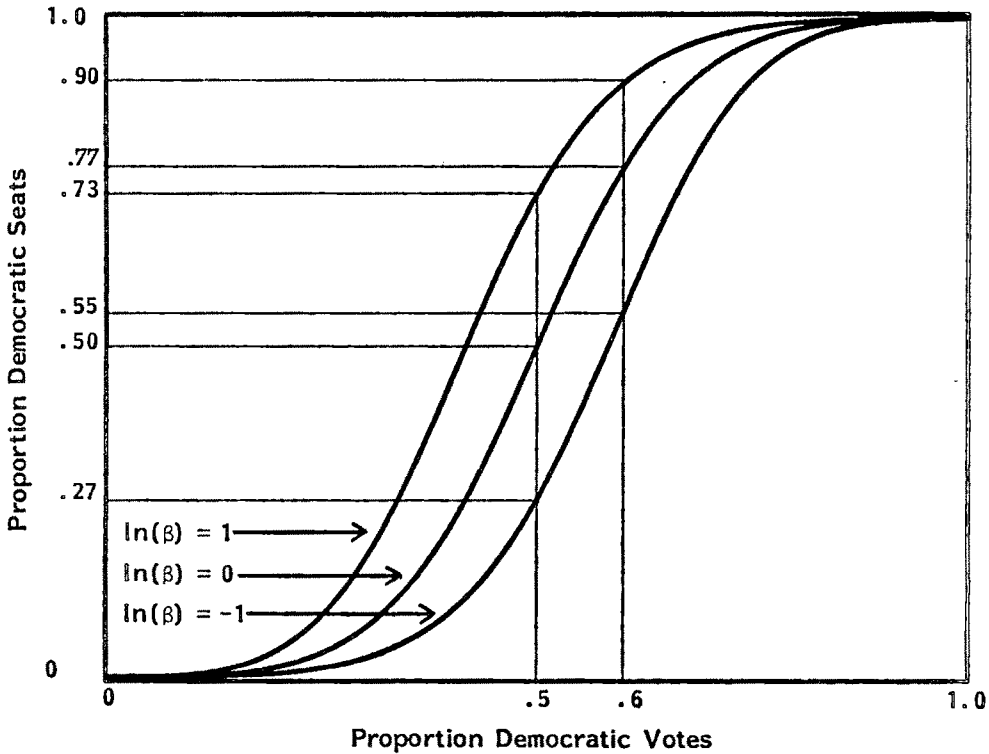
Deterministic Laws and Probabilistic Realities

Some of the most significant contributions to the literature on seats-votes relationships imply that the cube law, or some relevant variant, is deterministic. Whether this relationship is deterministic or probabilistic is an empirical issue, and it has important consequences for theoretical understanding and data analyses. We believe it is difficult to find even one meaningful example of a deterministic law anywhere in the social sciences.¹³ There is also strong evidence that a deterministic relationship between seats and votes does not exist: "While the cube law is stated deterministically, even a cursory examination of election statistics shows that it does not hold perfectly" (Schrodt 1981, 33). Michels's (1911) "Iron Law of Oligarchy" and many others have also failed the test. In making a general point (coincidentally using the cube law as an example), Achen (1982, 15) writes, "Any attempt at specifying exact causal functions must necessarily result in oversimplified explanations."

The Appendix develops a number of original, but somewhat technical, points: we justify a binomial disturbance term and add it to Equation 5, propose a

Representation and Partisan Bias

Figure 3. Bias and Majoritarian Representation (Based on Equation 5; $\rho = 3$)



method of estimation, and provide some empirical estimates. The sections that follow use the results from the Appendix.

Data and Measures

To demonstrate the empirical utility of this model, it is applied to data on U.S. congressional elections, 1950–84.¹⁴ For most analyses of the U.S. Congress, the entire nation has been used. But there are significant findings that demonstrate substantial variation across states (e.g., Scarrow 1983; Tuft 1973). We will therefore conduct separate analyses for each state, using the 18 elections between 1950 and 1984. Alaska and Hawaii were excluded

because they were not states during the entire period. Small changes in the elections included in the analysis did not materially alter the results because of the generally slow changes in state electoral systems.

The implied assumption in this approach is that β and ρ vary more across states than over time within any one state. We believe that this is justifiable on two grounds. First, the whole process of reapportionment is conducted separately by each state legislature. Aside from significant variation in the decisions by state legislatures, there are also large discrepancies in political geography and political culture among the states. Surely, the

variation over time would be less than these cross-state differences. Second, we report large variations in both the degree of bias and the type of representation across states (see Figs. 4 and 5). At the same time, empirical tests indicated substantial stability within states over time.¹⁵ If β and ρ do vary more than expected over time for a particular state, then our estimates are average values for that state.

These data were coded from, and cross-checked against, the *Statistical Abstract of the U.S.* (annual volumes), *Congressional Quarterly's* (1975) *Guide to U.S. Elections*, Cox's (1972) *State and National Voting, 1910-1970*, and Scammon and McGillivray's *America Votes* (annual volumes). As Niemi and Fett (1986) point out, the data collection is not as straightforward as it might seem. For example, we delete the very few representatives who had won seats under the independent-party label and subtracted the votes received by their Democratic and Republican opponents from the statewide total. We ignore resignations, deaths, and special elections. The member receiving the most votes in the general election was presumed to be elected. At-large districts, used by a few states immediately following reapportionment, are excluded unless there is only one district in the state. Several states have laws that do not require votes to be tabulated in uncontested races. For these states, we included only those districts and votes that were contested and reported. All other districts and votes are included.

Some have argued that the form of democratic representation is a function of the number of districts and voters, the geographical distribution, party competition, and the number of incumbents. Our approach follows two steps. We first estimate the bias and representation parameters for each state using the number of votes and seats for each party and election from 1950 to 1984. Once these parameters have been estimated, we show how differ-

ences in these estimates across states can be explained by measures of state political characteristics. This two-stage approach is as good as a simultaneous estimation because β , ρ , and many of these exogenous state characteristics change considerably more across states than over time.¹⁶

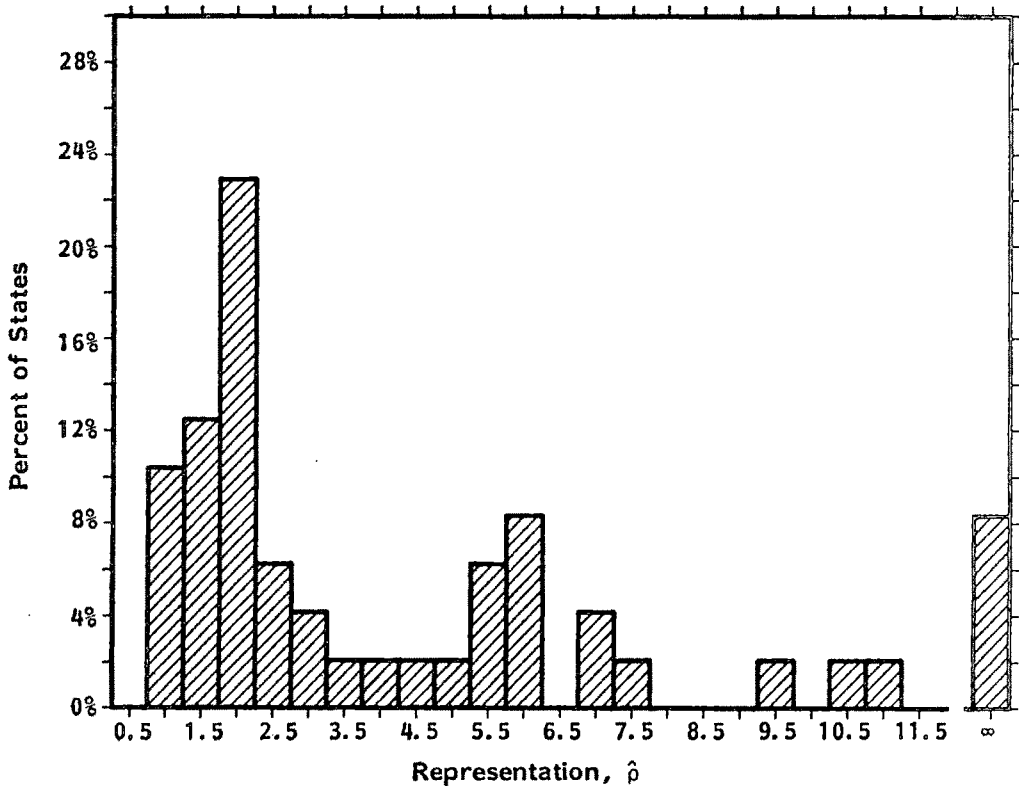
Estimating Representation and Bias in U.S. House Elections

Using Equation 5 as the general model, the estimation procedure described in the Appendix, and the data introduced below, bias and representation coefficients were estimated for each state. We present the results in Table A-1. For easier interpretation, Figure 4 presents a histogram of the representation coefficients.¹⁷ Note that the distribution of these coefficients are trimodal, with modes at or near 1, 6, and 10. Most of the states are quite near to proportional representation, but significant numbers are strongly majoritarian, and several are approximately winner-take-all. Note that although ρ ranges up to infinity, ρ coefficients greater than 8 or 9 are essentially winner-take-all.

This finding demonstrates the utility of the model in three ways. First, it shows the large variance in the type of representation across states. Second, it demonstrates that the plurality of states are just above proportional representation ($\rho = 1$). Although a number of studies have shown that ρ is not equal to 3, the cube-law value, most find that ρ falls between 2 and 4, with an average of about 3. Finally, the results indicate that previous estimation procedures may have been statistically inconsistent due to the exclusion of a bias parameter. The extent of inconsistency in previous research is also quite substantial: although deriving an analytical expression for the inconsistency appears intractable, it is possible to get a feel for the extent of the problem. To

Representation and Partisan Bias

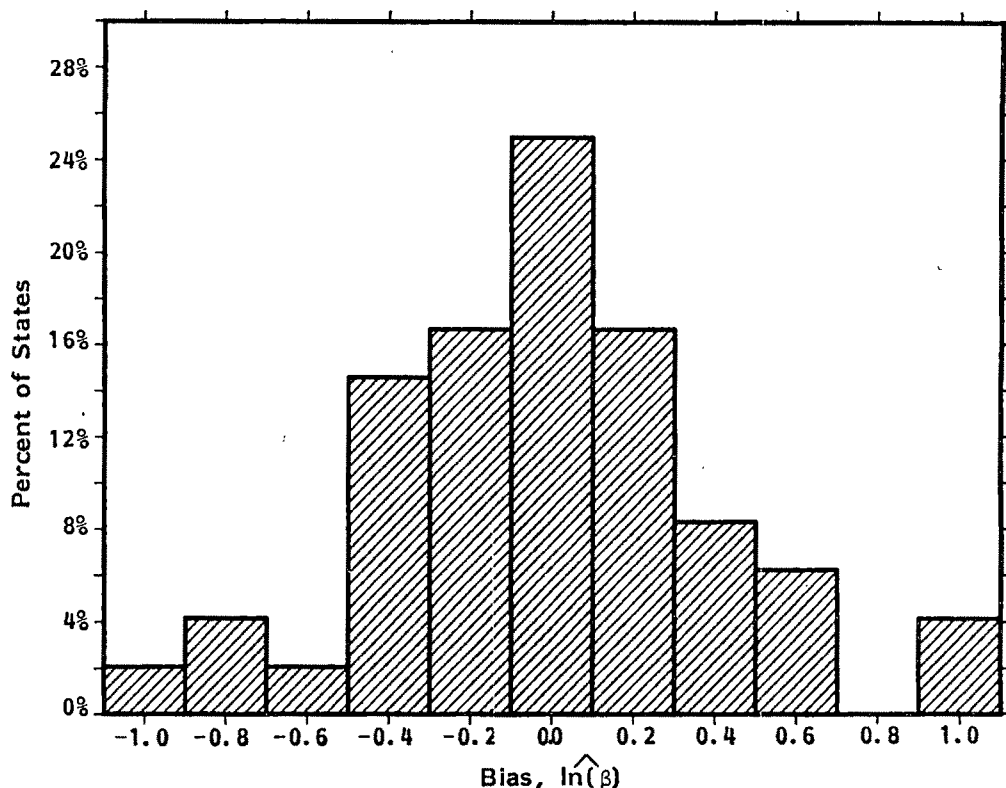
Figure 4. Representation in U.S. States



accomplish this, the state bilogit estimations were also run while constraining $\beta = 1$ [i.e., $\ln\beta = 0$]¹⁸—exactly as if the bias parameter had not been included. In these constrained estimations, ρ was too large about 1.5 times more often than it was too small. It thus appears that previous research has overestimated the degree to which U.S. democracy tended away from proportional representation and toward majoritarian representation. Although these are the tendencies, in any particular example it is unclear whether the inconsistency will cause the estimate of ρ to be too large or too small; the proper way to provide a consistent estimate of ρ is to use the joint model and estimation method proposed here.¹⁸

There is also substantial variation in the degree and direction of bias in U.S. congressional elections across states. Figure 5 presents a histogram of the estimated β coefficients. Note that the mean is almost exactly 0, and there is an approximately symmetric normal distribution around this point. This implies that the average of the states is not too biased toward one party more than the other. However, not all points fall on or about $\ln\beta = 0$, suggesting that at least some bias exists in individual states. In fact, even allowing for deviations from unbiasedness due to sampling variation and measurement error, the point estimates indicate that some states have quite large biases (see Fig. 5). There are two well-defined groups

Figure 5. Bias in U.S. States



of outliers at the ends of Figure 5. Kansas, Michigan, and Ohio have a substantial Republican bias, whereas Texas, California, and Florida have a substantial Democratic bias.¹⁹

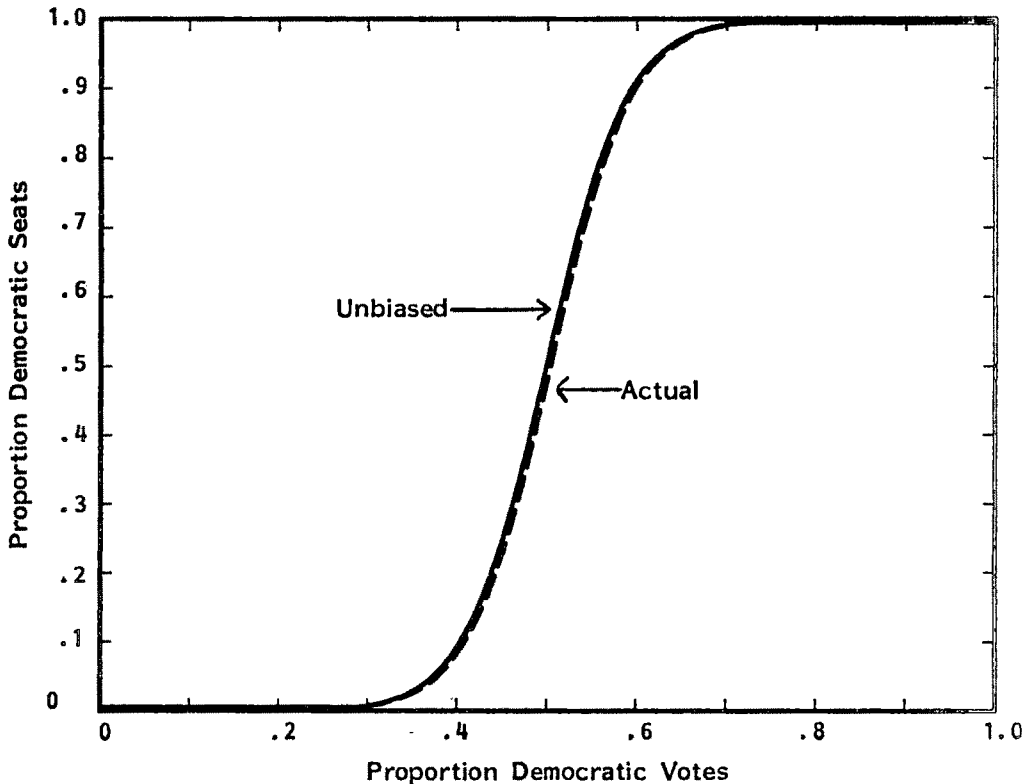
The effect of bias on proportional and majoritarian representation systems is seen in the examples of Indiana and Texas, graphed in Figures 6 and 7, respectively. Indiana is a generally Republican, but still competitive, two-party state. Our results indicate a very slight Republican bias (the unbiased baseline is almost indistinguishable from our empirical findings) and a steep, majoritarian, seats-votes relationship (the unbiased baseline is

a steep curve with two sharp bends). Indiana and other strong party states tend to demonstrate the slight bias and steeper slope portrayed in this figure.

Representation in Texas is quite near to proportional. The no-bias baseline in Texas is the nearly straight-line proportional relationship (see Fig. 7). However, this baseline deviates substantially from Texas electoral politics; the dominant Democratic tradition has created quite severe biases toward the Democrats, permitting them to win a majority of the seats with less than 30% of the votes. This is an extreme example of the biases that do exist in U.S. politics.

Representation and Partisan Bias

Figure 6. Bias and Representation in Indiana ($\hat{\rho} = 5.70$, $\ln(\hat{\beta}) = -.08$)



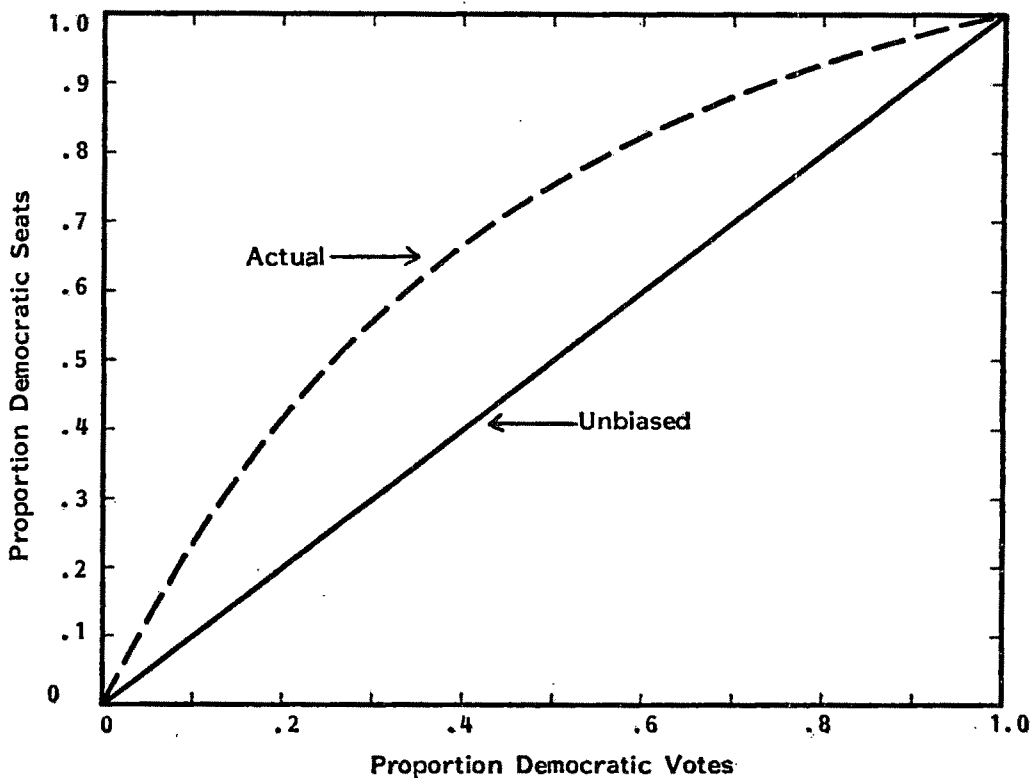
Explaining Representation and Bias in the U.S. House

In this section, we explore the differences among states in the bias and representation coefficients generated previously. This analysis helps to validate the estimates produced there. Leaving out the states with too little data for estimation or with coefficients equal to infinity (indicating winner-take-all representation), two series (bias and representation) of 44 observations each remain. The parameter estimates have different expected values (asymptotically equal to the population parameters) across states. The variation around these expected values is likely to be different for each coefficient, resulting in heteroscedastic disturbances. Fortu-

nately, we can estimate these variances by the approximate squared standard error resulting from the first-stage analysis. Furthermore, since the first-stage analysis was estimated with maximum likelihood, these coefficients will be normally distributed.

Taking all these factors into account, we use a weighted least-squares analysis, regressing the estimates of representation and bias on separate sets of explanatory variables.²⁰ Consider representation first. There are two main explanations for variation in the representation parameter. The best justified is Taagepera's (1973) index, $\ln(T)/\ln(D)$, where T is the total number of voters and D is the number of districts. To understand the logic behind this index, it is useful to focus on the

Figure 7. Bias and Representation in Texas ($\hat{\rho} = 1.05$, $\ln(\hat{\beta}) = 1.12$)



extreme cases. When there is no bias and one district for each voter (i.e., $T = D$), $x\%$ of the votes for one party will automatically yield $x\%$ of the seats for that party. This proportional representation result is captured by Taagepera's index, since $\ln(T)/\ln(D)$, where $T = D$, is equal to 1. The winner-take-all extreme is also captured by this index: When there is only one district for all the voters, as in a presidential election, we have $\ln(T)/\ln(1) = \ln(T)/0 \approx \infty$. Thus, the extreme values of this index are theoretically appropriate. Between these two extremes, the index predicts a gradual and continuous increase in the representation parameter—from proportional ($\rho = 1$) to majoritarian ($1 < \rho < \infty$) to winner-take-all ($\rho = \infty$) forms of representation—as the number

of districts increases relative to the number of voters. The precise rate at which ρ increases is defined by the form of his index, $\ln(T)/\ln(D)$. Other forms that also meet the boundary conditions are possible, but Taagepera's (1973) seems plausible. In the weighted-least-squares regression analysis performed here, this hypothesis would be confirmed if the regression coefficient on this index were 1.0 and the coefficients on the other variables in the equation were 0.

Party competition has been suggested as an examination for the form of representation. In strongly competitive party systems, where the percentage voting for each party in each election district is near 50%, a small increase in votes for one party across districts will likely result in a

Representation and Partisan Bias

Table 2. Explaining Representation

Independent Variable	Estimated Coefficient	Standard Error	t-Statistic
Constant	-1.17	.67	—
Taagepera's index	.32	.07	4.62
Party competition	2.17	.70	3.09

Note: Number of observations (states) = 44; standard error of the weighted-least-squares regression = .82; mean of the dependent variable ($\hat{\rho}$) = 1.54.

large increase in seats for that party. Party competition also encompasses the effect of the power of incumbency and uncontested seats. From the widely used Ranney index of state party strength (Ranney 1976), we construct a measure of party competition that ranges from 0 (no competition) to 1 (pure competition).²¹

Table 2 presents the weighted least-squares regression of our estimated representation parameters on Taagepera's index and party competition. The estimates for the effect of both variables are relatively precise and, at conventional levels, significantly different from 0. Taagepera's index has an effect about one-third of what his theory would predict, but it has the correct sign, and helps to explain interstate variation in the form of representation. This finding has several interpretations: a different form of the index might account somewhat better for the range of representation types between proportional and winner-take-all, or there may be a tendency for U.S. states to be more proportional than would be expected solely on the basis of the relative sizes of their voter populations and numbers of legislative districts. Both the value of, and the need for further research on, Taagepera's index are emphasized by this result.

The effect of party competition is shown to be relatively strong: if a state were to move from the lowest to the highest level of partisan competition, the representation parameter would move more than two points toward the winner-take-all extreme (see Table 2). This is well

within the bounds of the original hypothesis.

The propensity of a state to be biased toward the Republicans or Democrats has generally been explained by relative party strength. Parties may be in decline in some ways, but the desire to gerrymander remains as strong as ever.²² We would therefore expect all parties in all states to attempt to gerrymander, but only the states with dominant party systems would be successful.

As a measure of party strength, we use the Ranney index, ranging, as usual, from 0 (Republican) to 1 (Democratic). We also use a measure of state ideological orientation ranging from -1 (liberal) to 1 (conservative), estimated from state level CBS News-*New York Times* polls by Wright, Erikson, and McIver (1985).

The weighted-least-squares-regression results of estimated bias on state party strength and state ideological orientation are reported in Table 3. Both coefficients are in the hypothesized directions, although the ideology coefficient does not meet conventional significance levels. Our results indicate that the strongest Democratic state has a bias coefficient about .9 points higher (in the direction of Democratic bias) than the strongest Republican state, and that ideologically liberal states tend to be somewhat more biased toward the Democrats.

The plausibility of these second-stage results lends criterion validity to our original model. Variations in our estimates of bias and representation in the U.S. states vary relatively closely with

Table 3. Explaining Partisan Bias

Independent Variable	Estimated Coefficient	Standard Error	t-Statistic
Constant	-.44	.20	—
Party strength	.90	.32	2.82
Ideology	-.67	.59	-1.15

Note: Number of observations (states) = 44; standard error of the weighted-least-squares regression = .64; mean of the dependent variable ($\hat{\rho}$) = -.05.

these plausible criterion variables. Sensitivity analyses indicate that these findings are quite robust to marginally different specifications.²³

Conclusion

In this paper, the venerable cube law of electoral policies was reformulated, generalized, and reinterpreted to provide a model for the relationship between votes and seats in representative democracies. Whereas Taagepera (1986) generalized this law to proportional-representation elections, we generalize it to include both the form of democratic representation and the extent of partisan bias. Previous analyses tended to confuse the form of representation with the degree of partisan bias and have unintentionally constrained the partisan-bias parameter, in effect assuming no bias.

One consequence of this is that unbiasedness has mistakenly been associated with proportional representation. We find that all representation types captured by a single parameter in our model can treat the parties symmetrically using our definition of unbiasedness. Recent court rulings have upheld this view.²⁴

Additionally, by constraining the β parameter, past research has yielded statistically inconsistent estimates of the representation parameter, ρ , ranging between 2 and 4 for most electoral systems. In contrast, we find that the plurality of states were much closer to proportional representation than had

been previously believed. Nevertheless, it is not true that all previous estimations of representation can simply be slightly adjusted toward 1.0. Although the empirical tendency is in this direction, the direction and size of the inconsistency is unknown for any particular example. Thus, some previous analyses may be overestimates and some may be underestimates. Statistical consistency requires that partisan bias be incorporated as an additional parameter. After all, if a party receives, for example, 55% of the votes and 75% of the seats, this may represent a severe partisan bias or a fair system with majoritarian representation. The only way to distinguish between the two situations is to have joint estimates of both β and ρ .

The remarkably flexible bilogit functional form helped to highlight these problems with past research in the context of a theoretically meaningful solution. The model developed herein also demonstrates that different forms of representation are associated with different forms of bias, and that partisan bias can occur in widely varying degrees. This paper has also contributed to the statistical estimation of this inherently nondeterministic relationship between seats and votes. Applying a discrete probability distribution to the discrete variable, s , the number of Democratic seats, is a considerably more statistically efficient procedure than had previously been used. In an application to congressional elections, we demonstrate that there exists a wide variety of representation systems and degrees of

Representation and Partisan Bias

partisan bias. These variations have important implications for national politics and policy (Browning 1986).

Finally, a second stage of the analysis provided some validity to the estimates from the first stage and some evidence that there are systematic explanations for the direction and size of the bias and representation parameters. The level of state party competition and Taagepera's index of state and electorate size helped to explain differences in the representation parameter estimates across states. The Ranney index of party strength and a measure of the state electorate's ideological orientations indicated when and where bias was more likely to occur.

Beyond these contributions, these results also have important implications for the recent court cases on reapportionment. First, the courts must explicitly distinguish between bias and representation type. The discussion of using seats and votes to create indicators of political discrimination in the federal-district-court decision in *Bandemer v. Davis* (603 F. Supp. 1479 [S.D. Ind. 1984]) reveals confusion on that court's part on this issue (see Browning and King n.d.). While it may be desirable for the courts to decide on an acceptable range for the type of representation in U.S. states, partisan bias is a separate issue. The courts would obviously prefer a system with no bias, but this too may not be possible. In such a case, the courts might establish an "acceptable" level of bias. Using this method numerous elections over at least a decade are necessary to make a confident determination about the degree of bias and type of representation. Perhaps in those states with a history of bias, the courts might more closely monitor the reapportionment process, or might even direct a court-ordered reapportionment plan. Regardless, this analysis can provide an understanding of the type of democratic representation and the existence of partisan bias over a historical series of elections for a particular state.

Appendix

We show how our emphasis on probabilistic relationships transforms Equation 5. We also resolve a problem existing in the political methodology literature so that β and ρ may be estimated efficiently and consistently. Estimates for each of the states appear in Table A-1.

Several authors have recently attempted to convert the deterministic cube law in Equation 1 (with $\rho = 3$) to a statistical relationship. The main issue here is how to incorporate a disturbance term (Linehan and Schrodt 1978).²⁵ Schrodt (1981) provides the most comprehensive analysis of possibilities but is ultimately unable to select any particular method: "In the absence of a theoretical justification for a specific error structure in the cube law, there is no *a priori* reason for choosing one . . . model over the others" (Schrodt 1981, 35-36).²⁶

For a solution to this disturbance-term problem, we take a different approach. In reformulating the generalized cube law, we find that it is more politically interesting and more natural to model seats (s) than the odds of seats $S/(1 - S)$. The disturbance term should therefore be formulated in terms of the more fundamental and interpretable variable, s . Thus, we can rewrite Equation 5 to include an additive disturbance term:

$$E(s) = D \left\{ 1 + \exp \left[- \ln \beta - \ln \left(\frac{V}{1 - V} \right) \rho \right] \right\}^{-1} \quad (\text{A-1})$$

$$\text{or} \\ s = D \left\{ 1 + \exp \left[- \ln \beta - \rho \ln \left(\frac{V}{1 - V} \right) \right] \right\}^{-1} + \epsilon \quad (\text{A-2})$$

The expected value operator, $E(\cdot)$, means that the functional form on the right hand side of Equation A-1 (a bilogit function of votes) will correctly predict the number of seats, s , on average over

the long run. We assume in Equation A-2 that $E(\epsilon_i) = 0$. This means that, over the long run, the average error is zero.

We now only need to choose a probability distribution for s (or, equivalently, for ϵ). Recall that s is the number of seats allocated to the Democratic party. It is therefore a nonnegative integer ranging from 0 to D , the number of legislative districts. To incorporate as much information into the distribution as possible, we limit the range of possibilities to discrete probability distributions. The distribution should have the mean as a parameter but should not necessarily be symmetric, since the bounds at 0 and D make symmetry either impossible or implausible for means not equal to $D/2$. Instead, a formulation more faithful to the concept of partisan symmetry is required: the distributions with parameters $(D/2) + \lambda$ and $(D/2) - \lambda$ should be mirror images of one another.²⁷

The binomial distribution meets each of these requirements and is relatively easy to work with. One possible problem is that district outcomes within a state may not be independent, as is assumed by the binomial distribution. Experiments with alternative formulations that allow dependence among districts were performed and were found to add little to the analyses below. This result is consistent with recent research indicating that congressional elections are local, not national, events, fought primarily within individual election districts (Hinckley 1981). The likelihood equation that emerges from this distribution and Equation 5 is also quite similar to what would result if a Poisson or a variety of other distributions were chosen (see King n.d., 1987); indeed, estimates from this model would be consistent even if the distribution were

not binomial but were a member of the family of linear exponential distributions (Gourieroux, Monfort, and Trognon 1984). The results would therefore be quite similar if our assumption were incorrect.

Thus, for the analyses below, s was assumed to be distributed binomially; that is,

$$Pr(s = k; \theta|D) = [D!/(D - k)!k!] (\theta/D)^k [1 - (\theta/D)]^{D-k} \quad (A-3)$$

where $\theta = E(s)$, from Equation A-1. D , the number of districts, is assumed to be known a priori. ϵ is defined as $\epsilon = s - \theta$. For a maximum likelihood solution, the log-likelihood equation, reduced to sufficient statistics, can be written by substituting Equation A-1 into Equation A-3, taking logs, simplifying, and summing over all observations:

$$\begin{aligned} \ln(L_i) = & \sum_{i=1}^n \{s_i [\ln(D_i) \\ & - \ln[1 + \exp(-\ln\beta) \\ & - \ln\left(\frac{V}{1-V}\right)\rho]] \\ & + (D_i - s_i) \ln[1 - [1 + \exp(-\ln\beta) \\ & - \ln\left(\frac{V}{1-V}\right)\rho\{-1\}]]\} \quad (A-4) \end{aligned}$$

The Berndt et al. (1974) numerical-estimation algorithm, in combination with the positive definite secant update method, was used to maximize this likelihood function to derive the estimates described in the next section. Relatively quick convergence was achieved in nearly all cases.²⁸ The estimates of ρ and β , along with standard errors for each, appear in Table A-1. This table is further discussed and analyzed in the text.

Representation and Partisan Bias

Table A-1. Representation and Bias Coefficient Values

States	$\hat{\rho}$	Standard Error	$\ln(\hat{\beta})$	Standard Error
Alabama	1.95	1.14	-.70	.93
Arizona	1.55	1.78	-.46	.56
Arkansas	1.95	1.10	-.42	1.24
California	.93	.57	.16	.09
Colorado	2.08	2.55	.39	.51
Connecticut	6.11	1.83	.08	.33
Delaware	∞	—	.00	—
Florida	1.68	.97	.51	.44
Georgia	1.64	1.73	.46	1.98
Idaho	6.87	2.90	.14	.77
Illinois	1.34	1.60	-.09	.25
Indiana	5.70	2.39	-.08	.32
Iowa	7.07	2.13	-.31	.41
Kansas	2.43	1.68	-.82	.60
Kentucky	1.16	1.95	.51	.67
Louisiana	2.11	1.40	.04	1.81
Maine	11.87	7.65	-.92	1.50
Maryland	2.35	1.96	.02	.94
Massachusetts	1.96	1.04	-.14	.57
Michigan	3.23	1.96	-.47	.32
Minnesota	1.97	4.62	-.17	.51
Mississippi	1.38	.92	-.04	1.48
Missouri	2.62	2.62	.59	.80
Montana	2.11	4.17	.14	1.38
Nebraska	3.98	2.38	-.82	.93
Nevada	∞	—	.00	—
New Hampshire	6.31	5.48	-.13	1.37
New Jersey	2.83	2.37	.11	.25
New Mexico	5.15	2.34	.25	.44
New York	2.06	.98	.06	.14
North Carolina	∞	—	.00	—
North Dakota	7.43	7.88	-.25	2.20
Ohio	2.04	1.65	-.37	.28
Oklahoma	1.17	5.02	.95	1.58
Oregon	4.74	2.17	-.25	.55
Pennsylvania	1.63	1.25	-.03	.17
Rhode Island	11.23	19.11	-.33	3.59
South Carolina	2.20	1.66	-.12	.75
South Dakota	9.72	6.61	-.30	.94
Tennessee	1.25	1.62	.18	.66
Texas	1.05	.79	1.12	.68
Utah	10.60	4.43	.25	.67
Vermont	∞	—	.00	—
Virginia	2.09	1.54	-.21	.32
Washington	5.39	1.11	-.06	.25
West Virginia	5.67	5.06	.26	1.57
Wisconsin	3.52	1.88	-.32	.37
Wyoming	∞	—	.00	—

Notes

We appreciate the comments on earlier drafts of this work by, and discussions with, Christopher Achen, Nathaniel Beck, Gerald Benjamin, Paul Brace, Steven Brams, William Browning, Bernard Grofman, Seung-Hyun Kim, William McLaughlan, Elizabeth Rosenthal, and William Shaffer. Hsing-Pei Gary Kao and Seung-Hyun Kim assisted with the data collection.

1. The "swing ratio" is related to what we call *representation*. However, the swing ratio—the slope of an estimated linear relationship between seats and votes—is too restrictive to model the full range of nonlinear representational forms.

2. Because most nondemocratic regimes also have voting, the seats-votes relationship is important there as well. However, the mechanism for translation is obviously very different.

3. In *Davis v. Bandemer* (p. 2809), the Supreme Court recognized that previous cases "clearly foreclose any claim that the Constitution requires proportional representation or that legislatures in reapportioning must draw district lines as near as possible to allocating seats to the contending parties in proportion to what their anticipated statewide vote will be."

4. For most of his analyses, Tufte (1973) used a linear approximation to this form because at the time the logistic form was less commonly used and because the lack of extreme points in his data made the approximation relatively good. Since 1973, the logistic model has been considerably more popular; the figures in this paper will also aid in this interpretation. In addition, the data used in the bias section, as well as the data available from other electoral systems, have substantial numbers of extreme data points. Finally, as the figures will show, a linear approximation is not reasonable for many interesting and empirically common forms of bias and representation.

5. The bilogit form is formally undefined when $\rho = \infty$. As a result, it would be technically more appropriate to characterize the winner-take-all situation as $\rho \rightarrow \infty$.

6. Theil (1969, 521) describes a real electoral situation where the system was created to help the minority at the expense of the majority. Although this antimajoritarian representation system is unlikely to occur much in U.S. politics, it can be represented in the current scheme as $\rho < 1$.

7. In an interesting but wholly impractical proposal, Theil (1969, 524) suggests that citizens express a preference for ρ at the same time as they vote for candidates. Preferences for ρ would then be aggregated before translating votes into seats. Although it is not reasonable to "require the teaching of logarithms at an early stage" (1969, 524), this proposal does suggest the importance of the ρ coefficient in

standing for the type of representation in the electoral system.

8. If we used V_R and S_R in place of V and S , respectively, then the bias parameter would be $\beta_R = 1/\beta$. Thus, β is implicitly defined in terms of Democrats.

9. In systems where each party is guaranteed some minimum number of seats regardless of the outcome of the vote, it would be possible to include an additive parameter in Equation 4 to take this into account. However, for congressional elections, this is obviously not relevant.

10. This type of bias occurs, for example, in decisions of legislatures to seat members following closely contested elections. Since the majority party in the legislature can establish its own rules to count disputed ballots, the member seated may not be the candidate with the most votes. In our data, we assume that the candidate with the most votes wins the seat and ignore the bias that occurs if the legislature sat the "loser."

11. These values for β are empirically reasonable in the context of the analysis presented below.

12. Note that this demonstrates that a Gini index measure is inappropriate unless proportional representation is deemed the only fair representation system. If some form of majoritarian representation were considered acceptable, then comparing one of the biased curves in Figure 3 to be the fair proportionality curve in Figure 2 will be misleading; indeed, by that standard, even the fair majoritarian curve would be considered biased. It might be useful to generalize the Gini index and derive a measure of bias based on the area between the unbiased ($\beta = 0$ and ρ unconstrained) and the actual bias (β and ρ unconstrained) curves, thus incorporating all the absolute biases. But this is not needed, since $\ln\beta$ contains all of this information.

13. Indeed, many modern-day physicists do not believe that even the physical world is deterministic (see the discussion in Zellner 1984).

14. Niemi and Fett (1986) review several possibilities and conclude that their "historical swing ratio" is better than their "biyearly form." The main reason is that many data points are better than two. Curiously, Kowever, they conclude their article by favoring the "hypothetical (single-year) swing ratio." They give other reasons for preferring this method, but we do not conclude as they do that many data points are better than two, but that one datum is better than many. Our analysis will therefore use many data points collected over time.

15. The data were split into two nine-election-year samples for each state. The two sets each of β and ρ coefficients were each then correlated. Since positive infinity is a possible value for ρ , a simple measure of association was not possible. Omitting these values usually led to correlation coefficients of about .45. More revealing were scatterplots that indicated closer fits between the two time periods

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than any single coefficient could demonstrate. We also split the data to test whether the "one man, one vote" Supreme Court decision had an effect on ρ and β . As one would expect, we concluded that changes in state politics occur slowly. Incumbency, political parties, and geographical and constitutional factors prevent wholesale changes in the structure of representation and bias.

16. One might hypothesize that ρ is a linear function of a vector of explanatory variables, such as state size (X_1) and party competition (X_2): $\rho = \alpha_1 + \alpha_1 X_1 + \alpha_2 + X_2$. It might also be reasonable to hypothesize that β is a function of explanatory variables such as party strength (Z_1) and state ideological orientation (Z_2): $\beta = \gamma_0 + \gamma_1 Z_1 + \gamma_2 Z_2$. The right hand side of these two equations could then be substituted into Equation 5 to derive reduced-form estimates—resulting in a form analogous to interaction effects in regression analysis. This procedure is inapplicable here, however, because β and ρ are assumed to vary only across states. An alternative procedure is used below to express $\hat{\rho}$ and $\hat{\beta}$ as stochastic functions of these explanatory variables.

17. As is apparent from Equation 5, no estimates can be computed when a party received 0 votes for a year or when there is no variation in the number of seats across the years. For this reason, the coefficients of several of the states were not calculated on the full 18 years. The estimates from the following states are therefore not as reliable, since they have one or more years omitted (noted in parentheses): Georgia (1950, 1952, 1958), Louisiana (1950), Missouri (1954, 1956, 1958, 1962), South Carolina (1950, 1958, 1960), and Arkansas (1950, 1954, 1958). This and other causes of imprecision in the estimates are indicated in the standard errors in Table A-1.

18. It is likely that (1) less biased states (i.e., with smaller absolute values of $\ln\beta$) and (2) states where bias and representation vary independently will be closer to consistency than others, but there is no consistent way to know this or to estimate the bias parameter without the full joint estimation presented here.

19. The negative signs indicate bias toward the Republicans; the positive coefficients indicate Democratic bias. Most of the states listed in Table A-1 have estimated ρ and β coefficients within range of what one would expect. The exceptions are mostly those with large standard errors. These substantive points are more directly analyzed in the U.S. House.

20. The residuals from the two regressions correlated at only $-.117$ (t -value = $-.765$). A seemingly unrelated regression would therefore be of no help here.

21. If we let R stand for the raw Ranney index, the measure described as party competition in the text is calculated as $1 - 2|R - .5|$. This is the distance from being in between the parties, normalized

to a 0-1 scale.

22. For example, the architect of the recent and controversial California redistricting plan, late Congressman Phillip Burton (D-San Francisco), "did not deny that the gerrymander was alive and well in California. Burton publicly joked that his zigzagging district lines were 'our contribution to modern art'" (Lowell and Craigie 1985, 246).

23. In particular, no evidence of regional effects could be found: after controlling for the other variables in the equations, the South and other regions were not more likely to be characterized by different levels of bias or representation. There was also no interactive regional effect on the coefficients from the weighted-least-squares results.

24. The plurality in *Davis v. Bandemer* (n. 9) recognized that there was a range of fair seats-votes relationships that are not proportional. Justices White, Brennan, Marshall, and Blackmun's comment that their opinion "is not a preference for proportionality per se but a preference for a level of parity between votes and representation sufficient to ensure that significant minority voices are heard and that majorities are not consigned to minority status is hardly an illegitimate extrapolation from our general majoritarian ethic and the objective of fair and adequate representation recognized in *Reynolds v. Sims*."

25. It is useful to express relationships statistically even if there is no explicit sampling procedure. One only need conceptualize the dependent variable (Democratic seats, in this case) as a function of systematic (votes Democratic in the bilogit form) and random factors. The random factors are represented by a disturbance term with some known probability distribution.

26. Schrodtt finds that three of the five models "have the undesirable property of varying depending on which party is in the numerator and denominator" (1981, 36). (Our model avoids this problem: ρ is invariant and only the sign of $\ln\beta$ changes by using the Republican, instead of Democratic, party for V .) Of the remaining two, he concludes after empirical analyses that one "appears to have little utility" (p. 41). The final model seems best, but it produces the largest standard errors in applications. It also assumes that the disturbance term is log-normal, which may be appropriate at times, but "is not the sort of probability distribution for an error term that one is likely to choose by default" (Linehan and Schrodtt 1978).

27. Most probability distributions are eliminated by these axioms. The Poisson distribution is not bounded from above (King n.d.), and the truncated Poisson distribution does not meet the invariant requirement (Johnson and Kotz 1969). The *beta*-binomial and contagious binomial distributions are possibilities, but the latter makes implausible assumptions about sequential influence structures among congressional districts and the former is elim-

inated on empirical grounds.

28. We estimate $\ln\beta$ instead of β because it is more easily interpreted. Furthermore, because of the invariance property of maximum-likelihood estimation (DeGroot 1975, 291-92), the exponentiation of the estimate of $\ln\beta$ may be calculated in order to recover the maximum likelihood estimate of β .

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CONTROVERSIES

MACHIAVELLI'S PARADOX: TRAPPING OR TEACHING THE PRINCE

In The Discourses Machiavelli extolled the virtues of republican government, yet in *The Prince* he advised the ruler on how to perpetuate autocratic rule. What accounts for this paradox? Mary Dietz argues that Machiavelli sought to deceive the prince, trapping him into actions that would destroy his rule. John Langton contends, in contrast, that Machiavelli was seeking to teach the prince how to govern so that the autocratic state could evolve into a republic.

In his most substantial work, *The Discourses on the First Ten Books of Titus Livius*, Machiavelli defends the ideals of republican government by arguing on historical grounds that the most stable, solid, and humane states have been founded on social equality, political liberty, rule of law, popular elections, and mixed constitutions embodying a system of checks and balances. Yet in his best known book, *The Prince*, Machiavelli lays out an elaborate, "Machiavellian" program for acquiring and retaining autocratic power and thus seems to encourage a form of practical politics that vitiates the realization of his political values.

Students of political theory have proposed quite a few diverse solutions to this paradox. The prevailing view, it seems clear, is that Machiavelli is actually seeking in *The Prince* to teach an absolute ruler how to use his power to reform a corrupt and feeble state and thereby to lay the foundation for the emergence of a viable republic. In other words, according to the dominant interpretation, Machiavelli regards the absolutism he encourages in *The Prince* as the necessary precondition for the establishment of the kind of republicanism he endorses in *The*

Discourses.

Mary Dietz (1986) rejects this and other available attempts to reconcile the politics of *The Prince* and the values of *The Discourses* as unpersuasive. Instead, she advances the contention that *The Prince* is actually a "political act," "an act of deception," a piece of "duplicitous advice," designed to restore a republic in Florence by tricking a "gullible and vainglorious prince," Lorenzo de Medici, into implementing policies that would "jeopardize his power and bring his demise" (p. 781).

This is a novel and provocative thesis, but after analysis and reflection it strikes me as implausible and misleading. It is predicated on a selective, misguided reading of the relevant texts and an inadequate construal of both Machiavelli's fundamental values and his theoretical intentions. In exposing these flaws in Dietz's argument, I want to suggest that a particular "nationalistic" version of the dominant view of the relationship between *The Prince* and *The Discourses* offers a much more tenable interpretation than the one she devises.

Dietz (p. 782) essentially views *The Prince* as a well-disguised trap. The "text itself provides areas of 'solid ground,' or firm advice a new prince . . . can rely

upon to gain and maintain power." But hidden amid this solid counsel are "ditches and pitfalls" in the form of subversive directives," concealed by "promises of power, glory and popular support." My impression, in contrast, is that *The Prince* is too long, too historical, too involved, indeed altogether too judicious for this to be the case. Putting it another way, why is the solid ground around the putative pitfalls so well prepared—and so extensive?

Machiavelli examines many topics in *The Prince*, particularly in the area of foreign affairs, which have no direct bearing on the problem of maintaining power within a state. And he does so in a rigorous, bureaucratic "issues-and-options" style. Why did he not simply relate the solid counsel in a more straightforward, uncomplicated way? Indeed, if his goal was to bring Lorenzo down, why did Machiavelli not just write a short, snappy, meretricious memorandum on how to govern Florence instead of a densely packed handbook on *realpolitik* (which, by the way, a gullible and vain-glorious prince would probably not peruse)? The answer, I think it is clear, is that Machiavelli's purpose was instruction, not deception. Of course, it can be retorted here that the most effective trap always looks like something else, and this explains all the well-tended "firm ground" around the pitfalls in *The Prince*. But are there really consciously prepared pitfalls in the text? Or, to put the question in a more manageable form, Does *The Prince* contain any advice not consonant with what Machiavelli teaches in *The Discourses*?

Dietz contends that Machiavelli puts four main suggestions in *The Prince* that he hopes will induce Lorenzo to dig his own political grave: (1) that he should reside in the city of Florence itself; (2) that he should strive to gain the favor of the people; (3) that he should not build any fortresses; and (4) that he should create a

civilian militia or mass-based native army. Let us examine each prescription in turn to determine if it has a sinister intent.

Residing in a Conquered City (Florence)

As Dietz points out, according to Machiavelli, if a new prince wants to effectively govern a republic he has seized or conquered, he should either "destroy it" or "reside in it" (pp. 782–83). In *The Discourses* Machiavelli (1950, 183–84),¹ repeats the advice about destroying conquered cities or provinces in order to rule them securely, but he explains here that the destruction of a society in this context means the reorganization of its government, the redistribution of its population, and the restructuring of its stratification system. In other words, Machiavelli is apparently quite sincere when he advises Lorenzo that he has an option: to solidify his rule in Florence he can destroy the city or reside in it. What if Lorenzo had chosen the former option, which Machiavelli himself (1950, 184) describes as "the best means of holding a principality"? Why present an option here, thus giving Lorenzo a chance to follow a path leading away from the pitfall? Indeed, would any of Machiavelli's other alleged pitfalls be effective, if Lorenzo had elected to "destroy" Florence?

But what about the less radical option of residence within the city as a "means of securing possession"? Is this a trap? Dietz construes the suggestion to be a ploy to induce Lorenzo to abandon his country villa and take up residence in the city, where he could be more easily found and destroyed by a vengeful Florentine people still deeply imbued with republican sentiments (p. 783). Now, this argument turns on the assumption that the mass of Florentines had not forgotten "the name of liberty"—that they were, in fact, ardent republicans, just waiting for the

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opportunity to break the shackles of princely rule. Indeed, Dietz's whole thesis rests on the assumption that "the Florentines had become accustomed to a republic," that "in Florence, the idea of liberty was deeply rooted in political tradition" (p. 784). Yet, she herself quotes (p. 785) a passage from Machiavelli's *History of Florence* where he states that "liberty was unknown" in the city. Moreover, Machiavelli contends in *The Discourses* that republican values, traditions, and institutions were never strongly developed in Florence. The city had its "origin in servitude," and then

when afterwards the opportunity occurred for her to gain her liberty in a measure, she began by making a constitution that was a mixture of her old and bad institutions with new ones, and consequently could not be good. And thus she has gone on for the two hundred years of which we have any reliable account, without ever having a government that could really be called a republic. . . . And although Florence repeatedly gave ample authority, by public and free suffrage, to a few of her citizens to reform the government, yet these never organized it for the general good, but always with a view of benefiting their own party, which, instead of establishing order in the city, only tended to increase the disorders. (1950, 239-40)

The specific implication of this passage is that Machiavelli clearly does not see Florence as a hotbed of republicanism and certainly did not write *The Prince* under the illusion that most Florentines were chafing to regain their lost liberty. Indeed, all the evidence of *The Discourses* leads to the conclusion that Machiavelli regarded his countrymen as largely "corrupt" and "effeminate" (1950, 284-85, 369, 491). They were not a dangerous, rebellious mass in his estimation and the actual history of Florence after the fall of the republic in 1512 fully confirms this view.

The broader implication of Machiavelli's assessment of his native city is that it explodes Dietz's claim that the purpose of *The Prince* was to "restore" the previous Florentine republic (pp. 781, 794; see also Machiavelli 1950, 111). Machia-

velli did not think the old republic was worth restoring: its constitution was not good and its citizens, elites as well as common people, generally lacked civic virtue in the sense that their propensity was to put their private interests before the general good. He wanted the city reorganized, reformed, revitalized, and this, he emphasizes, requires the labor of an absolute ruler (1950, 110-11, 138-39, 166-67d, 170-71). But even that was not sufficient, because from Machiavelli's perspective, no Italian city-state, no matter what form of government it possessed, was really a viable geopolitical entity in a world dominated by large absolutist kingdoms. The only viable entity was the entire country of Italy, united under a strong (ultimately republican) government. As Machiavelli observed at one point in *The Discourses*,

A country can never be united and happy, except when it obeys wholly one government, whether a republic or a monarchy, as is the case in France and Spain; and the sole cause why Italy is not in the same condition, and is not governed by either one republic or one sovereign, is the Church. . . . The Church, then, not having been powerful enough to be able to master all Italy, nor having permitted any other power to do so, has been the cause why Italy has never been able to unite under one head, but has always remained under a number of princes and lords, which occasion her so many dissensions and so much weakness that she became a prey not only to the powerful barbarians, but of whoever chose to assail her. (1950, 151-52)

Gaining Popular Support

As Dietz relates, there is probably no more frequently repeated piece of advice in *The Prince* than that "the ruler should always strive to gain the favor of the people" (p. 783). Machiavelli not only reiterates this advice again and again but offers apparently prudent suggestions for implementing it (1950, 64-67, 85). How can practical advice about gaining popular support be a trap?

Dietz's conjecture is that Machiavelli's real view of the matter is just the opposite

of what he preaches in *The Prince*: in contrast to what he writes there, he actually believes that a dictator in Florence could best secure his position by gaining the support of the nobles, while distrusting and, if necessary, repressing the liberty-loving people. In other words, by telling Lorenzo through *The Prince* to be hostile to the nobility and to cultivate the favor of the people, Machiavelli was, in Dietz's view, attempting to trick the Florentine dictator into alienating his natural allies and trusting his natural foes. Dietz supports this conjecture in large measure through a strained interpretation of a memorandum written by Machiavelli to Pope Leo X, entitled "On Reforming the State of Florence."

Yet in *The Discourses* Machiavelli (1950, 162) says that he regards "as unfortunate those princes" who try to control a mass of hostile subjects with "extraordinary measures," such as "cruelty." This only erodes their authority. The "best remedy" for popular hostility is not repression but "to try to secure the good will of the people." And this can be achieved by, on the one hand, massacring the nobles and, on the other hand, allowing the people "to live in security," that is, to conduct government according to the rule of law (1950, 162-63). Whom is Machiavelli trying to deceive here? Although he employs a different argument, he repeats essentially the same advice in *The Prince* (1950, 35-36). Are both books, then, duplicitous? I think not. However Machiavellian their teachings may be, both works are sincere efforts to educate and to exhort.

Building Fortresses

In chapter 20 of *The Prince*, Machiavelli proffers some very equivocal advice about the practice of building fortresses. Indeed, the advice is so wishy-washy that it is difficult to imagine that it would persuade anyone to do anything. Yet Dietz

detects a clever trap (pp. 787-88). She thinks this chapter is intended to convince Lorenzo not to build a fortress, because Machiavelli really believes a central citadel would make the dictator less vulnerable to a republican insurrection. But is this, in fact, really Machiavelli's true belief? Dietz herself acknowledges in a footnote that in *The Discourses* Machiavelli unequivocally denounces the practice of erecting fortresses (p. 797). She does not, however, discuss his three main grounds for this advice. First, Machiavelli claims, fortresses are "injurious" because they induce the foolish presumption on a prince's part that he can systematically abuse his subjects and yet control them with force. In the long run this generates tremendous hostility, and no citadel can permanently protect a ruler from an aroused populace. Second, fortresses are "useless" against the artillery of a modern army (1950, 362-68). Finally, and most crucially, fortresses have a debilitating effect on the military capacity of a society: they foster the conviction that a large, well-trained army is unnecessary; whereas, as Machiavelli observes (1950, 369), fortresses cannot preserve states without good armies and they are unnecessary for states with them.

In light of this analysis, I cannot see how Dietz manages to convince herself that Machiavelli's advice on fortresses is a trap. But perhaps he is being duplicitous in *The Discourses* as well when he avers, "A good and wise prince . . . will never build fortresses, so that they may place their reliance upon the good will of their subjects, and not upon the strength of the citadels" (1950, 364).

Creating a Citizen Militia

A new ruler, indeed any wise ruler, Machiavelli strenuously argues in *The Prince*, should always have his subjects armed and trained for combat, and he should never rely on mercenary or auxili-

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ary troops (1950, 44–46, 48, 51–52, 77–78). For Dietz, this piece of advice is perhaps Machiavelli's most treacherous pitfall: "The new prince who arms his subjects may . . . easily make himself a mark for overthrow by creating the very instrument of his own destruction, namely, a civilian militia" (p. 786). Now obviously, arming and training a substantial body of citizens may "facilitate plots, incite insurrection, and inspire rebels." But what leads Dietz to maintain that this is what Machiavelli actually hoped would follow from his advice? She seems to offer two arguments. On the one hand, she intimates that Machiavelli had a high estimation for the republican commitments and revolutionary potential of his countrymen (pp. 786–87); but this, as I have tried to demonstrate, does not appear to be the case. On the other hand, she implies that Machiavelli knew that few successful dictators or princes had actually armed their subjects, and that, in fact, Lorenzo's own grandfather, Lorenzo the Magnificent, had actually disarmed the citizens of Florence in order to secure his dictatorial position. Nothing more graphically reveals Dietz's careless reading of the relevant texts and her misconstrual of Machiavelli's values than this argument.

According to Dietz, "when Machiavelli discusses such virtuous new princes as Francesco Sforza . . . or Cesare Borgia . . . he makes no mention of their having armed their subjects, doubtless because they did not. His bold claim that 'history is full of such examples' [of new princes who armed their subjects] is followed by no examples at all" (p. 786). But in point of fact, Machiavelli cites many examples, including the case of Cesare Borgia himself! As he explicitly points out (1950, 51), Borgia found his mercenary troops "uncertain to handle, unfaithful and dangerous." He therefore "suppressed them and relied on his own men. . . . [Thereafter,] his reputation . . . constantly increased, and he was never so highly esteemed as

when every one saw that he was the sole master of his own forces." Perhaps even more damaging to Dietz's argument, in chapter 21 of *The Prince*, a mere three pages after the point where Machiavelli claims that "history is full of examples" of new sovereigns arming their subjects, he discusses the case of his contemporary, "Ferdinand, King of Aragon, the present King of Spain." As Machiavelli observes (1950, 81–82), "Ferdinand . . . may almost be termed a new prince because from a weak king he has become for fame and glory the first king in Christendom, and if you regard his actions you will find them all very great and some of them extraordinary. . . . He was able with the money from the Church and the people to maintain his armies, and by that long war [with the Moors] to lay the foundations for his military power, which afterwards made him famous." For Machiavelli, this is a particularly instructive example, because Ferdinand did for Spain exactly what Machiavelli exhorts some "redeemer" to do for Italy in the famous last chapter of *The Prince*.

Although it now seems almost superfluous to say it, *The Discourses* is actually full of examples of absolute rulers who created popularly based armies—Tullus, Pelopidas, Epaminondas, the "current King of England" (1950, 175–76). In encouraging Lorenzo to establish a civilian militia, Machiavelli was hardly attempting to deceive and ruin the Florentine dictator; instead he was articulating one of his deepest convictions, namely, that "princes and republics of modern times as have no national troops for defense or attack ought well to be ashamed of it; for . . . if there are no soldiers where there are men, it is not owing to any natural or local defect, but solely to the fault of the prince" (1950, 175).

Dietz tries to make her argument about the duplicitous character of Machiavelli's advice by focusing exclusively on ques-

tions of domestic politics and internal power. But both *The Prince* and *The Discourses* address as well the other dimension of *realpolitik*, international affairs. And on this subject if there is one message which Machiavelli wants to drive home, it is the utter folly of being without a strong, indigenous army in a world filled with violent and voracious states. Again and again in *The Discourses*, Machiavelli describes Florence as "feeble," and she flounders in this execrable condition precisely because, having few troops of her own, she relies heavily on cowardly mercenaries (1950, 212-14, 285, 317, 369, 385, 491). When Lorenzo the Magnificent disarmed the Florentine people, he committed, from Machiavelli's perspective, a heinous political crime. Creating and maintaining a civilian militia poses risks to a prince's regime, but being without such a force guarantees his country's degradation, subjugation, and inevitable obliteration.

Political Values, Theoretical Intentions

Dietz's thesis rests substantially on the assumption that Machiavelli's only deep values are "republicanism and liberty" (p. 794). But Machiavelli was *also* a passionate nationalist. Indeed, if the last chapter of *The Prince* is a sincere cry for *risorgimento* and not, as Dietz suggests, one more piece of "bait" to trap a vain-glorious Lorenzo de Medici (p. 796), then it seems clear that the national unification, security, and glory of Italy were among Machiavelli's most cherished values. The destruction of Lorenzo and the restoration of a small, feeble, corrupt, and badly organized Florentine republic could do nothing for the achievement of these nationalistic aspirations. The establishment of a viable Italian state could, however, set the stage for the reintroduction and evolution of republican institu-

tions. If this was Machiavelli's view and if he shared it with friends and associates, then it is no wonder that he was not given a governmental position in the (ultimately weak and short-lived) Florentine republic that emerged in 1527 after 15 years of Medici rule. The problem was not so much that Machiavelli had sought a position with Lorenzo but that he had become, in terms of his partisan commitments, an Italian nationalist rather than merely a Florentine republican.

In Machiavelli's estimation, the first step in the process of creating a national state in Italy had to be the emergence of some autocratic leader who possessed the knowledge—and the motivation—to accomplish in Italy what Ferdinand had achieved in Spain.

But this is only one aspect of what Machiavelli wants. In *The Discourses* (1950, 116-17, 129-30, 166), he discloses his conviction that the Roman republic stands as the historical model of what a state should be. What Machiavelli ultimately hopes to foster through his writings is the rebirth in Renaissance Italy of the Roman republic in some modernized form. Viewed from this perspective, *The Prince* and *The Discourses* are easily reconciled. After a ruler had carried out the "moral" and practical program outlined in *The Prince*—that is, after he had secured control over a particular city-state, developed a strong, popularly based army, driven "barbarian" forces from Italy, and extended his control over the country—he could turn to *The Discourses*, perhaps near the end of his reign and under Machiavelli's own personal guidance, to learn how to give his new political creation the republican institutions and culture which would make it stable, long-lived and humane, thus ensuring its greatness and the prince's (and Machiavelli's) ultimate glory (see Machiavelli 1950, 145).

Whether Machiavelli actually believed that Lorenzo de Medici was up to this

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stupendous task, there is no question that he hoped he might be and that he would hire Machiavelli as chief consultant for the project. It is not too farfetched to say that Machiavelli wanted to do for Lorenzo what Count Cavour did for King Victor Emmanuel II in the late 1850s. In sum, Machiavelli was not trying to deceive and ruin Lorenzo; he was trying to educate him about the opportunity for, the demands of, and the glory to be gained from, the unification and political regeneration of Italy. This, of course, is not a novel thesis, but it has the merit of being much more plausible than the argument concocted by Dietz.

In criticizing the kind of reconciliation of *The Prince* and *The Discourses* proposed here, Dietz suggests that Machiavelli was too politically astute to entertain the naive belief that the "heroic politics" of a full-fledged, Machiavellian ruler would "somehow 'give away' to mass politics, that the death of the prince [would] lead to the rise of the republic" (p. 780). But a moment's reflection on what we know about the broad trajectory of European political evolution will show that this view, far from being naive, is incredibly prescient. Absolutism served as the precondition for the establishment of the modern nation-state, which in turn permitted the rise of mass politics. Indeed, this is what eventually happened in Italy. Alas for Machiavelli, he was 350 years ahead of his time.

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Perhaps, as Garret Mattingly once remarked, the puzzle of Machiavelli's *Prince* "has taken up more time and energy than it deserves" (Plumb 1961, 190). Nevertheless, Machiavelli remains a theorist ripe for controversy, and *The Prince* continues to fascinate and invite competing interpretations. In response to

my interpretation of *The Prince* as an act of political deception whose advice is intended to trap and destroy Lorenzo de Medici, John Langton resurrects the staple view of the nineteenth century. *The Prince*, he states, is a cry for national self-determination, and Machiavelli himself is an Italian nationalist, rather than an ardent republican. His familiar interpretation rests upon two general claims: first, that *The Prince* is a sincere and straightforward "handbook on *realpolitik*," written in a "rigorous, bureaucratic, 'issues-and-options' style," and second, that Machiavelli intended the "heroic politics" of the (nationalist) prince would give way (under Machiavelli's guidance) to the republican politics of the *Discourses*.

Since I addressed the second argument at some length in my article and since nothing new has been added to it by Langton, I will not reiterate my criticism of the "from-heroic-to-mass-politics" view here. Suffice it to say that I still find unconvincing the notion that Machiavelli believed that a Medici prince, once secure, would selflessly endow his regime with republican *ordini*, thereby hastening his own political demise. Langton's first claim, however—that *The Prince* contains honest nationalist advice—challenges my interpretation more directly. Let me turn to our particular disagreements over Machiavelli's "advice," after resituating the general problem historically.

Florentine Republicanism

At the center of my interpretation of *The Prince* as an act of political deception stand two key arguments: a historical one, that Florentine republicanism was a living reality when Machiavelli wrote his treatise, and a biographical one, that Machiavelli remained a republican not just from 1498 to 1512 but throughout his life. The latter argument gives him the impetus to plot against the Medici autocrat; the former gives him reason to

believe that the political environment would be hospitable to an overthrow if only the conditions to hasten such an event were in place. Langton casts doubt on both of these arguments by contending that after 1512 Florence was hardly a "hotbed of republicanism" and perhaps not accurately viewed as a city deeply rooted in republican traditions at all. Furthermore, Machiavelli himself "did not think that the old republic was worth restoring"; instead, he thought Florence would be better served by an absolute ruler who would "reorganize, reform, and revitalize" it in order to unify Italy. Thus, on two counts—for the city and for Machiavelli himself—republicanism becomes a dead letter, and "nationalism" rises in its stead.

These are indeed bold claims. As Langton notes, by questioning the republican tradition in Florence and Machiavelli's political preferences, he challenges the very context upon which my interpretation depends. But is he correct?

As we know from the discoveries of a generation of scholars, from Hans Baron and J. R. Hale, to Quentin Skinner and J. G. A. Pocock, Florentine thinkers in the fifteenth century developed a political theory celebrating the republican ideals of liberty, civic equality, and an arms-bearing citizenry (Baron 1961; Hale 1977; Pocock 1975; Skinner 1978). At the very least then, Florence seems to have been a "hotbed" of republican ideas. Moreover, thanks to the labors of recent historians, we know that Florence was also a city of strong civic republican practices—of "substantial and lasting" opposition to the Medici stretching back to their first regime in 1434, and of constitutionalist traditions espousing equality before the law, elections by lot, and freedom of speech (Brucker 1969; Hale 1977; Najemy 1982; Rubinstein 1968; Schevill 1936).

Langton comments upon none of this previous scholarship (not even to reject it). What he does declare is that "the

actual history of Florence after the fall of the republic fully confirms" that the Florentines were disinclined to attempt to regain their liberty. But to what "actual history" does he refer? The newly restored republic of 1527 may indeed have been "weak and short lived," but it was nevertheless born of a *popular rebellion* against the Medici lords, fully in keeping with a tradition of republican fervor and citizen opposition between 1434 and 1458 and again in 1466, 1478, and 1494, the year of Piero de Medici's overthrow. Hale, Rubinstein, and Schevill ably trace the story of popular unrest in the city after the return of Lorenzo. And if the Florentine propensity to revolt requires any further confirmation, we also have Machiavelli's own words. In advising Pope Leo X on "reforming" Florence, Machiavelli emphasizes the volatility of the city after 1512, warning that

the whole general mass of Citizens . . . are never satisfied—and whoever believes otherwise is not wise—unless you restore, or promise to restore to them their Authority. . . .

The general mass of Florentine Citizens will never be satisfied except the (Council) Chamber be reopened. . . . Therefore it is the better proceeding that You open it with secure methods and means, and that You take away from whoever was your enemy, the opportunity to reopen it against your will and with the destruction and ruin of your friends. (quoted in Pansini 1969, 635)

The picture Machiavelli paints for Pope Leo reveals a citizenry that cannot cast aside the memory of its ancient liberty. It does not portray, as Langton would have it, a populace immune to the promise of a republic and incapable of reclaiming its republican traditions. What Langton asserts, then, simply flies in the face of historical, textual, and political evidence.

Langton also emphasizes, and with reason, the critical attitude Machiavelli adopted toward his native city as well as his frequent attacks upon its factionalism, its political corruption, and its failure to achieve a stable government akin to the

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one he so admired in ancient Republican Rome and envied in Venice. Certainly no one, least of all Machiavelli, ever claimed that Florence was the pristine image of republicanism; the city was at best characterized by constant fluctuations between autocratic and republican rule. But do Machiavelli's realist appraisals mean, as Langton claims, that he had no interest in restoring the republic and instead welcomed the arrival of a state-building prince? I find no evidence of it.

In the first place, Machiavelli's criticism may be taken at face value—as evidence of his refusal to bow to idealistic visions and his awareness of what the Florentines were up against with regard to their political survival. Without question, as he acknowledges in his *History of Florence* and in reference to the Pazzi conspiracy, there were times in which “liberty was unknown” in Florence. The period of 1478—when the Medicean reaction against the republican revival of 1466 was in full swing—was certainly one such period. Likewise, Machiavelli was, without question, worried about the (in)ability of those cities “born in servitude” to effect successful republican regimes. But nowhere—including in the passage Langton takes as evidence of Machiavelli's disdain for republicanism in Florence—does he say that Florence would be better off governed by an absolute prince with nationalist aspirations. As Hale has observed, neither Machiavelli nor his contemporaries were asking themselves, “Should Florence be governed by a republican constitution or by an absolute prince?” Rather, Machiavelli's concern was with the question, “What qualities should our republic show the outside world, and how can our sick state have its vital tone restored?” (Hale 1961, 181). Thus, Machiavelli does not think, pace Langton, that once destroyed, a republic is best not recovered but rather that once destroyed (or overthrown), a republic faces its greatest difficulty and politics its most impor-

tant challenge—the restoration of its liberty and civic *virtù*.

As he writes of Rome in the *Discourses*, “in a great republic there are constantly evils occurring requiring remedies which must be efficacious in proportion to the importance of the occasion” (1950, 538). Much the same could be said of Florence, a far less glorious republic to be sure, but a republic nonetheless and in need of “efficacious remedies” to restore it to health. This is the issue that Machiavelli, as political theorist, political actor, and Florentine patriot, faces head on with historical creativity in the *Discourses*, with detailed advice about the restoration of republican *ordini* in “Reforming the State of Florence,” and with craft and cunning in *The Prince*.

Pitfalls in The Prince

The force of Langton's criticism turns upon a general thesis about Machiavelli and Florentine republicanism that lacks biographical, historical, and textual support. I will return to this in closing. But now, what of his specific counters to the “pitfalls” I uncover in *The Prince*, especially regarding the Medici residence, the civilian militia, and the building of fortresses? I am afraid that none of Langton's counters succeed, much less do they require abandoning an interpretation of Machiavelli's pieces of advice as traps for the Medici prince.

On the issue of residence, Machiavelli advises Lorenzo either to destroy the city or reside in it. The first choice, far from leading away from the “pitfall,” actually draws the prince toward it. The destruction of Florence, as Machiavelli knows, is an outrageous suggestion and a practical impossibility. It renders the truly dangerous second choice—residing in the city—as the only attractive alternative. Thus, Machiavelli would give Lorenzo the illusion of choice even as he narrows the

prince's range of possibilities. This is how Machiavelli usually conceives of the complex world of choices and deceptions; consider also Ligurio's ploys in *Mandragola*, where he often plays an outrageous choice off against one that seems reasonable to old Nicia but one which will actually compromise him.

As to the danger of residing in the city, we might recall that the history of republican upheavals in Florence was in part one of the people's taking and retaking the Palazzo Medici, which was vulnerable to mass action in a way the Medici villas in the Tuscan hills were not. Perhaps Machiavelli fondly remembered the ouster of Piero in 1494, when a vengeful Florentine populace drove the family from the palazzo, dragged Donatello's *Judith* from the family gardens, and set it up before the palace of the Signoria with a new inscription warning would-be tyrants and praising civic liberty. Such an event—of practical political as well as symbolic importance—could not have occurred had the Medici prince been fortified beyond the city walls. As Machiavelli understood, to oust a prince a people must have not only the spirit but also the opportunity to get at him.

The matter of Machiavelli's advice concerning "whom to arm" is even more important. I argue that in advising Lorenzo in chapter 20 to "keep his subjects armed" and make "partisans" of them, Machiavelli sets still another trap. He lays the groundwork for a new Florentine civilian militia that could contribute mightily to the destruction of the Medici regime. To convince Lorenzo that new princes regularly arm their subjects, Machiavelli appeals to history but he (uncharacteristically) offers no historical examples at all to illustrate the aptness of his advice. Thus, he presents the warrant of history to Lorenzo, but the warrant is in fact a sham, for the advisor knows that the practice he describes as routine for new princes is rare.

Langton, however, claims that Machiavelli offers at least two examples (in chaps. 5 and 21) of new princes who armed their subjects. In Cesare Borgia and Ferdinand of Spain, Machiavelli allegedly provides models whom Lorenzo can use for the course of action outlined in chapter 20. Leaving aside the obvious question—if Borgia and Ferdinand were such good examples, why did Machiavelli not mention them in Chapter 20?—let us turn instead to a more vital question: are Borgia and Ferdinand, in fact, examples of new princes who "always had their subjects armed?" On this score, I would suggest that it is Langton who advances the "careless reading." His misreading hinges on what should be the obvious difference between a "civilian militia" of the kind Machiavelli recommends to Lorenzo, and a "private army" or "national troops" which he credits Borgia and Ferdinand, respectively, with establishing. To put this otherwise, Machiavelli surely appreciates that in the Romagna (the greatest source of mercenaries) Borgia relies upon "his own men" and that in Spain Ferdinand taxed the people to build up his military might. As a result, neither was beholden to foreign soldiers. But nowhere does Machiavelli equate their actions with the creation of an *arms-bearing citizenry*. And it is the latter that he counsels Lorenzo to create in chapter 20, when in essence he advises the rearming of a formerly republican city. Thus, the examples of Borgia and Ferdinand are neither apt nor relevant in this context. In fact, the example of Borgia could be counterproductive, so Machiavelli does not mention him, just as he does not mention *il Magnifico*, Lorenzo's grandfather, who *disarmed* the Florentines and was the most successful of all the Medici lords.

These examples are important. Nevertheless, I think that by seizing upon them, Langton ultimately dodges the most important question of all. Regardless of whether Borgia or Ferdinand fit the bill, is

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Machiavelli's advice on arming one's subjects helpful counsel for a Medici in Florence? Langton himself concedes that "creating and maintaining a civilian militia poses risks to a prince's regime." Precisely so! But in admitting as much he simply begs the crucial question; he does not resolve it, nor does he persuasively challenge the contextual and historical evidence I present in order to reveal Machiavelli's deception.

To counter my interpretation of Machiavelli's advice on fortresses, Langton turns to the *Discourses*, and notes that there, too, Machiavelli is chary of fortress building and urges that the prince rely instead upon the good will of his subjects. He seems to imply that on matters where there is no contradiction between *The Prince* and Machiavelli's other writings (especially the *Discourses*) there must be no deception at work in *The Prince*. I see no reason to accept such an interpretation, unless one assumes (as I do not) that everything Machiavelli expresses elsewhere can be read as the "truths" that expose the "lies" of *The Prince*. If, however, this is Langton's presumption, he needs to be more consistent in his application of it, and acknowledge not only those passages that seem to point to a sameness in Machiavelli's advice, but also account for those passages where striking contradictions appear. He makes no attempt, for example, to explain the advice against fortress building in *The Prince* in terms of Machiavelli's letter to Guicciardini, where Machiavelli equates the successful Medicean conquest of Florence precisely with the building of a *fortezza*. Nor—to take another example of importance in my essay but unacknowledged by Langton—does he confront the counsel against liberality in *The Prince* with Machiavelli's treatment of it in the *History of Florence* as a valuable Medicean tactic to maintain power. Nor does Langton try to explain how Machiavelli's dictate to Lorenzo to "build upon the people" and be wary of

the nobility squares with his straightforward comment to Pope Leo that a prince in Florence "despoiled of Nobility cannot sustain the burden of the Principality," and thus must create a "middle group" between himself and the general public (Pansini 1969, 620). By simply asserting that the latter is a "strained interpretation," Langton again sidesteps the intriguing and difficult issues. He certainly offers no explanation for them.

Pitfalls aside, Langton's interpretation fails finally to confront the methodological premise behind my reading of *The Prince*—that genuinely historical study is the indispensable precondition for interpreting political texts of the past. Other than a few (unsubstantiated) assertions about Florentine republicanism, Langton offers no historical reading of Machiavelli's treatise and uses descriptions the author could not in principle have accepted as his. So, for example, we are given the nineteenth-century language of "nationalist aspirations" or the twentieth-century conception of a "viable geopolitical entity." Furthermore, Langton would have us accept an ahistorical description of *The Prince* and the *Discourses* as works of political literature. As noted, he describes the former as written in a "bureaucratic 'issues-and-options' style," and rhetorically he asks why, if my interpretation is correct, did Machiavelli not simply write a "short, snappy, meretricious memorandum?" But these are anachronisms. The bureaucratic memorandum and the "white paper" were not literary options in the fifteenth century, and within the existing genre of the Mirror of Princes tracts *The Prince* is remarkably short and snappy.

In his conclusion, Langton shifts from an ahistorical to a suprahistorical interpretation of Machiavelli's intentions. He praises the Florentine republican for having the prescience to anticipate nothing less than "the political evolution" of Europe down through the nineteenth cen-

ture. Among its lesser faults, this credits Machiavelli with a vision of political creation as a linear progress, a view he did not hold nor could have held. To the contrary, his own clearly expressed vision of the cyclical movement of glory, decay, and regeneration, or *anacyclosis*, has more to do with the *revolution* of order and disorder than with the *evolution* of "mass" from "absolutist" politics (D'Amico 1984, 132). Even more troubling, however, is Langton's evident willingness to subscribe to an overly simplified view of European history—only to reconstitute Machiavelli's contribution both to it and to political thought more generally as a grand moment in the telos of the modern nation-state. Writing history backwards, Langton would have us understand Machiavelli as "350 years ahead of his time"! Commentators may well take issue with this or that interpretation of *The Prince*, including the "politics of deception" I find there. But I would like to suggest that we can make headway in our controversies over the meaning of Machiavelli's little treatise only if we return it to the period in which it was written and examine Machiavelli's intentions within his own time, not ahead of it.

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VALUE CHANGE IN INDUSTRIAL SOCIETIES

Ronald Inglehart has argued that, while most of the major political parties in Western countries tend to be aligned along a social class-based axis, support for new political movements and new political parties largely reflects the tension between materialist and postmaterialist goals and values. This has presented something of a dilemma to the traditional parties, and helps account for the decline of social-class voting. Scott Flanagan takes issue with Inglehart's interpretation in several particulars. Although their views converge in many respects, Flanagan urges conceptual reorientations and adumbrates a different interpretation of post-World War II political development in Europe and Japan.

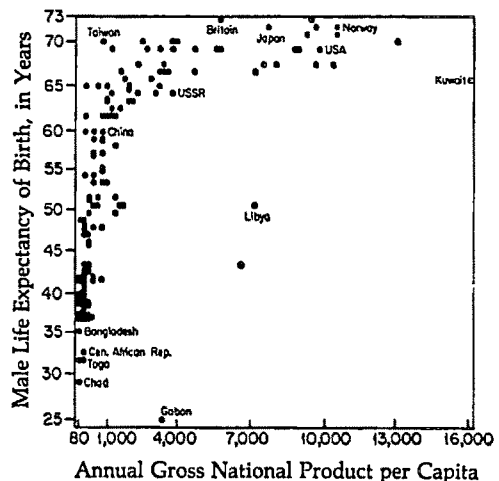
Diminishing Marginal Utility at the Societal Level

Ideology lags behind reality. Though Karl Marx died in 1883, his analysis of political conflict continued to fascinate, and sometimes mesmerize, social critics and social scientists for much of the following century. His thesis captured an important part of reality for the early phases of industrial society. But with the evolution of advanced industrial society new conflicts and new world views have emerged, making the economic conflicts Marx emphasized less central to political life.

This development reflects a principle that might be called the diminishing marginal utility of economic determinism: economic factors tend to play a decisive role under conditions of economic scarcity; but as scarcity diminishes, other factors shape society to an increasing degree. In this article we will examine evidence of this phenomenon, first from an aggregate cross-national perspective, then with time-series data at the individual level. The two types of evidence converge, pointing to a diminishing degree of both

economic determinism and class-based political conflict as advanced industrial society emerges. Figure 1 provides an illustration.

**Figure 1. The Diminishing Marginal Utility of Economic Determinism:
1975 Male Life Expectancy at Birth,
by GNP per Capita**



Source: Based on data from Charles L. Taylor and David A. Jodice, *World Handbook of Political and Social Indicators* (Cologne: Zentralarchiv für empirische Sozialforschung, 1982), 3:1948-77.

As Figure 1 demonstrates, human life expectancy is closely linked to a nation's level of economic development, especially at the low end of the economic continuum: it is virtually impossible for poor nations to attain a high average life span. A large number of countries cluster tightly together at the low end of the spectrum on both income and life expectancy, with such nations as Chad or Bangladesh showing per capita gross national products below one hundred dollars and a male life expectancy of 35 years or less in 1975. Just above them is a group of 20 nations having per capita GNPs of less than three hundred dollars and life expectancies of less than 45 years.

The curve rises steeply with relatively modest increases in wealth, until it reaches about two thousand dollars per capita. Thereafter, the curve levels off. Economic factors become less decisive, and life-style factors more so: longevity becomes less and less a question of adequate nutrition and sanitation facilities, and more and more a question of cholesterol intake, tobacco and alcohol consumption, exercise, levels of stress, and environmental pollution.

The leveling off of the curve does not simply reflect ceiling effects. In 1975 only a few nations had male life expectancies above 70 years; but in the ensuing decade, several nations raised them by several years. By 1982, female life expectancy in Switzerland had risen to 81 years, and this almost certainly does not represent the ultimate biological ceiling. Most developed nations still have considerable room for improvement—but it is no longer so closely tied to economic development. Thus, despite rising income, male life expectancy in the Soviet Union has *declined* in the last decade, apparently as a result of rising alcoholism rates.

As our hypothesis implies, a logarithmic transformation of GNP per capita shows a much better fit with life expectancy than does a linear model: the

former explains 61% of the variance in life expectancy, and the latter only 37%. Figure 1 shows untransformed GNP per capita in order to demonstrate the diminishing impact of economic gains directly. Another illustration of this phenomenon is the fact that among the poorer half of these nations, raw GNP per capita explains 44% of the variance in life expectancy; while among the richer half it accounts for only 14%. Similar patterns of diminishing returns from economic development are found with numerous other indicators of the quality of life. Caloric intake, literacy rates, the number of physicians per capita, and other objective indicators all rise steeply at the low end of the scale but level off among advanced industrial societies.

Equality of income distribution also increases sharply with economic development, up to a level of about 35 hundred dollars per capita; but above that threshold there is virtually no further rise. Figure 2 shows this curve (inverted by comparison with Figure 1, since high scores on the vertical axis represent high levels of *inequality*). In 70% of the nations with a GNP per capita below 35 hundred dollars, the top tenth of the population got more than one-third of the total income (in some cases as much as 57%). In *none* of the nations with a GNP per capita above 35 hundred dollars did the top tenth of the population get more than one-third of the total income; their share ranged from as low as 17% in Communist countries, to a high of 33% in Finland.

Does this cross-sectional pattern reflect a longitudinal trend? The point has been debated. The most reliable longitudinal data come from economically advanced countries which have shown little increase in income equality during the past 30 years. But if we interpret the pattern as reflecting a curve of diminishing returns rather than a linear trend, this finding is exactly what we would expect: it is only

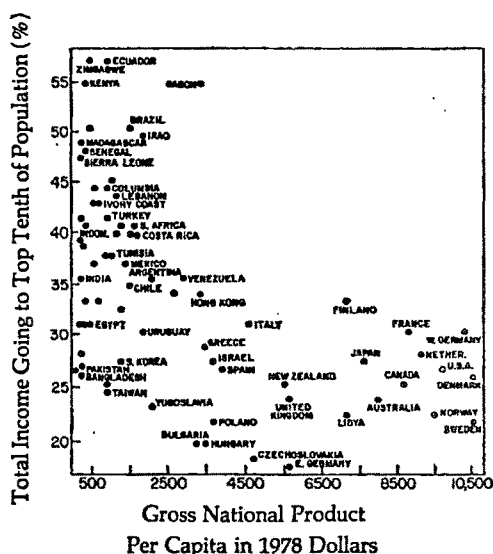
Values in Industrial Societies

in the earlier stages of economic development that we would find large amounts of change. The United States, for example, moved toward substantially greater income equality from 1890 to 1950, but has shown little change since then: absolute levels of income continued to rise, but relative shares changed only slightly. Conversely, Taiwan, South Korea, Singapore and Hong Kong have made a dramatic leap from poverty to prosperity only recently—and all have shown significant increases in income equality (Chen 1979).

Why do we find a curvilinear relationship between economic development and income equality? In the early phase, we believe, it reflects a process of social mobilization, engendered by economic development: industrialization leads to urbanization and mass literacy, which facilitate the organization of labor unions and mass political parties and the enfranchisement of the working class. Economic development does not necessarily have this effect, but it enhances the chances of transforming the masses from isolated and illiterate peasants into organized citizens with the power to bargain for a more equal share of the pie.

But why does the curve level off among mature industrial societies? There are two main reasons. First, as one approaches perfect equality, one necessarily reaches a point of diminishing returns. If one reached the point where the top tenth had only 10% of the income, any further transfer of income would be a move *away* from equality. None of these societies has actually reached this point, but some are getting close: in East Germany, the top tenth gets only 17% of the total income, according to official sources (these figures may exaggerate the degree of income equality in Eastern Europe: a Czech analysis cited by Connor [1979, 216–18] estimates that income equality in Eastern Europe is no greater than in some Western countries). In any event, there seems to be

Figure 2. Economic Development and Income Equality



Source: Based on data from Charles L. Taylor and David A. Jodice, *World Handbook of Political and Social Indicators* (Cologne: Zentralarchiv für empirische Sozialforschung, 1982), 3:1948–77.

a practical limit, for all societies use different rates of pay to motivate their labor force. China, during the Great Cultural Revolution, may have emphasized economic equality more heavily than any other nation in modern times, but some income differences were still retained. To motivate the people, the regime relied on a combination of intense moral exhortation and coercion. This system had severe drawbacks; the current Chinese regime has shifted toward less coercion and more reliance on economic incentives. The fact that Norway, Sweden, and Denmark have significantly greater income equality than the United States, West Germany or France, indicates that the latter countries could move farther toward equality without becoming ineffective economies, or coercive societies. But the Scandinavian countries seem to be approaching the limit

of what is possible in a democratic political system.

Why this is so, reflects a second basic principle: political support for increased income equality reaches a point of diminishing returns at a level well short of perfect equality. Przeworski and Wallerstein (1982) have demonstrated through mathematical modeling that under appropriate conditions workers and capitalists would reach a compromise in rational pursuit of their material interests: capitalists would consent to democratic institutions through which workers can effectively press claims for material gains. And workers would consent to profits in the expectation that they will be invested productively, improving their future material gains. Thus, given democratic institutions, some inequality of income may be acceptable to the lower-paid groups.

But as one moves closer to an equal income distribution, the political base of support for further redistribution becomes narrower. In a poor society where the top 10% gets 40%–60% of the total income, the vast majority would benefit from redistribution. In a society in which the top tenth gets only 20%–25% of the total income, far fewer people will benefit from further redistribution, and they will benefit proportionately less; a majority may even stand to lose by additional redistribution more than they would gain. This does not constitute a moral justification for not moving further toward equality; but it does constitute a serious practical problem. Under these conditions, the political base for further development of the welfare state is simply not there, at least not in so far as the citizens are motivated solely by economic self-interest. Ironically, further progress toward equality would come *not* from an emphasis on materialistic class conflict, but through an appeal to the public's sense of justice, social solidarity, and other nonmaterial motivations.

Is there any chance that such appeals

would work? We believe that there is. In the long run there seems to be a tendency for the pursuit of economic self-interest itself to reach a point of diminishing returns in advanced industrial societies, and gradually give way to postmaterialist motivation, including greater emphasis on social solidarity (Inglehart 1971, 1977). A large body of survey data, gathered during the past 15 years, suggests that economic development makes a sense of economic deprivation less widespread among mass publics, and consequently, a less important cause of political conflict.

This conclusion is not surprising. After the fact, it may even seem self-evident. It is not: it has been hotly debated and cannot be viewed as conclusively proven even now. A quarter of a century ago, the end-of-ideology school concluded that growing prosperity was giving rise to a *politics of consensus in an age of affluence*; the subsequent explosion of protest in the late 1960s led many to conclude that this school had been completely wrong. In fact, their analysis of what had been happening was partly correct; like Marx, they simply failed to anticipate new developments. While economic cleavages become less intense with rising levels of economic development, they gradually give way to *other* types of conflict.

Thus far we have seen indications that economic development leads to a diminishing impact of economic influences on such objective characteristics as life expectancy and economic equality. But do such changes actually reshape the political preferences of mass publics?

The evidence suggests that at high levels of economic development, public support for the classic economic policies of the Left tends to diminish. Table 1 sums up the responses of eleven Western publics to a set of questions dealing with three key issues underlying the classic Left-Right polarization. These questions were asked in Euro-Barometer surveys carried

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**Table 1. Support for the Classic Economic Policies of the Left
by Level of Economic Development, 1979-1983**

Nation (Ranked by per Capita Gross Domestic Product)	1982 GDP per Capita (European Currency Units)	Reducing Income Inequality	Percentage in Favor of ^a		
			More Government Management of the Economy	More Nationalization of Industry	Mean for 3 Issues
1. Greece	3,958	95	82	80	86
2. Ireland	5,408	90	72	64	75
3. Italy	6,287	88	68	36	64
4. Northern Ireland	6,852	76	65	57	66
5. Belgium	8,735	87	53	42	61
6. Great Britain	8,755	73	64	46	61
7. Luxembourg	9,407	82	60	31	58
8. Netherlands	9,830	78	64	32	58
9. France	10,237	93	63	44	67
10. Germany	10,927	80	52	41	58
11. Denmark	11,194	71	57	19	49

Source: Based on combined results from Euro-Barometer nos. 11, 16, and 19; respective Ns are 8,884 for 1979; 9,909 for 1981; and 9,790 for 1983. See ICPSR codebooks for details concerning fieldwork. Data on GDP per capita are from Eurostat, *Structural Data* (Luxembourg 1985).

^aText of questions: "We'd like to hear your views on some important political issues. Could you tell me whether you agree or disagree with each of the following proposals? How strongly do you feel [show card] (1) Greater effort should be made to reduce income inequality. [Those who said they *agree* or *agree strongly* are classified in this table as favorable.] (2) Government should play a greater role in the management of the economy. (3) Public ownership of industry should be expanded. In the 1983 survey, the polarity of Item 2 was reversed: while 70% of the 10 publics agreed that government should play a *greater* role in 1981, only 52% rejected the proposal that government should play a *smaller* role in 1983. For Item 2, these figures are based on the 1981 and 1983 results, only, giving equal weight to the two formulations. Missing data are excluded from percentage base.

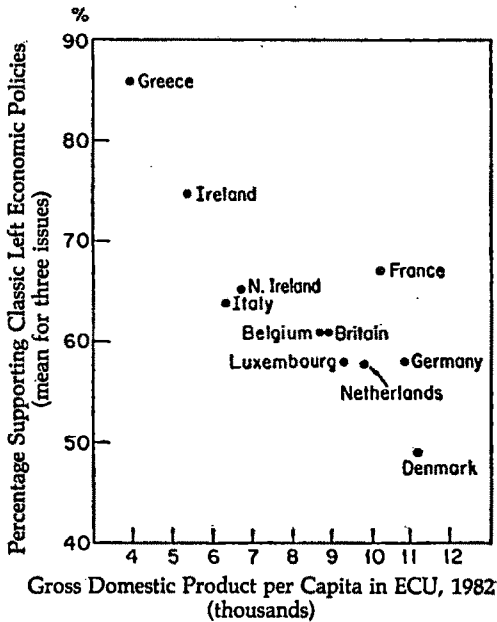
out in 1979, 1981, and 1983 in the 10 member nations of the European Community (Greece, not yet a member, was not surveyed in 1979). The questions deal with redistribution of income, government control of the economy, and nationalization of industry—the central elements of the traditional Left's prescription for society. They were worded as follows:

We'd like to hear your views on some important political issues. Could you tell me whether you agree or disagree with each of the following proposals? How strongly do you feel? (1) Greater effort should be made to reduce income inequality; (2) Government should play a greater role in the management of the economy [in 1983, this item was reversed, to refer to "a smaller role" and its polarity recoded accordingly]; (3) Public ownership of industry should be expanded.

Though a majority supports greater income equality in every country, while further nationalization of industry is rejected by majorities everywhere except in Greece and Ireland, the *relative* levels of support for these three policies among the publics of given nations show impressive consistency both across items and across time. Taken together, the results add up to a remarkably clear picture of which publics are most favorable to the classic Left economic policies—and the picture does not correspond to conventional stereotypes.

Everyone knows that Denmark is a leading welfare state, with advanced social legislation, progressive taxation, a high level of income equality, and well over half the GNP going to the public sec-

Figure 3. Support for the Classic Economic Policies of the Left by Level of Economic Development, 1979-83



Note: Based on responses to three items, asked in three surveys.

tor. Obviously, the Danish public must be relatively favorable to these traditional policies of the Left. Conversely, everyone knows that Ireland is a largely rural nation, with a modest public sector and no significant Communist or Socialist movements. Clearly, Ireland must be a bastion of conservatism on the classic Left-Right issues.

In fact, the conventional stereotypes are dead wrong on both counts. The stereotypes reflect patterns that were true in the past, but precisely *because* Denmark has now attained high levels of social security—and very high levels of taxation—the Danish public has little desire for further extension of these policies. Instead, support for the classic economic policies of the Left tends to reflect a

nation's level of economic development. As Figure 3 demonstrates, Greece is by far the poorest country among the 11 societies surveyed in 1979-83; and the Greek public has by far the highest level of support for nationalization of industry, more government management of the economy, and reducing income inequality. Ireland is the second poorest country, and overall Ireland ranks second in support for these policies. At the opposite end of the spectrum, Denmark is the richest country and has the lowest level of support for these policies. West Germany ranks next to Denmark in economic level and also in support for the classic Left policies.

The French show more support for these policies than their economic level would suggest, which may be linked with the fact that France also has an incongruously high level of economic inequality. But France constitutes the only significant anomaly. The other 10 societies show an almost perfect fit between level of economic development and support for the classic economic policies of the Left.

These findings suggest that the principle of diminishing marginal utility applies at the societal level, as well as the individual level. Greece is an economically underdeveloped country, with many living in extreme poverty and a small affluent elite. In such a context, the balance between rich and poor can be redressed only by strong government intervention. Denmark, on the other hand, is a rich country that has long since developed some of the most advanced social-welfare policies in the world and one of the world's highest rates of taxation. Almost 60% of the GNP is spent by the government, approaching the point where it becomes impossible to move much farther in this direction (even in the Soviet Union, the government's share probably isn't over 75%). In Denmark, further redistribution by the government seems less urgent than in Greece; and the costs of government intervention impinge on a larger share of the popula-

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tion. The incentives to press further with the traditional economic policies of the Left become relatively weak, and public resistance relatively strong.

In this sense, the policies that dominated the agenda of the Left throughout most of this century are running out of steam. Increased state intervention was desperately needed to alleviate starvation and social upheaval in the 1930s; was essential to the emergence of the welfare state in the postwar era; and still makes sense in some areas. But in others, it has passed a point of diminishing returns. The renewed respect for market forces that has emerged throughout most of the industrial world recently reflects this reality. It can be debated whether Gary Hart had come up with the right "new ideas" in 1984, but his recognition of the need for a new liberal agenda was well founded. His rival, Walter Mondale, doggedly but haplessly stuck to the New Deal formula a half century after the New Deal.

The neoconservative claim that the classic welfare state policies have failed is false, however. Quite the contrary: in countries like Denmark these policies have largely solved the problems they are capable of solving and have thereby reduced the demand for more of the same. Insofar as they *succeed*, they reach a point of diminishing returns and begin to cede top priority to problems that have *not* been solved.

Any attempt to turn back the clock to the savage laissez-faire policies of the early twentieth century would be self-defeating, ultimately leading to a resurgence of class conflict in all its former harshness. But the fundamentalists of the Left are equally self-defeating in their rigid adherence to a traditional program based on class conflict and state ownership and control of the means of production.

This does not mean that economic factors are no longer politically important. On the contrary, some of the most signifi-

cant recent research in political science has demonstrated strong linkages between fluctuations in the economies of Western nations, and support for the incumbent political party (Hibbs, Rivers, and Vasilatos 1982; Kramer 1971; Tufte 1978). But this research has also produced a surprising finding: while support for the incumbents *does* reflect the performance of the national economy, it does *not* seem motivated by individual economic self-interest. The electorates of advanced industrial societies do not seem to be voting their pocketbooks, but instead seem primarily motivated by "sociotropic" concerns: rather than asking "What have you done for me lately?" they ask "What have you done for the *nation* lately?" (Kinder and Kiewiet 1981). Recent research carried out in France, West Germany, Italy, and Great Britain confirms that there—as in the United States—the linkage between the economic cycle and support for the incumbents is overwhelmingly sociotropic (Lewis-Beck 1986).

In short, economics remains an important influence on electoral behavior, but it reflects sociotropic motivations rather than class conflict. The politics of advanced industrial societies no longer polarize primarily on the basis of working class versus middle class; and the old issues, centering on ownership of the means of production, no longer lie at the heart of political polarization.

Political Change at the Individual Level

The argument presented above is implicit in the materialist-postmaterialist value-change thesis; it is new only in its application to the societal level. In this section we examine political change at the individual level. Here we have a rare opportunity: the chance to test a set of predictions about social change that were

published years before the data by which they are tested came into existence.

At the individual level, our hypothesis concerning the diminishing role of economic factors, is supplemented by a second basic hypothesis: that early-instilled values tend to persist throughout a given individual's life. In context with the unprecedented economic development of the postwar era, these hypotheses imply a shift from materialist toward postmaterialist values. At the individual level, we should find sizable and persisting differences between the value priorities of young and old, reflecting their differing formative experiences; but at the societal level, this shift will manifest itself only gradually, as one generation replaces another. Moreover, because this shift involves basic goals, it implies a gradual change in the types of issues most central to political conflict and in the types of political movements and parties people support. Finally, it also implies a decline in social-class voting and increasing polarization over noneconomic values (Inglehart 1971, 1977).

The intergenerational value differences these hypotheses predict have now been explored extensively, in 26 different nations. Survey after survey reveals dramatic differences between the goals emphasized by old and young. Moreover, cohort analysis demonstrates that there is no tendency for given birth cohorts to become more materialist as they age, as they would if these differences reflected life-cycle effects. In 1985, given birth cohorts were fully as postmaterialist as they had been 15 years earlier. There were significant short-term fluctuations, reflecting period effects linked with inflation (Inglehart 1985a, 1985b). But by 1985, inflation had subsided almost to the 1970 level. With period effects held constant, there is no sign of the gradual conversion to materialism that would be present if a life-cycle interpretation were applicable. When short-term forces

returned to normal, a substantial net shift toward postmaterialism was manifest, most of it due to intergenerational population replacement (Abramson and Inglehart 1986).

The predicted intergenerational value shift seems to be confirmed by a massive amount of empirical evidence, but the predicted changes in prevailing types of political cleavages have barely been touched on. Let us examine the relevant evidence.

From Class-based to Value-based Political Polarization

The idea that politics is a struggle between rich and poor can be traced to Plato. But unquestionably the most influential modern version of this idea has been Marx's argument that throughout industrial society, social class conflict is the central fact of political life, and the key issue underlying the Left-Right polarization is conflict over ownership of the means of production. Marx's influence is reflected not only in a vast literature of social criticism but also in the existence of an entire family of political parties that were inspired by his writings and, in varying degrees, purport to be guided by his analysis. The rise of postmaterialism makes this analysis less adequate today. Let us consider why.

The postmaterialist outlook is linked with one's having spent one's formative years in conditions of economic and physical security. Hence it is more prevalent among the postwar generation than among older cohorts, and tends to be concentrated among the more prosperous strata of any given age group.

The political implications are significant and at first seem paradoxical. Postmaterialists give top priority to such goals as a sense of community and the non-material quality of life, but they live in societies that have traditionally empha-

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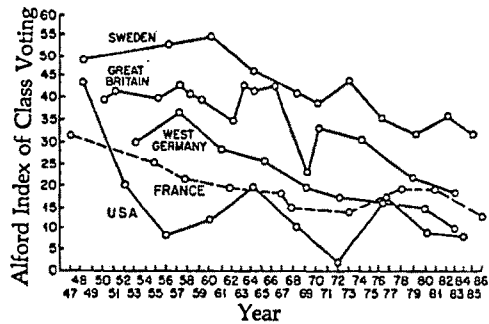
sized economic gains above all, even at the expense of these nonmaterial values. Hence, they tend to be relatively favorable to social change. Though recruited from the higher-income groups that have traditionally supported the parties of the Right, they tend to shift toward the parties of the Left.

Conversely, when postmaterialist issues (such as environmentalism, the women's movement, unilateral disarmament, opposition to nuclear power) become central, they may stimulate a reaction in which part of the working class sides with the Right, to reaffirm the traditional materialist emphasis on economic growth, military security, and domestic order.

The rise of postmaterialist issues, therefore, tends to neutralize political polarization based on social class. Though long-established party loyalties and institutional ties link the working class to the Left and the middle class to the Right, the social basis of *new* support for the parties and policies of the Left tends to come from middle-class sources. But at the same time, the Left parties become vulnerable to a potential split between their postmaterialist Left, which seeks fundamental social change, and their materialist constituency, which tends to take a traditional stance on the new issues raised by postmaterialists.

In 1972, this phenomenon temporarily split the Democratic party in the United States, when an insurgent movement won the presidential nomination for George McGovern. Though he won the postmaterialist vote overwhelmingly, much of the normally Democratic working-class electorate voted Republican, and social-class voting fell almost to zero, as Figure 4 demonstrates. This was an extraordinary election in which nearly half of all Democratic-party identifiers voted for the Republican candidate. In subsequent elections, many Democrats returned to their normal political loyalties, partially restor-

Figure 4. The Trend in Social-Class Voting in Five Western Democracies, 1947-86

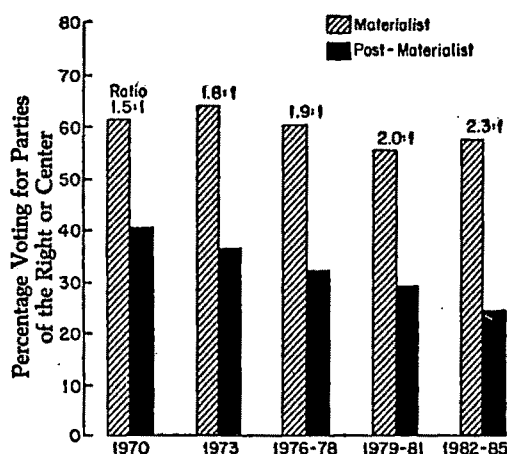


Source: Adapted from Lipset 1981, 505; updated by present author with results from recent elections and French elections. British data from Books and Reynolds (1975), Finer (1980); 1983 results calculated from Euro-Barometer no. 19 data; Swedish data, Stephens (1981), Zetterberg (1986); German data, Baker, Dalton and Hildebrandt (1981), Dalton (1984); American data based on whites only, Abramson et al. (1985); French data, MacRae (1967), later data from surveys conducted by Converse and Dupeux (1962), Converse and Pierce (1986) and Euro-Barometer surveys.

ing class voting. Nevertheless, it now hovers at some of the lowest levels to be found anywhere in the Western world.

In 1981, value cleavages contributed to a more enduring division of the British Left, split between a Labour party that had been captured by a neo-Marxist and neutralist left wing, and a new Social Democratic party that won over much of the party's mass constituency. Throughout the past decade, the materialist-postmaterialist cleavage threatened to split the German Social Democratic party, torn between a postmaterialist "Young Socialist" wing, and the labor-oriented main body. Though the postmaterialist Left was unable to take over the Social Democratic party as long as it still held power in Bonn, they did succeed in launching a Green (Ecologist) party that in 1983 won enough votes to enter the

Figure 5. Percentage Voting for Political Parties of the Right or Center, by Value Type in Britain, France, Italy, West Germany, Belgium and the Netherlands, 1970-85



West German national parliament for the first time.

A longstanding truism of political sociology is that working-class voters tend to support the parties of the Left, and middle-class voters those of the Right, throughout Western society. This was an accurate description of reality a generation ago, but the tendency has been getting steadily weaker. As Figure 4 illustrates, social-class voting has declined markedly during the past few decades. If 75% of the working class voted for the Left, and only 25% of the middle class did so, one would obtain an Alford class-voting index of 50 (the difference between the two figures). This is about where the Swedish electorate was located in 1948, but by 1985 the index had fallen to 31. Norway, Sweden, and Denmark have traditionally manifested the world's highest levels of social-class voting, but all have shown declining levels of social-class voting during the past three decades (Borre 1984, 352). In the United States, Great Britain, France, and West Germany

in the late 1940s and early 1950s, working-class voters were more apt to support the Left than were middle-class voters by margins ranging from 30 to 45 percentage points. In the most recent national elections in these countries, this range had shrunk to 8-18 points. In the most recent national elections (from 1983 to 1986) class voting fell to or below the lowest levels ever recorded to date, in Britain, France, Sweden, and West Germany. Though long-established political-party loyalties tend to maintain the traditional pattern, it is being eroded by (1) new support for the Left coming increasingly from middle-class postmaterialists; and (2) by working-class shifts to the Right, in defense of traditional values (Inglehart 1977; Lipset 1981).

It is important to note that the class-conflict model of politics is not a mere straw man: a few decades ago it provided a fairly accurate description of reality. But that reality has changed gradually but pervasively to the point where today, class voting in most democracies is less than half as strong as it was a generation ago. This change seems linked with inter-generational population replacement: throughout Western Europe, social-class voting indexes are about half as large among the postwar birth cohorts as among older groups.

We have argued that Western politics are coming to polarize according to social class less and less, and according to values more and more. We have just seen evidence of the former. Now let us examine evidence of rising polarization according to materialist-postmaterialist values. Figure 5 sums up voting intentions by value type from almost 95 thousand interviews carried out in Britain, France, Germany, Italy, the Netherlands, and Belgium from 1970 to 1985. A vast number of nation-specific events took place in these six nations during 15 turbulent years, which we will not attempt to discuss here. The overall pattern is clear,

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however: from 1970 to 1985, there was a trend toward increasing polarization on the basis of materialist-postmaterialist values.

In 1970, 61% of the materialists intended to vote for parties of the Right and Center, as compared with 40% of the postmaterialists. Materialists were likelier than postmaterialists to vote for the Right by a ratio of almost exactly 1.5 to 1. This already was a sizable difference, but it has grown steadily larger since 1970. In 1973, the ratio had increased to 1.8 to 1. In 1976-78, it grew to 1.9 to 1. By 1979-81 it was slightly more than 2 to 1. And in 1982-85, the ratio had risen to 2.3 to 1. This changing ratio was mainly due to a loss of postmaterialist votes by the parties of the Right. In 1970, 40% of the postmaterialists supported parties of the Right and Center; in 1982-85, only 25% did so; 75% were voting for the Left.

But which Left? In order to fully understand the significance of what has been happening, we must differentiate between various forces *within* a changing and divided Left. There has been only a modest increase in the proportion of postmaterialists voting for the two major long-established parties of the Left, the Socialists and Communists. In 1970, they drew 48% of the postmaterialist vote; in 1982-85, they got 53%. The major gains have been made by New Politics—above all, Ecologist—parties. In 1970 these parties won 13% of the postmaterialist vote. In 1982-85, they obtained 22%. The New Politics gains reflect two countervailing trends: (1) stagnation or decline of the Marxist New Left parties of the 1960s and early seventies; and (2) spectacular growth of Ecology parties, having a distinct and still evolving ideology concerning the quality of the physical and social environment. They have grown from almost nothing in the mid-1970s, to being the largest component of the New Politics parties. In the last few years, Ecology parties have won representation in the

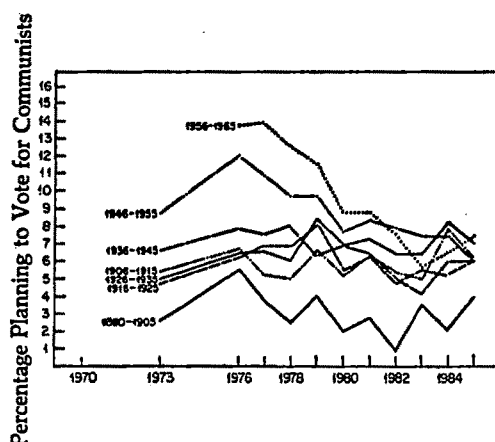
national parliaments of Belgium, Luxembourg, and West Germany, and in the delegations to the European Parliament elected in 1984 from Belgium, Germany, Italy, and the Netherlands. Their future potential may be even more than meets the eye, as we will see below.

The rapid growth of the Ecologists and the decline of the New Left parties of the 1960s reflect an important characteristic of both sets of parties: they have not yet developed strong voter loyalties or party organizations. Whether they ever will is an open question. If they don't, in the long run their electorates will probably be absorbed by larger parties that modify their ideological stance sufficiently to present an attractive alternative, just as the Socialist party has absorbed much of the New Left electorate in the Netherlands, partly capturing it, and partly being captured by it. We will not attempt to forecast the fate of given parties in given countries; it is influenced by the party's leadership, the strategies they adopt, and by nation-specific events, as well as by the values of the electorate. Figure 5 makes one thing clear, however: since 1970 there has been a growing tendency for electoral behavior to polarize on the basis of the materialist-postmaterialist value cleavage.

Postmaterialists have become increasingly likely to vote for the Left but this trend has become increasingly *selective*, with the Postmaterialist vote going to parties that have distinctive programs tailored to postmaterialist concerns. One striking consequence is that the Communists have lost their relative appeal to postmaterialists. In the early and mid-1970s, postmaterialists were about 2.5 times as likely to vote for the Communists as were materialists. By the mid-1980s, there was little difference between the two groups.

When postmaterialism emerged as a significant political force in the 1960s, its proponents tended to express themselves

Figure 6. Percentage Voting for Communist Party in Nine European Community Nations, by Age Cohort, 1973-85



Sources: Based on representative national samples of publics of Britain, France, West Germany, Italy, Belgium, Netherlands, Luxembourg, Denmark, and Ireland, weighted according to population of given nation. Includes data from Euro-Barometer survey nos. 5 through 23, plus 1973 European Community survey.

in Marxist slogans, which were then the standard rhetoric of protest in Western Europe. To a large extent the term *Left* meant the Marxist parties, and it was natural for the postmaterialists to assume that they were Marxists. But in fact there were profound and fundamental disparities between the goals of the postmaterialists and those of the Marxist Left, as the postmaterialists gradually discovered. These disparities became apparent in France earlier than in other countries—partly because the crisis of May-June 1968 brought to light the basic contradiction between the bureaucratic and authoritarian materialism of the French Communist party, and the postmaterialist desire for a less hierarchical, more human society, in which the quality of life was more important than economic growth. Even there ideological reorientation took

many years. But today, a Left has evolved in France that is increasingly non-Marxist or even anti-Marxist, and increasingly independent of the Soviet Union. The French Communist party, on the other hand, has remained one of the most authoritarian and Moscow-oriented parties in the West, with disastrous electoral consequences. After winning 20%–25% of the vote in most French elections from 1945 to 1978, the Communists suffered a sharp decline in the 1980s, falling to 16% of the vote in 1981, then to 11% in 1984, and dropping below 10% in the 1986 election. Though the degree varies from country to country, by the 1980s communism had lost its disproportionate appeal to the growing postmaterialist constituency in Western Europe.

With this has come the decline of the Communists' relative appeal to youth. For decades it had been axiomatic that the young were disproportionately likely to vote Communist: "If a man isn't a Communist when he's 25, he has no heart; if he's still a Communist when he's 45, he has no head." This linkage between youth and the Communist party, however, was not a biological constant; it was based on the belief that the goals of a specific younger generation converged with those of the Communist party.

That belief persisted until recently. As Figure 6 demonstrates, the two postwar cohorts among the West European publics were markedly more likely to support the Communist party than were their elders in each of the Euro-barometer surveys carried out during the 1970s. But the differences were growing weaker in 1978 and 1979. In the early 1980s, Communist support among the two postwar cohorts continued to decline, falling to, or even below the level of older cohorts. The young and the postmaterialists had shifted to other parties.

Where did they go? For the most part, not to the parties of the Right, despite a good deal of recent talk about the grow-

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Table 2. Support for Traditional Left and New Politics Parties by Age Cohort in Britain, France, West Germany, Italy, Denmark, Ireland and Benelux, in 1984-85 (%)

Birth Years	Communists	Socialists and Social Democrats	Ecologists and Other New Politics Parties	Right and Center	Number of Cases
Postwar					
1966-70	5	31	19	45	1,026
1956-65	8	36	17	39	3,782
1946-55	8	40	9	43	3,692
Prewar					
1936-45	7	38	6	50	3,143
1926-35	8	36	4	52	2,810
1916-25	6	40	3	51	2,886
1906-15	7	35	2	56	1,732
Before 1906	3	30	1	66	421

Source: Combined data from Euro-Barometer nos. 21, 22, and 23 (April and November 1984 and April 1985) weighted according to population of each nation (total number reporting a voting intention, 19,492).

ing conservatism of youth. The old Left clearly *is* in decline, which presents an opportunity for enterprising and adaptive parties of the Right and Center. But given the option, younger voters gravitate toward New Politics parties, above all the Ecologists, rather than toward the Right.

As Table 2 demonstrates, support for the Communists no longer bears much relationship to age. The Socialists and Social Democrats have also lost their special appeal for the young in most countries. Nevertheless, support for the parties of the Right and Center is substantially *weaker* among the three postwar cohorts than among all of the older groups. There is no overall trend to the Right. Instead, the slack is taken up by a pronounced rise in support for the Ecologists and other New Politics parties. Consequently, the relative strength of the Communists and the New Politics parties has shifted dramatically among the young. Among the cohorts born before World War II, Communist support outweighs New Politics support by as much as 2:1 or 3:1. In the cohort born in 1946-55, the New Politics parties are slightly stronger than the Communists. And among the two young-

est cohorts, support for the New Politics parties outweighs support for the Communists by margins of 2:1 and nearly 4:1.

Neither the young nor the postmaterialists automatically vote for any party that claims to represent the Left. They are influenced by past loyalties, like other voters. But when they abandon these loyalties, they do so in order to support the party that seems most likely to attain their goals, which are not necessarily those of the old Left, and emphatically not those of rigidly authoritarian parties such as the French Communist party. The old Left parties are losing ground among the young: they win the support of only 44% of the 1945-65 cohort, and only 36% of the 1966-70 cohort. But the Right need not win them by default. When an option is available that addresses the postmaterialists' concerns, they tend to take it. The evidence indicates that the Left can win the young *provided* it develops programs that appeal to the postmaterialists, as well as the old Left constituency—clearly not an easy task, but not an impossible one.

Though Western communist parties are in decline, it is extremely unlikely that

they will disappear. They still have millions of hard-core loyalists. Moreover, they have made important contributions to politics in their societies: by advocating greater economic equality they stimulated mainstream parties throughout the West to adopt welfare state policies that were motivated, in part, by the need to meet the challenge from the Left, but that helped their societies adapt to the contemporary world.

Today, Marxism itself needs to readapt—and for the sake of survival, is likely to do so. The Italian Communist Party already has reshaped itself extensively. Conservative Marxist parties like the French Communist Party will eventually be forced to do so, regardless of what the current leadership prefers. The need for change is so clear that it is taking place even in societies ruled by communist parties, with first Yugoslavia and Hungary and more recently, the People's Republic of China moving to relax the grip of central controls and allow greater freedom of expression. As the 1980s draw to a close, the Soviet Union itself is experimenting with ways to liberalize its economy and society. Even Marxist movements must respond to the forces of history.

Conclusion

The rise of a new axis of politics, based on polarization between postmaterialist values and traditional cultural values, and the decline of class-based polarization, has left Western political systems in a schizophrenic situation. Most of the major political parties have been aligned along the class-based axis of polarization for decades, and established party loyalties and group ties still hold much of the electorate to this alignment. But the most heated issues today tend to be New Politics issues, on which support for change comes mainly from a postmaterialist middle-class base. This creates a stress that can be resolved in two ways: by a

repositioning of the established parties; or by the emergence of new ones. Both have been taking place.

The Marxist interpretation of society seems in decline throughout the industrialized world. Its emphasis on economic factors as the driving force of history provides a good first approximation of reality in the early stages of industrialization, but is of diminishing value as scarcity diminishes and new problems emerge. Similarly, the *policies* that are needed to counter the ruthless exploitation of capitalism in its laissez-faire stage, reach a point of diminishing returns in advanced welfare states. Where government spending is already 40%–60% of GNP, there is less potential to move further in this direction; and the concentration of power in big government itself becomes an increasingly serious problem. The old assumption of the Left that more government was automatically better has lost its credibility. But to elevate government nonintervention into a theological principle is equally untenable. Some of the emerging problems of advanced industrial society will require more government intervention, not less. Within the next few decades, we will need to shift from petroleum to other energy sources or face dislocations far more severe than the OPEC-triggered recessions of the mid-1970s and early 1980s. Developing solar energy and nuclear fusion to meet this need requires a massive research and development effort, decades ahead of time, that market forces are not making and are not likely to make. Governmental inertness in such areas may be disastrous.

The meaning of *Left* and *Right* has been transformed. The key Marxist issue—nationalization of industry—remains a central preoccupation only to Marxist fundamentalists like the embattled hard-liners of the British Labour party. Properly handled, nationalization does little harm; but it is not the panacea it once appeared to be. And in so far as it diverts attention

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from increasingly pressing problems concerning the nature of modern war and the quality of the physical and social environment, it can be downright counter-productive—for it provides no solution to these problems. Nationalized factories pollute just as much as private ones. Indeed, in so far as nationalization merges the political regulators and the military-industrial complex into one cozy elite, it may even make things worse. Thus, according to a recent UNESCO study, East Germany is the most severely polluted nation in Europe, with air and water pollutant levels two to three times as high as those in West Germany. And it is no coincidence that the only nuclear-power-plant accident that has cost human lives, occurred in the Soviet Union, where public pressures for safety measures are minimized and, until recently, plants were built without containment structures, using a type of graphite reactor that Western countries stopped building decades ago because it was too hazardous. Though their environmental problems are even more severe and their arms expenditures at least as high as those in the West, the political systems of the East European countries rule out the emergence of independent and vigorous environmentalist movements or peace movements comparable to those in the West. It is relatively easy for the ruling elite to simply ignore such issues: officially, the problems underlying them exist only in capitalist countries.

War is as old as human society, but modern technology has given it new implications: today, it could terminate the human race. This fact has shaped a post-war generation for whom war has a different meaning from what it had in previous history. If one still accepts the Marxist conventional wisdom that war is caused by capitalist greed for profits, then one has a ready solution: abolish capitalism. But the fact that most of the bloodiest battles of the past decade have been

fought *between* nominally Communist countries—with Cambodia against Vietnam, Vietnam against China, China against the Soviet Union, and the Soviet Union against Afghanistan—has led to growing skepticism that the answer is this simple. The problem is rooted in the mentality of a military-industrial complex deeply rooted on *both* sides of an obsolescent ideological boundary.

The old ideological paradigm no longer corresponds to reality. Neither the Marxist fundamentalists nor the laissez-faire fundamentalists have adequate answers for the problems of advanced industrial society. The goals of individuals and the challenges facing society are different from those of a generation ago.

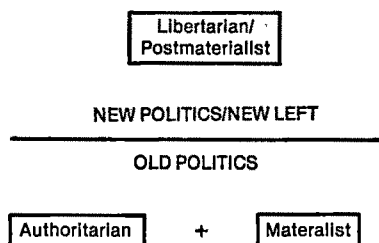
RONALD INGLEHART

University of Michigan

This controversy harks back to a debate I started with Ronald Inglehart in 1979 (Flanagan 1979), which has found its clearest expression to date in an exchange of articles in 1982 (Flanagan 1982a, 1982b; Inglehart 1982). There are two reasons for revisiting this debate now. First, Inglehart's most recent contribution to the values debate, presented above and in a companion piece,¹ represents a significant shift in his position, which I believe brings his argument much closer to my theoretical view. Second, new data is now available to present a definitive test, at least in the Japanese context, of the issues that divide us. Thus, I believe we are finally in a position to take a giant step towards a resolution of the debate.

The crux of the debate revolves around my argument that there are two distinct kinds of value change taking place in the advanced industrial democracies and that Inglehart has obscured this distinction by collapsing indicators of both into a single scale. My argument has been that these

Figure 7. Inglehart's View of Cleavage Structures in Advanced Industrial Democracies

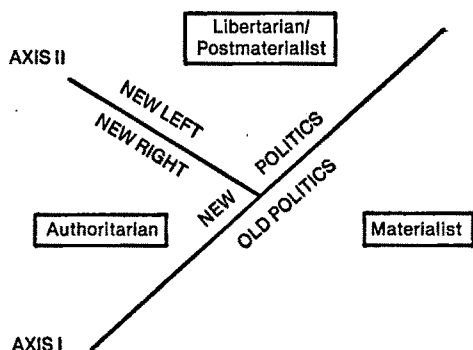


two kinds of change are not only conceptually distinct but are explained by different causal phenomenon and exhibit different patterns of relationships with key demographic and political variables.

Libertarian-Postmaterialist Values

The distinctions between our respective positions can be confusing because we use overlapping but not identical kinds of indicators to measure our respective scales, and similar labels to identify different conceptual phenomenon. To help clarify the differences, Figures 7 and 8 depict our contrasting conceptualization of value change, with the types of indi-

Figure 8. Flanagan's View of Cleavage Structures in Advanced Industrial Democracies



cator items used to measure the scales presented in boxes. Each line represents the axis of a value cleavage, with labels on either side of the cleavage indicating the political impact of the particular type of value orientation defined by the cleavage.

First it should be noted that our three scales, my two and Inglehart's one, are measured by essentially only three kinds of items. What I call *libertarian* and Inglehart and others label *postmaterialist* items are essentially identical. Looking across the full collection of items that have been designated by these labels, we find they include an emphasis on personal and political freedom, participation (more *say* in government, in one's community, and on the job), equality, tolerance of minorities and those holding different opinions, openness to new ideas and new life styles, environmental protection and concern over quality-of-life issues, self-indulgence, and self-actualization (Calista 1984; Hildebrandt and Dalton 1978; Inglehart 1977; Lafferty and Knutsen 1985). There may be some minor differences over which of the above elements are more or less central to these two concepts, but to the extent that they are all found to cluster, I expect none of the above authors would have trouble accepting any of these elements as part of the concept of *postmaterialism*. I include them all in my notion of *libertarianism*, so we have two different labels representing the same cluster of items. While I believe that Inglehart's term *postmaterialism* is a misnomer as a characterization of either the causes or nature of this value cluster, I will label it here *libertarian-postmaterialist* to clarify that regardless of how we choose to identify these items, we are measuring essentially the same set of values.

The differences emerge at the other end of our scales. I have used the term *materialism* in a more limited sense than Inglehart to identify the emphasis attached to economic concerns, both for oneself and

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one's society. In my sense of the term, then, *materialists* are those who place a high priority on a stable economy, economic growth, fighting rising prices and, at the more personal level (the private domain), on securing a high-paying job, adequate housing, and a comfortable life. Inglehart includes in his *materialism* concept, however, a second set of non-economic issues, namely support for a strong defense, law and order, and fighting crime. I label this second set of non-economic issues as one component of an *authoritarian* value orientation. This *authoritarian* orientation designates a broader cluster of values, which, along with concerns for security and order, includes respect for authority, discipline and dutifulness, patriotism and intolerance for minorities, conformity to customs, and support for traditional religious and moral values. To avoid confusion, I will use the term *materialism* in this discussion to refer to my definition of the term rather than his.

As Figure 7 shows, Inglehart in his original argument presented us with one dimension of value change. Those holding his combination of what I label *materialist* and *authoritarian* value orientations defined the Old Politics for him, while those with the libertarian-postmaterialist orientation the *New Politics* or *New Left*, terms which he used virtually synonymously. He made no mention of a New Right.

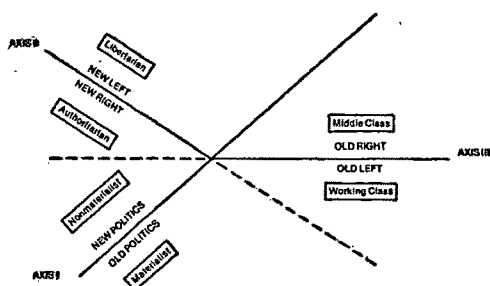
This conceptualization encounters difficulty when it attempts to explain realignment. On the one hand we can readily grasp why the younger generation of highly educated, middle-class respondents, whose families have traditionally supported the Right for economic reasons, may now be induced to vote Left as a result of their socialization into libertarian-postmaterialist values. Inglehart's argument breaks down in trying to explain why working-class voters should abandon their historic support of the par-

ties of the Left. Here his argument has rested on a variation of the *embourgeoisement* thesis that he still endorses (Inglehart and Rabier 1986, 458). In his original formulation he wrote that working-class respondents who "have attained a certain level of prosperity relatively recently" will "continue to place a comparatively high value on defending and extending their recent gains" (Inglehart 1971, 992). Because the value priorities of these working-class respondents remained primarily acquisitive or materialist, their growing share of the good life would lead them to become more economically conservative and, hence, potential recruits for the conservative parties.

However, while the working-class Tory phenomenon is well documented, it is capable of explaining the behavior of only a deviant portion of the working class (Goldthorpe et al. 1968; Hamilton 1967). Conservative parties do not defend the economic interests of the working class, and certainly we cannot expect a major realignment of working-class support from Left to Right based on the economic appeals of conservative parties. Moreover, realignment via *embourgeoisement* applies only to the most affluent portion of the working class. However, since affluence in Inglehart's formulation is the primary cause of postmaterialism, these are the very workers most likely to be developing postmaterialist values, which should only reinforce their traditional support for the Left.

In contrast to Inglehart's view, my conceptualization defines two distinct dimensions along which values are changing. The first value cleavage, running along the lower-left-to-upper-right diagonal, taps the priority a respondent attaches to economic issues as opposed to non-economic, value issues. This cleavage divides materialists from nonmaterialists and the Old Politics from the New Politics. Since materialists attach primary importance to economic concerns, they

Figure 9. Full Cleavage Structure for Advanced Industrial Democracies



tend to relate to politics in economic terms, which corresponds to the traditional class politics of the Old Right and the Old Left party alignments. Conversely, the nonmaterialists emphasize the importance of value concerns over economics. This emphasis on value issues defines the New Politics and includes, for example, *both* prochoice and right-to-life alternatives and *both* strong defense and antinuclear positions. Nonmaterialists, then, are those holding either authoritarian or libertarian value preferences, and those who place a higher priority on the kinds of issues defined by these value preferences than on economic issues.

The New Politics is divided by the second value cleavage, which distinguishes the New Left from the New Right. Those falling on the libertarian side of this second value cleavage support for the New Left issue agenda, including liberalizing abortion, women's lib, gay rights, and other new morality issues; protecting the environment, antinuclear weapons, and other quality-of-life issues; and support for protest activities, more direct forms of participation, and minority rights. On the other side of this value cleavage, the authoritarians endorse the New Right issue agenda, which includes right-to-life, anti-women's lib, creationism, antipornography, and support for traditional moral and religious values; a strong defense, patriotism, law and order,

opposition to immigration and minority rights, and respect for the traditional symbols and offices of authority.

Figures 7 and 8 are presented to demonstrate the contrasting ways that Inglehart and I have chosen to combine three types of items into our respective scales. Figure 9 depicts the full cleavage structure and its implications for realignment. As in Figure 8, Axis 1 again divides the Old Politics from the New Politics and materialists from nonmaterialists on the basis of the relative salience accorded to economic, as opposed to value, issues. The relevant cleavage on the New Politics side of Axis 1 is Axis 2, which divides the New Left from the New Right and libertarians from authoritarians. The relevant cleavage on the Old Politics side of Axis 1 is Axis 3, which divides the Old Right from the Old Left and the middle class from the working class. As the dotted extension of the New Left-New Right value cleavage into the Old Politics domain in Figure 9 suggests, the Axis 1 value priority distinction and the Axis 2 value preference cleavage are essentially independent of each other. Thus we should expect to find libertarian-materialists as well as authoritarian-materialists. The line is dotted in the Old Politics domain because for the materialist, who places greatest priority on economic concerns, the libertarian-authoritarian distinction will have little effect on his or her voting behavior. Similarly, the Axis 3 class cleavage extends as a dotted line into the New Politics domain, but for nonmaterialists these class distinctions will not be paramount in their voting decisions.

It is important, therefore, to reach an independent determination of a respondent's position on both value dimensions. An authoritarian-libertarian value preferences scale will tell us whether the respondent is likely to support the New Right or New Left issue agenda. A materialist-nonmaterialist value priorities scale will tell us whether these New Poli-

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tics kinds of value concerns or the Old Politics economic issues will be foremost in the voter's mind when he or she makes a choice. This latter distinction is particularly important for predicting the behavior of cross-pressured voters, who, for example, may fall on the Left side on the Old Politics cleavage because of their working-class occupations and union memberships but on the Right side of the New Politics cleavage because of their authoritarian values.

The conceptualizations of value change in Figures 8 and 9 can explain the realignment of working-class voters to the Right more effectively than can Inglehart's conceptualization (Figure 7). Across Axis 2 in Figure 9 there has been a long-term shift from authoritarian to libertarian values associated with the changing circumstances under which younger generations are being socialized and the growth of higher education. As the number of libertarians in the advanced industrial democracies reached a critical mass, they began pushing for New Left issues and achieving some successes. The increasing articulation of libertarian values and the protest movements organized to press for the adoption of the New Left agenda mobilized a backlash among authoritarians, who felt that their basic values and way of life were being threatened. This increasing polarization on the New Politics issues in turn heightened their salience in relation to the Old Politics class issues, which were already on the decline due to growing affluence and the success of the welfare state. Thus the shift across Axis 2 from a heavily asymmetric balance in favor of the traditional authoritarian values to a more symmetric balance between authoritarian and libertarian values heightened the salience of New Politics relative to Old Politics issues, thereby inducing movement across Axis 1 as well. Since education is related to the class and values cleavages in a cross-cutting pattern, associating high

education with both the Old Right and the New Left, the combined trends on both dimensions promote the middle-class-to-the-Left and working-class-to-the-Right realignment pattern Inglehart and others have been describing.

One thing that is new in Inglehart's most recent contributions is that he is now beginning to adopt much of the terminology of the Figure 9 conceptualization explicitly. However, he is doing this without in any way revising or altering his scale, which collapses the two dimensions into one and hence treats authoritarian values and the New Right issue agenda as essentially synonymous with materialism. This approach of grafting a new terminology onto his old conceptualization simply adds to the confusion.

Examples of this subtle shift in terminology are abundant in his recent discussions of the value change-realignment phenomenon. In this discussion, Inglehart first reiterates the argument that he and others have made explaining why the New Politics values cleavage is an important phenomenon to study even though there is as yet little evidence that it has induced a realignment of party systems in the advanced industrial democracies. That argument points out that the expression of the New Politics value cleavage in patterns of party support is inhibited by frozen party loyalties and institutionalized interest-group-to-party linkages (Dalton, Flanagan, and Beck 1984; Flanagan and Dalton 1984). Nevertheless the rising salience of the New Politics is increasingly placing these party systems under stress. Perhaps Inglehart's most important contribution in this regard is to demonstrate that the meaning of *Left* and *Right* is changing among the elites and mass electorates of these societies from economic-issue orientations to non-economic-value-issue orientations (Inglehart 1984). And most recently, he demonstrates that the amount of class-party realignment that can be shown to have

already taken place is mostly benefiting New Left and New Right parties rather than the traditional Old Left and Old Right parties and that beyond that, there exists considerable potential support in these electorates for New Left and New Right kinds of parties (Inglehart and Rabier 1987; and the opening to this *Controversy*).

In this discussion of the shift from the Old Politics to the New Politics and its implications for realignment, he seems to buy into many of the distinctions presented in the conceptualizations of Figures 8 and 9. He says that the New Politics issue polarization "primarily reflects the new noneconomic issues" and that "the issues that define Left and Right for Western publics today are not class conflict [issues], so much as a polarization between the goals emphasized by post-materialists, and the *traditional social and religious values* emphasized by materialists" (Inglehart and Rabier 1986, 470, 471; emphasis added). While still using the term *materialists*, the cluster of values and issues preferences that emerge from his own factor analyses reported in his 1984 and coauthored 1986 pieces as being associated with self-placement on the Right end of a Left-Right scale are that the military-defense effort should be stronger, the existing social order should be defended, terrorism should be punished more severely, nuclear power plants are essential, the unemployed don't want to work, there are too many immigrant workers, one should sacrifice oneself for one's country, abortion is wrong, religion is important, and God exists. To apply the label *Materialist* to these New Right issues is clearly a misnomer. Moreover, the New Left issues he identifies (pro-environment, abortion, peace, homosexual rights, etc.) clearly did not "stimulate a materialist reaction" in the sense of an increased concern for economic issues. Quite to the contrary, the New Left issues have helped to crowd the economic issues

off the agenda and have rather provoked the emergence of the above New Right set of moral and religious issues.

Here we begin to see the problems inherent in the theoretical arguments and conceptual labels that Inglehart has attached to his value scale. The emergence in his own studies of a New Right cluster of noneconomic value issues around the New Politics cleavage that fit my definition of authoritarian values implies a delinking of the New Right with the concept of *materialism*. The point is that the New Right is as much *nonmaterialist* as the New Left. The program of the National Front, the French example of a New Right party, stresses law and order, restrictions on immigration, opposition to abortion and anticommunism. It is because these issues have little or nothing to do with traditional class issues and material, economic concerns that the New Right parties are able to draw their support disproportionately from the working class, as Inglehart so clearly demonstrates.

A New Theoretical Basis for Materialism

In my view, the most important fresh contribution found in Inglehart's analysis presented above is the new theoretical basis he provides for understanding the shift from materialist to nonmaterialist issue priorities. He labels this argument "the diminishing marginal utility of economic determinism." The diminishing-marginal-utility approach is a much sounder theoretical foundation for explaining the shift from economic to noneconomic value priorities than Maslow's need hierarchy. Maslow presents us with a theory of psychological development and motivation arguing that an individual reared in an environment in which lower-level needs are satisfied will develop into a mature personality, one

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who as an adult is better able to cope with the deprivation of lower-level needs and hence is insulated from need regression (Maslow 1970). In contrast, Inglehart's items typically tap issue priorities in the domain of public policy. He is, then, dealing with a vastly different phenomenon, and thus the inappropriateness of the Maslowian argument leaves him with no rationale for explaining why respondents would not increasingly place a higher priority on the issue of rising prices when inflation becomes an increasingly serious problem in their country. Indeed the evidence suggests that they will (Flanagan 1982b).

What the *diminishing marginal utility* concept suggests, however, is something quite different than the Maslowian early-childhood-socialization notion. As Inglehart's Table 1 and Figure 3 demonstrate, the higher a nation's GNP per capita, the weaker both the felt need for further reducing income inequality and the other classic economic policies of the Left designed to redistribute the wealth more equally. As Inglehart argues, after a certain level of equality has been achieved, further movement in the direction of perfect equality has a diminishing marginal utility, as there is less and less left to redistribute, and fewer gain and more lose in the process.

Figure 1 in his companion piece (Inglehart and Rabier 1987), most clearly presents the lesson that is to be learned from the diminishing-marginal-utility thesis. As median income goes up in a nation, the ratio of what the respondent views would be the absolutely necessary income to what the actual household income is drops. In other words, more people find themselves with a comfortable surplus. Their basic economic needs are being met, so economic issues become less intensely held and are assigned a relatively lower priority than they were when household income was at or below the level of absolutely necessary income. All other

things being equal, the proportion of non-materialists will rise as average household income within a nation rises above a level perceived as necessary to provide the basic necessities of life. As the margin of surplus income increases, the citizen's expenditure of energy in support of economic issues will yield a diminishing marginal utility, and the nonmaterialist is born.

Being a nonmaterialist at time one, however, does not ensure that a respondent will still assign a relatively low priority to economic concerns at time two. As has been shown elsewhere, the relative priority that a respondent attaches to economic issues in relation to other concerns varies across the life cycle as the respondent's economic responsibilities and burdens change (Flanagan 1982a; Milkis and Baldino 1978). A respondent's priorities will also shift in response to short-term changes in perceptions of how well-off he or she is. Aggregate stability in the proportion of materialists, therefore, is a function of national affluence and will remain rather stable so long as economic conditions in the country do not change. Nevertheless, aggregate stability can mask considerable instability at the individual level.

Inglehart claims that his diminishing-marginal-utility (DMU) argument is not new but was implicit in his original value-change thesis. Rather, the logic of the DMU argument as a causal explanation of value change is completely different from his original Maslowian approach. It should be recalled that it was his reference to Maslow's hierarchy of needs that enabled Inglehart to claim that the materialism-postmaterialism value change was the product of early childhood socialization. The Maslowian analogy gave birth to the idea that socialization under conditions of affluence and security create a distinct prioritization of needs that immunizes the individual from regression to lower-level concerns as an

adult, even if the conditions of life change. Apparently Inglehart has found that there is a diminishing marginal utility in citing Maslow for his purposes, and indeed we find no references whatsoever to Maslow or the need hierarchy in either of his two current companion pieces. I believe history will show that his Maslowian argument was a clever analogy that did not fit.

In contrast, childhood socialization logically plays no role in the DMU argument. An estimation of the diminishing marginal utility of added increments of income is a rational-choice assessment based on the individual's current level of need and sense of relative deprivation. This assessment, then, is very much a *context-dependent* phenomenon, very similar to the life-context arguments that Milkis and Baldino (1978) and I have made. What is new in Inglehart's notion is that he has changed the relevant context from the individual level, a level at which we would expect to find considerable change across the life cycle, to the societal level. In doing so, he has convincingly made the case that we should find important thresholds in national income and household income, which, once crossed, yield aggregate shifts in the relative priority attached to economic concerns. These thresholds are probably not absolute but rather are likely to shift somewhat in line with changing societal standard-of-living expectations. Still, the notion that at some point a sufficient surplus should lower the priority attached to economic issues and to further increasing one's income, not in every case but in the aggregate, is compelling.

The *diminishing marginal utility* notion provides some legitimacy for using the term *postmaterialist*. I have preferred the term *nonmaterialist* because of my focus on the fluctuation that occurs on the individual level across the life cycle. However, the notion that advanced industrial societies as a whole are moving towards a

lower priority ranking for materialistic concerns suggests more of a unidirectional shift, perhaps justifying the *post-* prefix. Still this is clearly not the kind of irreversible phenomenon as suggested by the Maslowian arguments. Indeed we would expect that serious economic problems, such as runaway inflation or depression, would alter the current assessments of many respondents as to the relative priority that should be attached to economic concerns.

Thus, we are dealing with two distinct kinds of value change explained by very different kinds of causal phenomenon. On the one hand we are witnessing an eclipse of the salience of economic issues. This trend is explained by the diminishing-marginal-utility argument. As the affluence of, and equality within, nations increases, the percentage of the population that enjoys a cushion of surplus income above what is needed for the basic necessities of life grows. For these people, noneconomic issues begin to gain in salience relative to material, economic concerns. There may be some variation in the priority accorded to economic issues at the individual level over time, with changes in fortune and life context, and also short-term setbacks at the aggregate level associated with economic downturns. Over the long haul, however, we should be seeing a net growth in the proportions of *postmaterialists*—as here defined in terms of the relative priority attached to economic issues (Old Politics) as opposed to noneconomic, value issues (New Politics).

The second and distinct trend is the one that defines the change in value preferences. Within the so-called New Politics can be detected a long-term, gradual movement from the New Right to the New Left poles, as a function of age (inter-generational change) and education. Space constraints will not permit a full elaboration here of the causal mechanisms, elaborated elsewhere, behind this

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pattern of value change (see Flanagan 1979, 1982a, 1984). In brief, the argument is that an intergenerational pattern of value change along the authoritarian-libertarian (A/L) dimension has resulted from four major changes in the basic conditions of life under which successive generations have been socialized—a growing equality in incomes and life styles, the accelerating pace of change, the advance and diffusion of scientific knowledge, and the rise of the “no-risk” society (Aharoni 1981). These changes are increasingly liberating mankind respectively from the constraining conditions of subservience to authority figures, conformity, ignorance, and insecurity, and enabling the individual to pursue more fully the goal of self-actualization. Given the nature of many of these changes in basic life conditions, which are driving the change along the A/L value dimension, it is not difficult to understand why the increasing levels of higher education found in the advanced industrial societies are also playing an important role in diffusing libertarian values.

New Evidence of Dimensional Independence

At this point one might raise the question why—if I am in fact right—it has been so difficult to demonstrate the problems with Inglehart’s scale empirically and why he has been able to pile up evidence from an exhaustive number of surveys conducted in 10–15 nations over the past 15 years. For example, Inglehart has consistently claimed that there is no evidence of a life-cycle effect on the proportion of materialist found in given age cohorts over time and that there was no substantial reduction in the proportion of his postmaterialists across a large number of countries despite inflation, recession, and other economic woes in the seventies.

Moreover, he seems to have the evidence to make good on those claims.

Such a finding is central to his argument because it supports his notion that the materialist-postmaterialist change is a function of early learning that insulates the individual against later change. However the problem here is that while Inglehart labels his scale *materialist-post-materialist*, 75% of the items used to operationalize the scale rather tap the authoritarian-libertarian dimension. This latter dimension, which measures how respondents divide on the New Politics value cleavage, is not affected by changes in the economy. Changes in one’s income, all other things being equal, will not affect one’s position on abortion, pornography, or nuclear weapons or one’s respect for authority. The relative aggregate stability of Inglehart’s scale over time in the face of changes in the economic context, therefore, is derived in large part from the fact that his scale, in essence, is tapping more of the New Left-New Right (libertarian-authoritarian) cleavage than the New Politics-Old Politics (materialist-non-materialist) division.

What further confounds our ability to disaggregate the two dimensions combined within his scale is the format of his value-priority questions, which artificially forces an association between his materialist and authoritarian items. Whether using his original 4-item format or his expanded 12-item version, respondents are typically presented with groups of four alternatives and asked to select their first and second priorities. In each case two items are—to use my terminology—libertarian (e.g., free speech and participation); one item is authoritarian (e.g., defense); and one is materialist (e.g., rising prices). This presents the respondent with a *dilemma of constrained choice*. Libertarians who are also non-materialists will naturally select the two libertarian items. Libertarians who are materialists, placing greater priority on

their economic than on their value concerns, will pick the one materialist item and then, having no other materialist item to pick, will select one of the libertarian alternatives, dropping them into Inglehart's mixed category. Authoritarian-materialists, following the same logic, will pick the materialist item first and then select the authoritarian option.

The real problem, however, is confronted by the authoritarian-nonmaterialists. As a nonmaterialist the respondent will be mostly concerned with the non-economic, value issues. But he or she finds only one authoritarian option. As an authoritarian, the respondent is very unlikely to select either of the libertarian options and thus, by default, is most likely to pick up the economic option as a second choice. The result is that the authoritarian-nonmaterialists are classified as *materialists*, which is an incorrect coding for a materialist-nonmaterialist scale but a correct coding if we view Inglehart's scale as a stand-in for the authoritarian-libertarian scale.

This logic explains two important characteristics of Inglehart's scale. First it helps us understand why we always find a heavily skewed distribution with many materialists and typically only 5%–15% postmaterialists, while the authoritarian-libertarian type of scale tends to divide samples much more evenly. Although both types of nonmaterialists are properly sorted, the authoritarian-materialists are classified as *materialists* while the libertarian-materialists are classified as mixed. Secondly, and more importantly, whatever real association there may be between authoritarian and materialist orientations is greatly exaggerated by the constrained-choice feature, which forces authoritarian-nonmaterialists to select the materialist option.

The answer to this problem is to adopt an item format that allows an independent and unconstrained assessment of the priority of all three types of items identi-

fied in Figure 7—libertarian, authoritarian, and materialist. This is a little tricky, due to the inherent differences between value priorities and value preferences. The materialism items are designed to tap the relative importance of economic and value concerns, both of which are presumably positively valued by the respondent. In contrast, authoritarian and libertarian values stand in opposition to each other, with those who stand at one pole typically viewing the opposite set of preferences in a negative light.

The solution to these problems is to follow the approach developed for the 1976 German Youth Survey conducted by the United States Information Agency (see Dalton 1981). Respondents are handed a card with a number of social and personal goals listed and in each case are asked to indicate whether they feel that their society places too much or too little emphasis on that goal. For the value-priority items, this allows for a rough ranking of the items' importance; for the value-preference items it indicates on what side of the New Politics value cleavage the respondent is found. Since all three types of items are asked in the same way, this procedure will enable us to determine how they cluster in an unconstrained situation. If Inglehart is correct, the materialist items on economic concerns should cluster with the security subset of authoritarian items, and the rest of the authoritarian items should lie distinctly outside of that cluster. If I am correct, all the authoritarian items should cluster together while the materialist items form a distinct separate cluster. A third option somewhere in the middle would be to find that all the authoritarian and materialist items cluster together.

The data to test these conflicting hypotheses comes from a 16-item value question I designed using the above unconstrained format administered in a 1984 nationwide Japanese survey of university-educated respondents.² Due to the rich-

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ness of the value themes tapped by the survey, it was possible to add several more items, bringing the total to 21, with 9 libertarian, 8 authoritarian and 4 materialist items. As shown in Table 3, the *libertarian* items cover the themes of participation, support for minorities, open-mindedness, personal freedom and freedom of speech, improving the quality of life, self-fulfillment, and self-indulgence.

The *authoritarian* items include the themes of patriotism, a strong defense, respect for authority and strong leaders, preserving traditional morals and values, conformity, and intolerance of dissenters. The *materialist* priorities were measured by the four items identifying those who place high emphasis on maintaining high economic growth, securing a high-paying job, and working hard and saving for the

Table 3. Factor Loadings for 21 Libertarian, Authoritarian, and Materialist Items on the First Unrotated Factor and for the Three-Factor Rotated Solution, Japan 1984

Items by Type	Factor Loadings For			
	1st Unrotated Factor	Three-Factor Rotated Solution		
		1st	2d	3d
Libertarian items				
Personal freedom	.611	.523	-.293	.267
Freedom of speech	.530	.413	-.292	.279
Giving people more say in govern- ment decisions	.475	.548	-.117	-.098
Increasing benefits for disadvantaged	.475	.672	.032	-.047
Improving environment and quality of life	.436	.633	.073	.113
Active citizen participation in local politics	.363	.318	-.185	.040
Being open-minded to new ideas	.309	.421	.015	.040
Seeking personal fulfillment	.309	.7286	-.081	.395
Living for today and enjoying oneself	.281	.7188	-.143	.431
Materialist items				
Securing a high-paying job	.104	.131	.100	.664
Working hard and saving for the future	.002	.299	.390	.7329
Maintaining high economic growth	-.114	-.116	.132	.541
Feel our society is not too materialistic	-.115	-.192	.037	.482
Authoritarian items				
Preserving traditional morals and values	-.197	.150	.468	-.009
Following custom and neighbors' expectations	-.325	.066	.592	.145
A few strong leaders better than parties	-.343	-.131	.380	.044
Respect for authority	-.351	-.106	.419	.029
Providing for strong defense forces	-.491	-.349	.399	.295
No room in Japan for dissenters	-.509	-.103	.633	-.145
Patriotism and loyalty most important	-.631	-.268	.638	-.106
Should increase defense spending	-.637	-.468	.456	.106

Note: The three-factor solution was derived from a varimax rotation.

Table 4. The Association among the Libertarian, Authoritarian, and Materialist Item Scales and between Those Scales and Several Key Demographic and Attitudinal Variables

Scales and Variables	Libertarian	Authoritarian	Combined Authoritarian- Libertarian	Materialist
Authoritarian scale	-.32			
Materialism scale	.13	.23	(-.07)	
Age	-.28	.30	-.37	(-.02)
Right-Left party identification	.28	-.39	.41	(-.04)
Right-Left self-placement	.30	-.41	.44	(-.05)

Note: All correlations are significant at the .001 level except those in parentheses, which are not significant even at the .01 level.

future and those that do not feel that their society overemphasizes material things.

As Table 3 shows, the libertarian items all load positively on the first unrotated factor and the authoritarian items all load negatively, while the four materialist items all fall in the middle, closer to a zero loading. Even more dramatically, the table demonstrates that with the exception of two libertarian items and one materialist item that have somewhat ambiguous loadings, the rotated three-factor solution neatly divides the three types of items, with the libertarian items loading on the first factor, the authoritarian items on the second factor, and the materialist items on the third. In discussing the exceptions below, it is important to note that all of the items included in each of the three reported value domains drawn in Table 3 load highly on a first unrotated factor when the analysis is limited to only those items within a given value type. The inclusion of these three items (marked ?) within their identified value domain is only brought into question because of the presence of other factors on which they also load.

Each type of item was then combined into a scale and the resulting three scales were then correlated with each other and with several key demographic and attitudinal

items as shown in Table 4. As expected, a strong inverse correlation was found between the libertarian and authoritarian scales ($-.32$). Moreover, the libertarian scale is inversely associated with age ($-.28$) and positively correlated with the Left ends of the Right-Left self-placement and party identification scales (.30 and .28 respectively). Conversely, the authoritarian scale is associated with old age (.30) and the political Right ($-.41$ and $-.39$). The strength of these correlations is somewhat further increased when all the libertarian and authoritarian items are combined into a single scale. In marked contrast, the materialism scale yields no significant correlations with either age or political preference. Unfortunately, the relationships with the other key demographic variable, education, could not be tested because the sample was limited to the university educated. However, the relationships reported in Table 4 are precisely those reported in my earlier study (Flanagan 1982a), only now the materialism scale is derived from the kinds of materialism items Inglehart employs (rather than on a post hoc basis from a most-important-problem question), so there can be no question of comparability. The earlier study included education and class, and we can infer that the

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relationships would be the same here if the sample had not been restricted on those dimensions.

The most important finding in Table 3 is that the security items Inglehart uses to measure materialism cluster with the authoritarian items while the economic items stand alone. The most important finding in Table 4 is that the pattern of relationships supports my argument that the materialism-nonmaterialism and authoritarian-libertarian dimensions of change are explained by different causal phenomenon and yield contrasting patterns or relationships with key demographic and political variables.

The most interesting unexpected finding in Table 4 is that materialism is positively correlated not only with the authoritarian scale (.23) but also with the libertarian scale (.13). Since I have viewed materialism as essentially an utilitarian orientation, I expected some significant correlation between the authoritarian and materialist items based on the following logic: The relative importance a respondent will place on the economic issues of the day—inflation, depression, unemployment, further economic growth, etc.—is a function of both the respondent's margin-of-income surplus and the extent to which the respondent perceives that his or her economic interests are being threatened by those issues. Thus new middle-class professionals in the advanced industrial democracies are likely to be nonmaterialists, both because of the substantial cushion their incomes provide them with beyond the basic necessities of life and because the skills they possess are highly valued in postindustrial economies, providing them with great job security. Conversely, the blue-collar worker is more likely to be a materialist, both because of his or her smaller income surplus and because this respondent's skills are increasingly becoming obsolete, thus heightening concern over livelihood.

The finding of positive correlations

with both of these opposite value preferences suggests that those lying towards both of the extreme ends of the authoritarian-libertarian value dimension might have other kinds of inducements beyond utilitarian evaluations. It may be that something in the values themselves that are held by the extreme form, or pure type, of authoritarian or libertarian predispose each of them towards materialist priorities. Clues as to what these motivations might be are derived from the three items in Table 3 with ambiguous loadings. The materialism item *working hard and saving for the future* actually loads slightly more heavily on the authoritarian dimensions than on the materialism dimension. Elsewhere I have argued that the values of frugality, discipline, and hard work are part of the traditional authoritarian orientation (Flanagan 1979).

One might just mark this down as a bad item, since it combines the authoritarian orientation towards diligence and the traditional "Confucian" and "protestant" work ethics with the materialist concern with economic well-being. If we look more deeply, however, we may find a *psychological materialism*; that is, the authoritarian's preoccupation with discipline and hard work, in contrast with the self-indulgent libertarian orientations, which place a high priority on leisure activities, may be related to the same heightened need for power and sense of weakness that drive one to identify with established authority figures and symbols. Thus the same insecurities that prompt one to support a strong military and law and order may also motivate hard work, frugality, and a heightened effort to maximize one's economic security to placate anxieties about one's own weaknesses and lack of power. Psychological studies have provided evidence suggesting that such authoritarian needs for power and feelings of weakness and insecurity are the product of child-rearing practices that stress discipline over affect-

tive ties, a behavioral distinction that has been shown to correlate with class and education (Adorno et al. 1950; Winter 1973). Thus, a number of lower-class, low-education authoritarians may also be psychological materialists.

Just as revealing is the loading of the two libertarian items, *seeking personal fulfillment* and *living for today and enjoying oneself*, on the materialism factor. This suggests a Yuppie type of *terminal materialism*. Some libertarians who are preoccupied with self-fulfillment and self-indulgence may place an excessive valuation on maintaining a strong economy, acquiring high-paying jobs and surrounding themselves with all the material trappings of affluence. While for some libertarians, materialism may only be a secondary instrumental priority, something to be valued because it makes other life goals possible; for others, materialism may become an end in itself—a terminal value—because of the status, self-esteem, sense of achievement, comfort, self-indulgence and other gratifications that it provides. Some libertarians, then, may be susceptible to a “terminal materialism” in which, notwithstanding their relative affluence, increasing their wealth, possessions, and consumption of the good life comes to be valued more highly than other kinds of libertarian values. Thus, for them, economic issues may take precedence over their support for the New Left issue agenda.

The diminishing-marginal-utility thesis is a powerful one for explaining why there is a higher proportion of nonmaterialists in the United States than in Colombia. However, after reaching and surpassing some level of societal and personal affluence, further gains in affluence will have little effect on altering one's value priorities, and we are left with having to explain why some upper-middle-class respondents are still materialists. Indeed there is some evidence of a countertrend in the United States running against the logic of

the diminishing-marginal-utility thesis. The American Council on Education's (1973-86) annual nationwide surveys of roughly two hundred thousand entering college freshmen, which began 20 years ago, have documented a growing emphasis on materialism. In 1966 only 45% of the respondents nationwide selected *money: being well off* as an essential or very important life goal. By 1976 this had increased to 52% and by 1986 to 74%. Undoubtedly for some of these entering freshmen, money is viewed as a strictly secondary instrumental priority. On the other hand, it is likely to be a vital priority for others and the notion of a Yuppie type of terminal materialism may be useful here in explaining the failure of materialism to wither away among some of the affluent.

As shown in Figures 10-11, when the three items with ambiguous loadings are dropped and the remaining 18 items are factored again, the factor plots yield three tightly clustered and distinct value domains. The plot of the first and second factors in Figure 10 depicts a sharp differentiation of the libertarian and authoritarian items along both dimensions while the materialism items fall near the origins of both axes. The plot of the second and third factors show the libertarian and authoritarian items splitting on the second factor but lying close to the origins on the third, while the materialist items load heavily on the third factor but lie close to the origins on the second.

When the value scales are recomputed based on the smaller set of 18 items, the correlation between the libertarian and materialism scales drops from .13 to an insignificant but still positive .04. The correlation between the authoritarian and materialism scales also drops from .23 to .18, and if the two authoritarian items loading below .40 are removed, the correlation declines further to .14. The finding of a higher correlation between authoritarianism and materialism in the

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Figure 10. Two-Dimensional Plot of the Item Factor Loadings for the 18-Item Three-Factor Varimax Rotated Solution (horizontal axis = factor 1; vertical axis = factor 2)

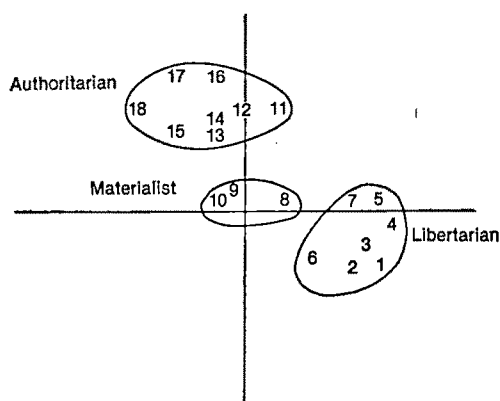
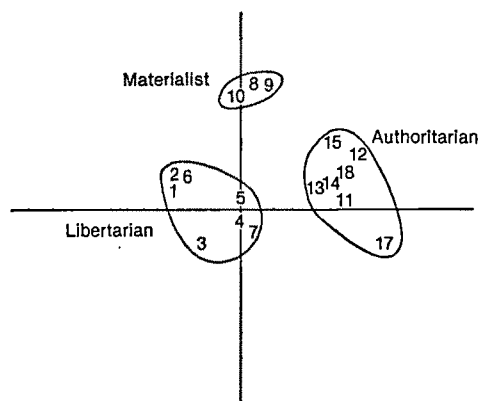


Figure 11. Two-Dimensional Plot of the Item Factor Loadings for the 18-Item Three-Factor Varimax Rotated Solution (horizontal axis = factor 2; vertical axis = factor 3)



Note: Items 1-7 are respectively the first seven libertarian items listed in Table 3, Items 8-10 are the three materialist items (omitting *working hard*), and Items 11-18 are in order of the Table 3 listing of the eight authoritarian items.

case of a class-constrained sample of university-educated respondents, where the utilitarian argument discussed above is likely to have less relevance, suggests that even among the highly educated there are still more authoritarians who are psychological materialists than there are libertarians who are terminal materialists.

Where Do We Go from Here?

Inglehart's theory of postmaterialist value change and his related research clearly constitutes one of the most important theoretical contributions in the comparative field over the last two decades. My argument here and elsewhere has been that Inglehart was correct in identifying an important value cleavage with strong relationships to vote choice and important implications for realignment. Where we differ is in my claim that he has incorrectly conceptualized and measured the value cleavage that divides the New Left

and New Right. To date, Inglehart has resisted tampering with his scale because it "works." As I have shown, however, it continues to work for the wrong reasons—namely that it is rather tapping the authoritarian-libertarian dimension while at the same time causing a forced constraint between the authoritarian and materialist items, making it very difficult to disentangle the two distinct value dimensions using his procedures.

The conceptualization of the politically salient value cleavage that divides the advanced industrial democracies has been dominated by the notion of *materialism* for too long. As the wide variety of conceptual subdimensions associated with the authoritarian-libertarian scale suggests, we now need to cast our nets more broadly to determine what clusters with what, and in this regard much work remains to be done. While the results reported above appear to provide definitive proof for my arguments, some cautions are in order. A sample that cut across all levels of educa-

tion might have yielded somewhat higher correlations between the authoritarian and materialism scales, due to the utilitarian argument. Also, in other countries cultural differences might somewhat affect what does and does not cluster within the authoritarian and libertarian factor spaces. Notwithstanding these caveats, the above evidence makes a strong case for now moving beyond Inglehart's original scale and developing two distinct scales. The two-scale approach is superior because it enables us to distinguish between the materialist-non-materialist value-priority dimension, which simply identifies whether the Old Politics economic issues or New Politics value issues are of higher salience to the respondent, and the authoritarian-libertarian dimension, which identifies whether the respondent is likely to support the New Left or New Right issue agendas.

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Notes

1. My response to Inglehart's new contribution grew out of my review of a conference paper he presented at the IPSA conference in Paris in July 1985, which in published form was divided into two articles, including the above offering and a second one published elsewhere (Inglehart and Rabier 1986). My comments, therefore, will occasionally refer to this second companion piece as well.

2. This Japan's Successor Generation survey was conducted under the sponsorship of the United States Information Agency in October and November of 1984, yielding 803 completed questionnaires of university-educated Japanese between the ages of 20 and 91. I am greatly indebted to the principal investigator of the study, James S. Marshall of the East Asia and Pacific Branch of the USIA's Office of Research, for including my values battery in the survey and making the data available to me (see Marshall 1985).

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RESEARCH NOTE

SOPHISTICATED SINCERITY: VOTING OVER ENDOGENOUS AGENDAS

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The empirical findings on whether or not legislators vote strategically are mixed. This is at least partly due to the fact that to establish any hypothesis on strategic voting, legislators' preferences need to be known, and these are typically private data. I show that under complete information, if decision making is by the amendment procedure and if the agenda is set endogenously, then sophisticated (strategic) voting over the resulting agenda is observationally equivalent to sincere voting. The voting strategies, however, are sophisticated. This fact has direct implications for empirical work on sophisticated voting.

Over the past decade, considerable theoretical attention has been devoted to voting in committees. The focus has been on two related questions. First, given an arbitrary agenda, what is the structure and consequence of strategic (sophisticated) voting and how does this differ from sincere (naive) voting? Second, given strategic voting and an arbitrary status quo, what final outcomes can a monopoly agenda setter reach by an appropriate choice of agenda? For decision making under the amendment procedure¹ with complete information, the answers to these questions are now known (see, in particular, Farquharson 1969, McKelvey and Niemi 1978, and Moulin 1979 on the first problem; and Banks 1985, Miller 1980, and Shepsle and Weingast 1984 on the second).

In the wake of the theoretical advances, empirical work on legislators' voting behavior has become increasingly directed toward trying to discover whether legislators do in fact vote strategically (Bjurulf and Niemi 1978;

Denzau, Riker, and Shepsle 1985; Enelow 1981; Enelow and Koehler 1980; Krehbiel and Rivers 1985). For if they do, then any legislator's voting behavior cannot be used unequivocally as an instrument to reveal his or her (legislative) preferences. The findings are mixed: this is at least partly because, as Krehbiel and Rivers remark, "to assess the voting strategies of [legislators] one needs to know [their] preferences" (1985, 3), and preferences are private information. Bjurulf and Niemi and Enelow (and Koehler) find evidence of strategic voting; Denzau, Riker, and Shepsle examine a case in which some legislators appear to vote strategically and others do not; and Krehbiel and Rivers are unable to reject the hypothesis of sincere voting.

To account for legislators not voting strategically to promote their *prima facie* interests, Denzau, Riker, and Shepsle extend the domain of individual preferences to "home-style" constituency concerns. With this wider framework, they show that there are situations in which it

is rational not to vote strategically: the cost of doing so in terms of the probability of reelection is too high. Because preferences and motives are private information, constituents are unable to disentangle the reasons why a legislator voted in any particular way. Since they can never be sure whether their representative was voting strategically or simply against their interests, the legislator faces a risk, when deciding to vote strategically, that constituents will believe the legislator is voting against their interests. The evidence that "home-style" considerations are important in voting is strong (Kingdon 1973). But the Denzau, Riker, and Shepsle explanation is problematic in that it makes it even more difficult to test any hypothesis of strategic voting. If a legislator is observed to vote sincerely when there exists an opportunity to be strategic, it is *prima facie* impossible to reject the hypothesis of sophisticated behavior (relative to the broader domain of preferences): the unobserved "home-style" costs may be too high.

Krehbiel and Rivers (1985) estimate legislators' preferences and find that sophisticated and sincere voting coincide for the issue they study. Consequently, "failure to reject sincere voting does not automatically imply rejection of sophisticated voting" (p. 26). They conjecture that, since the issue was one-dimensional and a median most-preferred outcome existed, a process of amendments and counteramendments resulted in this median being proposed. Although the existence of a median on the agenda does not, in and of itself, guarantee that sincere and sophisticated voting will coincide, the suggestion is that the locus of strategic behavior may be more at the agenda-setting stage of the process than at the subsequent voting stage.

I show that this suggestion generalizes. If decision making is by the amendment procedure, if the agenda is set endogenously, and if information is complete,

then sophisticated voting over the resulting agenda is observationally equivalent to sincere voting. The voting *strategies* that support the observed voting behavior, however, are sophisticated in the classical sense. The intuition behind the result is straightforward. Given an arbitrary agenda, a legislator knows that rational committee members will vote strategically. So the legislator takes these voting strategies into account when selecting a proposal or amendment to put on the agenda: proposing is "sophisticated." And given the structure of sophisticated voting on binary agendas, this results in choosing proposals and amendments that defeat previously proposed alternatives under sincere voting. The class of agenda-setting procedures explicitly covered by the result is quite broad. Effectively, any mechanism satisfying the following three criteria fall within it: (1) legislators are given at most one opportunity to offer an alternative; (2) proposals are made openly within the committee; and (3) the agenda is set sequentially, that is, the s^{th} proposal on the agenda can be offered only after the first $(s - 1)$ proposals have been fixed and before the $(s + 1)$ -to-final proposals are volunteered.

In the next section, the model and the result, which is technically quite trivial, are developed formally. A final section offers some concluding remarks.

Model and Result

The model is fairly standard. A committee N consists of an odd number, $n \geq 3$, of individuals, $N = \{1, 2, \dots, n\}$. Each individual $i \in N$ has preferences defined on the k -dimensional issue space, R^k : i 's preferences are assumed representable by a continuous and strictly quasi-concave utility function, $u_i: R^k \rightarrow R$. The committee makes a choice from the alternative space, X ($R^k \supseteq X$), using simple majority voting over an agenda based on

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the amendment procedure. An agenda of length t is a t -tuple $y = (y_1, y_2, \dots, y_t) \in X^t$. The agenda is *based on the amendment procedure* if the sequence of votes over the agenda is given by first putting y_t against y_{t-1} ; putting the winner of this contest against y_{t-2} ; and so on until y_1 is paired against the winning alternative in the $(t - 2)^{\text{th}}$ contest: the winner of this last contest is the *final outcome*. Let $\Sigma(y)$ be the voting tree defined by this process. Hereafter, leave the dependency of $\Sigma(\cdot)$ on y implicit, and write $\Sigma(y) \equiv \Sigma$.

The agenda is determined endogenously. The mechanism assumed is as follows.² If there is a historically given status quo, labeled $b(0)$, then this occupies the position of y_1 in any agenda. Thereafter, alternatives are offered by eligible committee members in a sequence, π . Although all committee members have voting rights, it is not assumed that all committee members are necessarily eligible to make proposals: agenda setting may be confined to a subcommittee. Let $M, N \supseteq M$, be the set of legislators eligible to offer proposals and amendments. Let $|M| = m$.

The first legislator under π is chosen by some (possibly degenerate) lottery over M . Let $p(j = i_1)$ be the probability in this lottery that legislator $j \in M$ is the first-ranked individual in π . Assume $p(j = i_1) \in [0, 1]$, $\forall j \in M$, and $\sum_M p(\cdot) = 1$. Once the first-ranked individual is chosen, he or she determines the second alternative on the agenda, y_2 : call this proposal $b(1)$. Once $b(1)$ is revealed, the second individual under π is selected by a second lottery from the remaining $m - 1$ eligible legislators, and this individual then determines y_3 , denoted $b(2)$. In general, given the sub-sequence $\pi_{s-1} = (i_1, i_2, \dots, i_{s-1})$, where i_r is the r^{th} -ranked individual in π , and given proposals $\{b(0), \dots, b(s-1)\}$, the s^{th} -ranked legislator is chosen by lottery from legislators in $M \setminus \{i_1, \dots, i_{s-1}\}$, $s = 2, \dots, m$. For any $j \in M \setminus \{i_1, \dots, i_{s-1}\}$, the probability that j is selected to

make proposal $b(s)$ is given by $p(j = i_s | \pi_{s-1}) \in [0, 1]$: by assumption, $\sum_{M \setminus \{i_1, \dots, i_{s-1}\}} p(\cdot | \pi_{s-1}) = 1$ (so that $p(j = i_s | \pi_{s-1}) > 0$ for at least some $j \in M \setminus \{i_1, \dots, i_{s-1}\}$). So while the lottery at any stage s can depend on the subsequence π_{s-1} , it is presumed independent of the proposals $\{b(0), \dots, b(s-1)\}$. By convention, write $p(j = i_1) \equiv p(j = i_1 | \pi_0)$, $\forall j \in M$. By definition, $\pi = (i_1, \dots, i_m)$ and $\forall j \in M$, $\exists s \in \{1, \dots, m\}$ such that $j = i_s$.

All the probabilities are common knowledge and can be used at each stage of the process to compute the likelihood of any given sequence, π , occurring. Let $\mu(\pi | \pi_s)$ be the probability that the de facto sequence of proposers is π , given the realized subsequence π_s , $s = 0, \dots, m - 1$. For example, if $M = \{1, 2, 3, 4, 5\}$ and $\pi_2 = (4, 2)$, then

$$\begin{aligned} \mu[(4, 2, 1, 3, 5) | \pi_2] &= p[1 = i_3 | (4, 2)] \\ &\bullet p[3 = i_4 | (4, 2, 1)] \\ &\bullet p[5 = i_5 | (4, 2, 1, 3)] \\ &= p[1 = i_3 | (4, 2)] \\ &\bullet p[3 = i_4 | (4, 2, 1)]. \end{aligned}$$

Evidently, $\mu(\pi | \pi_{m-1}) \in \{0, 1\}$ for all π .

At any stage $s \geq 1$ in the agenda-setting process, if the s^{th} legislator (i_s) does not wish to offer any alternative at all or any not already proposed, then $b(s) = \emptyset$ (where, by an abuse of notation, $\emptyset \in X$): under the amendment procedure, this captures the parliamentary rule preventing reconsideration of previously eliminated alternatives. The final agenda, then, is at most of length $m + 1 \leq n + 1$ and is given by $b = [b(0), b(1), \dots, b(m)]$. Once b is determined, voting proceeds according to the amendment procedure to arrive at the committee decision.

To save on notation, given any subsequence π_s , $s \leq m$, relabel individuals in N so that $i_r = r$, $\forall r = 1, \dots, s$. For every $s = 1, \dots, m$, let $B_s = \{b(0), \dots, b(s-1)\}$. A (*pure*) *proposal strategy* for indi-

vidual $i \in M$ is then a function: $b_i: X^i \rightarrow (X \setminus B_i) \cup \emptyset \equiv \beta_i$. For legislators $j \in N \setminus M$, define the (vacuous) proposal strategy, b_j , by setting $\beta_j \equiv \emptyset$.

Let $|\Sigma|$ be the number of possible pairwise contests identified by branches of the voting tree, Σ . A (pure) voting strategy for individual $i \in N$ is a function: $v_i: \Sigma \rightarrow \{0, 1\}^{|\Sigma|}$. In any pairwise contest, a value 1 denotes a vote for the alternative proposed later in the agenda (i.e., with the larger index under π), and a value of 0 denotes a vote for the alternative proposed earlier.

Since a legislator will cast at most m votes during the voting process, i 's voting strategy can be decomposed, namely, $v_i = \{v_{it} \mid t = 1, \dots, m\}$, where t indexes the voting stage ($t = m$ being the final vote). Legislator i votes sincerely at stage t , $t = 1, \dots, m$, if and only if for any $j > m - t$,

$$u_i[b(j)] > (<) u_i[b(m - t)] \\ \rightarrow v_{it}([b(j), b(m - t)]) = 1 (0).$$

If ever a legislator is indifferent and sincere at stage t , assume he or she votes surely for the alternative with the higher index under π (with endogenous agenda setting, this turns out to be the unique equilibrium strategy [Banks and Gasmir 1986]). Legislator i is said to vote sincerely if and only if i votes sincerely at every stage $t = 1, \dots, m$.

A (pure) strategy for legislator i is thus an ordered pair, $\sigma_i = (b_i, v_i)$, $i \in N$. Let $\sigma = (\sigma_i)_N$ and $\sigma_{-i} = (\sigma_1, \dots, \sigma_{i-1}, \sigma_{i+1}, \dots, \sigma_n)$. Given any list of strategies, σ , let $y(\sigma)$ be the final outcome of the decision-making process. The noncooperative solution concept used for the extensive-form committee game induced by the structure described above can now be defined. An equilibrium is a list of strategies $\sigma^* = (\sigma_i^*)_N$ such that, $\forall i \in N$:

1. $b(i)^* = b_i^*(\cdot)$ maximizes $Eu_i[y((b, v_i^*), \sigma_{-i}^*)]$ over β_i , where the expectation

operator E is with respect to the sequence π .

2. $v_i^* = \{v_{it}^*\}$ is such that $\forall t = 1, \dots, m$, conditional on i being pivotal at each stage t , v_{it}^* maximizes $u_i[y((b_i^*, v), \sigma_{-i}^*)]$ over $\{0, 1\}^{|\Sigma|}$.

Because there is complete information and decision making is by the amendment procedure, in equilibrium legislators will adopt sophisticated voting strategies in the sense of Farquharson (1969), and McKelvey and Niemi (1978). So, taking this as given, it is enough to consider the agenda-setting stage of the game.

For any policy $x \in X$, define the win set of x , $W(x) = \{y \in X \mid \{i: u_i(x) > u_i(y)\} > n/2\}$, and let $W^c(x)$ be its closure. If $W(x) = \emptyset$ for any alternative x , x is a Condorcet winner. By the assumptions on individual preferences, for any alternative x , $W(x)$ is open. Therefore, if ever an individual is constrained to choose an alternative in $W(x)$ and $W(x)$ is nonempty, i 's maximization problem is not well defined. However, by the observation made earlier regarding equilibrium voting strategies for pairs of alternatives over which an individual is indifferent, the statement, "Maximize over $W(x)$ " can always be replaced (without loss of generality) by the statement, "Maximize over $W^c(x)$," which is well defined. Hereafter, this is left implicit.

THEOREM. Suppose there is no Condorcet winner in X . Then there exists a unique equilibrium σ^* to the game, and the observed voting behavior of all legislators in equilibrium is sincere.

Proof. To save on notation (and without loss of generality), set $M = N$. For any legislator j , define, $Q(j) = \{\cap W[b(s)]\} \cup \emptyset$, where the intersection is taken over all $b(s) \in B_j$. Given π_{n-1} and $B_n = \{b(0), \dots, b(n-1)\}$, legislator i_n offers the final proposal. Without loss of generality, set $i_n = n$. Hence,

Sophisticated Voting

$$b_n^*(B_n) \in \{b(n) \in \beta_n \mid b(n) \\ \in \operatorname{argmax}_{Q(n)} u_n(b)\}$$

is, given sophisticated voting, a best response for legislator n . Given π_{n-2} , the individual offering the penultimate proposal knows surely the identity of the final proposer, $i_n (= n$, by assumption). Moreover, n 's proposal strategy is uniquely defined. Again without loss of generality, label N so that $i_{n-1} = n - 1$. Then legislator $n - 1$ can do no better than to adopt

$$b_{n-1}^*(B_{n-1}) \in \{b(n-1) \\ \in \beta_{n-1} \mid b(n-1) \\ \in \operatorname{argmax}_{Q(n-1)} u_{n-1}[b_n^*(\{b \\ \cup B_{n-1}\})]\}.$$

Given π_{n-3} , there are two possible de facto sequences π that can occur, depending on the outcome of the final lottery over $\{n, n-1\}$ to determine who proposes the penultimate alternative on the agenda. But whichever sequence occurs, the proposal strategies of the remaining proposers are well defined. Therefore, individual i_{n-2} (identified by assumption with $n-2$) has a best response to adopt,

$$b_{n-2}^*(B_{n-2}) \in \{b(n-2) \\ \in \beta_{n-2} \mid b(n-2) \\ \in \operatorname{argmax}_{Q(n-2)} Eu_{n-2}(\cdot)\}$$

where

$$Eu_{n-2}(\cdot) = \sum_{\pi} \mu(\pi \mid \pi_{n-3}) \\ \cdot u_{n-2}[b_{m(\pi)}^*(b_{(m-1)(\pi)}^*(\{b \\ \cup B_{n-2}\}))]$$

and $m(\pi)$ is the final proposer under π , and so on. Proceeding iteratively in the obvious manner, define analogous proposal strategies for all i_{n-3}, \dots, i_1 . By construction, this set of proposal strategies is well defined, constitutes an equilibrium, and is the unique such set. However, by sophisticated voting, for some

$Q(j)$, j 's best payoff can be invariant across several feasible proposals, or none. Therefore, there may exist several equilibrium agendas induced by σ^* .

In the equilibrium, voting strategies are sophisticated. Consider voting behavior along the equilibrium path for any agenda b^* , derived from the specified proposal strategies. At the final nonvacuous voting stage of this path—that is, $b(s)$ versus $b(0)$, $n \geq s \geq 1$ and $b(s) \neq \emptyset$ —all legislators surely vote sincerely. Because there is no Condorcet winner, $W[b(0)] \neq \emptyset$, and such a stage exists. By construction, $b(s) \in W[b(0)]$, so $b(s) = y(\sigma^*)$ is the final outcome. Consider the penultimate voting stage in which $b(1)$ is put against an alternative $b(j) \neq \emptyset$, $n \geq j \geq 2$. Then, $y(\sigma^*) \in \{b(j), b(1)\}$. If there is no such alternative, we are done. So suppose there is a nonvacuous alternative. If $y(\sigma^*) = b(1)$, then, by the proposal strategies, $W[b(1)] = \emptyset$ implying $b(1)$ is a Condorcet winner: contradiction. Hence, $y(\sigma^*) = b(j)$. By construction, $b(j) \in Q(j)$. Clearly, any individual $i \in N$ for whom $u_i[b(j)] > \max\{u_i[b(1)], u_i[b(0)]\}$ will vote sincerely at this stage. Similarly, since $b(1)$ and $b(j)$ are in $W[b(0)]$, any individual i for whom $\min\{u_i[b(1)], u_i[b(0)]\} > u_i[b(j)]$ has sincere voting as a weakly dominant strategy here. Suppose $i \in N$ is such that $u_i[b(1)] > u_i[b(j)] > u_i[b(0)]$. Then because $b(1) \in W[b(0)]$ by construction, i has a weakly dominant strategy to vote sincerely. Finally, suppose $i \in N$ is such that $u_i[b(0)] > u_i[b(j)] > u_i[b(1)]$. By construction, $b(j) \in W[b(1)] \cap W[b(0)]$ and $b(1) \in W[b(0)]$. So it is again a weakly dominant strategy for i to vote sincerely at this stage. Hence sincere voting over $\{b(j), b(1)\}$ is a weakly dominant strategy for all individuals. By sincere voting at the final stage, definition of $b(j)$ implies all legislators vote sincerely at this penultimate stage. Now apply an obvious inductive argument to complete the proof. QED

COROLLARY. Suppose there exists a Con-

dorcet winner, x^* , in X . Then there exists an equilibrium σ^* to the game, which is unique up to the proposal of x^* . The observed voting behavior of all legislators in equilibrium is sincere, except possibly at voting stages up to and including the first vote involving x^* .

Proof. For any legislator $j \in M$ such that $Q(j) \neq \emptyset$, the proposal strategy specified in the proof of the theorem is the best response. Suppose there exists $i \in M$ and $b(i) \in B_i$ such that $W[b(i)] = \emptyset$. Then $Q(i) = \emptyset$. Set $b(j) \equiv x^*$. By a theorem of McKelvey and Niemi (1978), x^* will be the final outcome under sophisticated voting whatever alternative i elects to propose. Hence, any proposal strategy is a best response for i . In particular, if $b(i) \neq \emptyset$ and $b(i) \notin \cap \{W[b(t)] \mid b(t) \in B_i \text{ and } W[b(t)] \neq \emptyset\}$, then sophisticated voting along the equilibrium path need not be sincere. Moreover, by the argument for the theorem, a necessary condition for voting not to be sincere at some stage for some legislator is that there be at least one such proposal $b(i)$. The corollary now follows from the theorem. QED

The corollary identifies the logically possible exception to the claim that, with endogenous agenda setting, sophisticated voting is always observationally equivalent to sincere voting. Having said this, three points should be emphasized. First, the conclusion of the theorem goes through if a Condorcet winner x^* is not proposed, even though it exists. Second, if x^* is placed on the agenda, then making no further proposals is equilibrium behavior, as is offering alternatives that can defeat (under sincere voting) all those previously proposed other than x^* . In both these cases, voting along the entire equilibrium path will be observationally equivalent to sincere voting. Thus, even when there exists a Condorcet winner x^* , there always exist equilibria to the game, in which sincere and sophisticated voting

coincide along the equilibrium path; and these equilibria are unique up to the proposal of x^* . Third, in the spatial context at least, the existence of a Condorcet winner is generically confined to one-dimensional issues.

Finally, note that if there exists a Condorcet winner x^* in X and if all legislators are eligible to propose alternatives ($M = N$), then x^* is surely the outcome to the agenda game above. To see this, simply note that by Plott's (1967) theorem and n -odd, there exists an individual $j \in N$ with ideal point x^* . Therefore x^* will be placed on the agenda.

Conclusion

If committee decision making is by the amendment procedure and the agenda is determined endogenously (either by a subcommittee or by the committee of the whole), then the theorem offers an explanation for why legislators are observed to vote sincerely. Furthermore, along the equilibrium path, no individual will vote against his or her own proposal should this be put against an alternative offered earlier in the agenda-setting sequence.³ This explanation does not rest on widening the domain of legislator preferences.⁴ Nevertheless, it is worth reiterating that the observed behavior is supported by sophisticated voting strategies. In empirical work attempting to test for strategic voting, therefore, it is important to determine whether or not agenda setting is endogenous. If it is, then, *ceteris paribus*, the theorem predicts that a hypothesis of sincere voting will not be rejected.

The key to the result is that sophisticated voting over an agenda induces legislators to confine their proposals to alternatives that can beat—under sincere voting—the proposals offered earlier in the agenda-setting process. Since the formal structure of sophisticated voting is invariant across the class of binary agen-

das (McKelvey and Niemi 1978), this suggests that the theorem holds not only for the amendment procedure, but also for any binary agenda and (binary) agenda-setting mechanism.

Notes

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1. Formal definitions of the amendment procedure and related concepts are given in the next section.

2. For related models of endogenous agenda setting, see, for example, Banks and Gasmi 1986, McKelvey 1986, Miller 1985, and Miller, Grofman, and Feld 1986.

3. Unfortunately, the observation that legislators behave as if constrained to vote for their own proposals against those offered previously has not been formally documented. It is nevertheless a commonplace among students of Congress. In the case where a Condorcet winner, x^* , has been placed on the agenda, a legislator can ensure against having to vote for alternatives previously placed on the agenda against his or her own proposal—if this is made after the appearance of x^* on the agenda—only by proposing nothing or by offering an alternative that beats all previously volunteered proposals other than x^* . (Both constitute equilibrium strategies here.) Thus, if legislators, for whatever reasons, do feel obliged to vote for their own proposals against those offered previously, the logically possible exception (identified in the corollary to the theorem) to the claim that sophisticated and sincere voting observationally coincide with endogenous agenda setting vanishes.

4. It has been rightly observed that, just as the model here excludes “home-style” reasons for voting behavior, it excludes similar (“position-taking”) reasons for proposal behavior. These may well prove important, giving rise to a distinct model of strategic agenda setting. Having said this, there is in fact some room for position taking in the model here, as the corollary makes clear. If there is an underlying Condorcet winner (x^*) and if this is put on the agenda before all eligible committee members have made a proposal, then subsequent proposers can “position take,” since whatever they offer will lose to x^* under sophisticated voting.

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REVIEW ESSAYS

Women and Politics in Western Europe.

Edited by Sylvia Bashevkin (Totowa, NJ: Frank Cass, 1986. 101p. \$29.50).

Women of Europe: Women MEPs and Equality Policy. By Elizabeth Vallance and Elizabeth Davies (New York: Cambridge University Press, 1986. x, 180p. \$37.50).

Women, State, and Party in Eastern Europe.

Edited by Sharon L. Wolchik and Alfred G. Meyer (Durham, NC: Duke University Press, 1985. xiv, 453p. \$42.50, cloth; \$16.95, paper).

The study of women and the politics of individual European nations has been less developed than the study of women in U.S. politics for a variety of reasons. Truly comparative studies of women and politics have been even rarer. Yet more difficult to find are analyses of women in international politics and organizations. These three books, therefore, are bound to make a contribution to our knowledge despite some unevenness in their quality.

Women and Politics in Western Europe is a reprint of a special issue of *Western European Politics*. It begins with an essay on the women's movement and the state by Jane Jenson that claims to be general in its focus but actually focuses on France. Of the remaining articles, one deals with the connection between the Italian women's movement and Communist party by Karen Beckwith; one is on women, the women's movement, and the French Socialist government (Wayne Northcutt and Jeffra Flaitz); and three are more comparative in nature, including Lawrence Mayer and Roland Smith's piece on electoral behavior and religiosity in Italy, West Germany, and the Netherlands, Ingunn Norderval's article on politics in Scandinavia, and Pippa Norris's study of legislative participation in Western Europe.

Women of Europe focuses on women and the European Community (EC) and European Parliament. Based on documentary and interview analysis, it considers both the role of women as participants in EC politics and European legislation as it affects women. The book provides necessary background material on

the structure and processes of the EC and its relationship to member nations.

Women, State, and Party in Eastern Europe is an anthology based on papers presented to a 1981 conference on women in Eastern Europe that includes articles by both U.S. and Eastern European scholars. It is divided into five sections. Part 1, "Conditioning Factors," includes one article on Marxist and feminist theory (Meyer) and one on development and transformation (Wolchik). Part 2, on the pre-Communist period, includes two articles on Czech women (Karen Freeze and Bruce M. Garver), one on Ukrainian feminism in Poland (Martha Bohachevsky-Chomiak), and one on peasant women in Croatia (Mary E. Reed). Part 3, "Women and Politics," includes case studies of Romania (Mary Elen Fischer) and Albania (John Kolsti) and comparative studies of Romania and Poland (Daniel B. Nelson) and Czechoslovakia and Poland (Barbara Jancar). Part 4, "Women and Work," includes work on Hungary (Rozsa Kulcar, Ivan Volgyes), Yugoslavia (Silva Meznaric, Susan L. Woodward), and Czechoslovakia (Alena Heitlinger) and two comparative studies (Robert J. McIntyre and Bogdan Mieczkowski). Finally, part 5, "Women's Voices," looks at Poland (Renata Siemienska), Romania (Gail Kligman) and East Germany (Dorothy Rosenberg).

The research presented in *Women, State, and Party* employs a rich variety of sources and approaches, including theoretical treatments; historical and documentary evidence; census materials; surveys; and literature, song, and ritual explorations. The major sections of the book are introduced by synthesizing essays by Wolchik.

These three books, separately and together, raise a series of questions that are or should be central in the study of women and politics. All three include policy change as a major theme and provide interesting evidence regarding the relative roles of political organization mobilized on the basis of feminist politics and women's interests and that mobilized on the basis of other interests. It has long been noted that some of the most important changes in women's policy takes place in the absence of a

strong feminist movement. This theme is taken up most directly in the two anthologies. Both raise the particularly interesting question of the uses "women's policy" serves for state and party. A related question, also a theme in the anthologies, is the organizational relationship between women's or feminist groups and party organizations. One wishes, however, that more attention had been paid in both volumes to distinguishing carefully between *women's* groups and *feminist* groups.

Both *Women of Europe* and *Women, State, and Party in Eastern Europe* focus our attention on one of the questions that is most unfortunately neglected elsewhere: the role of international politics in gender relations and policy. The agencies of the EC have gained importance in affecting the condition of women in member nations; we must be grateful to Vallance and Davies for taking up this topic. Many of the articles in *Women, State, and Party* underscore the need to recognize an international politics of gender in Eastern Europe, not just because of the relationship between the Soviet Union and Eastern European states, but because of the historical relationships among Eastern European communities.

The link between religion and gender politics is also a subtheme in both Bashevkin's anthology and Wolchik and Meyer's. Whether the gender relevance of politics and religion is viewed through the lens of the affinity between religion and some political parties of the West, as treated as Bashevkin's book, or through struggles between religion and state, as is often the case in Eastern Europe, it is a problem that needs more work. These two books serve as a useful corrective to feminist scholars whose understanding of gender, religion, and politics has been exclusively embedded in the peculiar relationship of religion to politics in the United States.

Each book, of course, has its own merits and problems. *Women and Politics in Western Europe* is uneven in quality. Its particular high points are Beckwith's rich article on the women's movement and party in Italy and the comparative syntheses of Scandinavia by Norderval and Western Europe by Norris. The most serious criticism is owed to the publisher: Frank Cass and Company should note that readers grow weary of paying 30 dollars for hundred-page books, especially when they are

reprints of special issues of journals already published.

Women of Europe is a pathbreaking book because of its topic, but some of the research methods are disappointing. The book is based in part on interviews, but there is no discussion of how the respondents were selected or how the interviews were analyzed. One is often left with no sense that the choice was systematic or representative of anything in particular. There is considerable use of anecdotal reporting, which leaves the scholarly reader with no confidence in what observations to accept and what not to accept. There is a considerable literature on gender and politics available that should have been used but was barely touched. The study is largely descriptive rather than analytical or theoretical. Finally, Cambridge University Press must share the bouquet of onions for the price of the book.

Women, State, and Party in Eastern Europe is a very good book. The range of methods and topics makes one forgive a certain unevenness in quality. This book handles the tie between theory and ideology on the one hand and practice on the other in a way the other two books do not. It usefully joins historical and contemporary research. The existence between two covers of some articles based on rich and systematic Eastern European demographic and survey data (see especially the articles on production and reproduction) and one on oral poetry and ritual in Romania that is both provocative and lovely to read is impressive. It is also good to see people based in the United States and Eastern Europe cooperating in scholarship that deserves the name.

These three books, then, each deserve to be read on their own terms. But even more, they should be read together.

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Book Reviews: Review Essays

Why We Lost the ERA. By Jane J. Mansbridge (Chicago: University of Chicago Press, 1986. xii, 327p. \$30.00, cloth; \$9.95, paper).

Why ERA Failed: Politics, Women's Rights, and the Amending Process of the Constitution. By Mary Frances Berry (Bloomington: Indiana University Press, 1986. 147p. \$12.95).

Rights of Passage: The Past and Future of the ERA. Edited by Joan Hoff-Wilson (Bloomington: Indiana University Press, 1986. xx, 140p. \$22.50, cloth; \$6.95, paper).

Serious attempts to amend the U.S. Constitution are rare. Even rarer are attempts to amend the Constitution that prove unsuccessful despite the mobilization of thousands of people and the investment of hundreds of thousands of dollars. On this basis alone, the proposed Equal Rights Amendment to the Constitution merits serious scholarly attention. However, the struggle over the ERA is important not only because of the insights it can offer about the amending process but also because of the central role it has played in one of the most significant social movements in U.S. history. Indeed, the ERA became the major focal point for the U.S. women's movement during the late 1970s and early 1980s.

Individually and collectively, these three books represent an important first step toward understanding the history and struggle surrounding the ERA. While all of the authors agree that in some ways the unsuccessful attempt to ratify the ERA was worth the effort, the three books take different approaches and offer distinctive explanations for the ultimate failure of the proposed amendment.

Jane Mansbridge argues that the ERA failed because a majority of citizens did not want major changes in gender roles. Both sides in the ERA dispute exaggerated the probable effects of the amendment, which Mansbridge concludes would have had little short-term impact on the status of women. As a result, many state legislators came to fear that the ERA might lead to major substantive changes, and they were reluctant to give the Supreme Court new language that might lead to more progressive decisions in the area of gender relations.

Mansbridge claims that the polarization of views evident in the ERA campaign and the exaggeration of the probable effects of the pro-

posed amendment stemmed largely from the voluntary nature of movement participation. The need to mobilize volunteers and the reliance on their efforts fostered ideological purity, decisions by accretion, and lack of accountability, which ultimately worked against the proponents' goal of ratification. The positions taken by ERA proponents on the issues of women in combat and the relationship of abortion to the ERA illustrate these tendencies. Mansbridge suggests that feminists might have won more support from legislators if they had argued for a "deferential" interpretation on the question of whether the ERA would require women to be sent into combat. However, the argument that the war-powers clauses of the Constitution would lead the Supreme Court to defer to military judgment in military matters was never really seriously considered within the pro-ERA ranks. Rather, proponents automatically opted for an "egalitarian" interpretation, consistent with their commitment to the general principle that the law should be gender blind, and argued that women should be sent into combat on the same basis as men.

On the abortion issue, the relatively autonomous actions of feminist lawyers, who were not accountable to national leadership, were partially responsible for linking abortion to the ERA. In several states feminist attorneys brought suits arguing that restrictions on public funding of abortions were unconstitutional under due-process and equal-protection clauses of state constitutions and under state ERAs. The arguments put forward by feminist lawyers in state courts made it more difficult for feminists in unratified states to argue convincingly that a federal ERA would not require federal funding for abortion. Although Mansbridge claims that tendencies toward ideological purity, decisions by accretion, and lack of accountability can be countered by institutional arrangements that foster sustained internal dialogue within organizations, it is difficult to imagine a social movement that would not show these tendencies.

The major problem with Mansbridge's intriguing and provocative book is that it appears on the surface to be a more comprehensive and nationwide study of the ERA ratification effort than it really is. One has to search for a description of the study's methodology, which is found in a footnote to the

preface. Although the author conducted almost a hundred interviews, the majority of these were with activists and legislators involved in the Illinois campaign. Of the interviews conducted with individuals outside Illinois, a significant number were with lawyers, and indeed one of the great strengths of this book is the insight it provides into the thinking and actions of attorneys involved in the ERA struggle. Despite Mansbridge's claim that the patterns and processes she observed in Illinois, the only nonsouthern state apart from Mormon Utah to fail to ratify the amendment, were "probably quite typical" (p. 177), one cannot help wondering if and how the book might have been different had her interviews focused primarily on the ERA struggle in Utah or Florida or Virginia. Despite an occasional quote from National Organization of Women President Eleanor Smeal, the perspective of national leaders in the ERA movement also seems largely absent from the book. Thus, *Why We Lost the ERA* is most appropriately read as an in-depth study of the Illinois ratification effort framed in the larger context of the national struggle over the ERA, and as such it makes an important contribution to our understanding of the politics of the ERA and of social movements generally.

Mary Frances Berry adopts an approach to examining the ERA quite different from that of Mansbridge. Berry's book places the ERA in historical perspective by comparing the circumstances surrounding the attempt to ratify the ERA with the circumstances surrounding past attempts, both successful and unsuccessful, to amend the Constitution. Specifically, she compares the ERA campaign with attempts to add to the Constitution amendments on the status of blacks, an income tax, prohibition, woman suffrage, and child labor.

Berry maintains that the defeat of the ERA was predictable when examined in historical perspective. The framers of the Constitution intentionally made it difficult to amend the document. Successful amendments have required a consensus that constitutional change is necessary not only at the national level but also within enough states to achieve ratification. Successful attempts to amend the Constitution have taken place during eras of reform, not reaction, and they have required a widespread perception that a social problem

could not be remedied without constitutional change.

Proponents of the ERA were successful in building the national consensus necessary to obtain congressional passage of the amendment in 1972. However, proponents did too little too late in trying to build a state-by-state consensus. Moreover, in the decade following congressional passage of the ERA, the Supreme Court broadened its protection of women under the Fourteenth Amendment, making it harder for proponents to demonstrate the necessity of the amendment. Support for the ERA actually eroded during the final stages of the ratification campaign.

Berry seems more optimistic than Mansbridge about the future of the ERA. Unlike Mansbridge, she holds out the possibility that the ERA might someday be added to the Constitution. To this end, she concludes her book with one paragraph of general advice for ERA proponents based on her analysis of how and why past amendments succeeded, which leaves the reader longing for a more detailed and expansive examination of the implications of her argument for the future of the ERA. Nevertheless, Berry's scholarly caution does not seriously detract from the important and compelling argument of her book.

Like Berry's book, the goal of the volume edited by Joan Hoff-Wilson, clearly intended as a text for classroom use, is to place the defeat of ERA in historical perspective. An impressive array of historians, political scientists, and other scholars engaged in relevant research examine the origins of the ERA, the reasons for its defeat, and its legal and political impact. The section on origins includes informative essays on Alice Paul, who drafted the ERA and had it introduced into Congress in 1923, and Florence Kelley, a politically active social reformer who opposed the ERA. The defeat of the ERA is examined from many perspectives: as a cultural conflict over the meaning of womanhood, as a non-zero-sum game, as the result of proponents' reluctance to use civil disobedience and nonviolent direct action, and as the product of supporters' failure to devise effective persuasive appeals.

Unlike many edited volumes, the editor's introductions to this book and to its various sections not only link together the essays of the various contributors but also make an impor-

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tant contribution in their own right. Hoff-Wilson discusses the difference between feminism based on "radical individualism," as represented by ERA, and "relational" feminism, which seeks to preserve differences between women and men. She notes that many contemporary feminist scholars are now questioning whether equality based on individualistic and male notions of rights should be the major goal of contemporary feminism. Somehow, contemporary feminism must find a way to bridge the gap between these two distinct strains of feminism.

The books by Mansbridge and Berry are "must" reading for scholars interested in social

movements, women, and politics, or the amending process. All three books are also potentially useful as classroom texts. Mansbridge's book would be particularly appropriate for courses on social movements, Berry's book should be considered as a supplementary text for courses on the legislative process, and the Hoff-Wilson reader would seem best suited for courses on twentieth-century U.S. political history. All three books deserve consideration for use in courses on women and politics.

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POLITICAL THEORY

Strong Democracy: Participatory Politics for a New Age. By Benjamin R. Barber (Berkeley: University of California Press, 1984. xvi, 320p. \$9.95, paper).

In *Strong Democracy* Benjamin Barber argues powerfully for a government in which "all of the people govern themselves in at least some public matters at least some of the time" (p. xiv). With verve, style, passion, and insight, Barber explains how this ideal is possible, why we have never practiced it, and what conceptual and practical innovations might make it work.

Rejecting the "liberal" idea of a natural, pre-political state whose inhabitants are endowed with liberty, equality, and rights, Barber insists, correctly, that we acquire these goods through the process of governing ourselves in common. Rejecting as well the static, aggregative liberal conception of citizenship, Barber insists that citizens become capable of common purpose through the process of common governance rather than through the simple coincidence of preexisting interests. Such a politics involves activity, energy, and work. It involves institutions that help people create "public ends where there were none before" (p. 152), and individual interests that will "change shape and direction when subjected to these participatory processes" (p. 152). It involves a

concept of political knowledge that is provisional, evolutionary, and mutable, "produced by an ongoing process of democratic talk, deliberation, judgment, and action" (p. 170). It involves a political judgment that is neither "subjective" nor "objective," but rather proceeds from the "kind of 'we' thinking that compels individuals to reformulate . . . 'I want x' . . . as 'x would be good for the community to which I belong'—an operation in social algebra for which not every 'x' will be suitable" (p. 171).

"At the heart of strong democracy is talk," Barber writes (p. 173). And the section on talk is the most persuasive in the book. Strong democratic talk, he tells us, "entails listening no less than speaking; . . . is affective as well as cognitive; and . . . its intentionalism draws it out of the domain of pure reflection into the world of action" (p. 174). Barber contends, correctly I believe, that representative democracy (which he sometimes calls "thin" democracy) diminishes this kind of talk, and, in doing so, dramatically reduces the meaning and effect of citizenship.

Barber concludes with a collection of imaginative suggestions for adding strong democracy to the primary representative institutions of large-scale modern societies. He argues for "common work and common doing"—collectively creating pocket parks, urban farms,

storefront community-education centers, neighborhood skill teams, crime-watch units, and universal-citizen service. He proposes neighborhood assemblies, for populations of from 5 to 25 thousand citizens, which could deliberate, vent grievances, act as ombudsmen, and possibly form part of an initiative and referendum process. He suggests representative town meetings, office holding by lot, and handling decriminalized small offenses through new forms of lay justice. He proposes a national initiative and referendum process, which would include a mandatory tie-in with neighborhood assemblies and interactive-television town meetings for the purpose of civic education, a multichoice referendum format (in which citizens would have a choice among support strongly, support but with [a specified] objection, oppose strongly, oppose the proposal, but not the principle behind it, and so forth), and a two-stage voting process providing for two readings. He supports experimental electronic balloting, vouchers in schooling and housing (with some reservations), universal-citizen service, neighborhood-action programs, workplace democracy, and a program for redesigning public space. If we tried all these things at once, as Barber recommends, citizens would undoubtedly "fly to the assemblies"—at least until the novelty wore off. Even after ennui set in, the intellectual and emotional residue would be incalculable. Why could not a state like Oregon or a town or an innovative university adopt some of his agenda on a trial basis, building in, of course, the prudence and the ways to "institutionalize regret" (p. 308) that Barber suggests?

Barber's repudiation of static liberalism and his creative suggestions for an ever self-renewing "strong" democracy are the best parts of this book. The worst side emerges when, in order to establish a conceptual foundation for strong democracy, Barber distorts opposing views in a way that is woven so tightly into the analysis that it undoes much of the good I have described. For all Barber's preaching about "empathy" and "listening" in his sections on democratic talk, he does not himself seem to have learned to listen. He hears what he wants to hear, and when the reality is otherwise, he distorts to fit. In this respect, *Strong Democracy* sometimes reads like a series of polemics against some of Barber's favorite bugaboos, including repre-

sentative government and anarchism. In this, as in his past works, he argues by destroying straw monsters—caricatures of ideas that their adherents would never recognize.

Twelve years ago Barber gave to a photograph of uniformed officers subduing a citizen the simple caption, "Representative Democracy" (*The Death of Communal Liberty* [Princeton: Princeton University Press, 1974], 275). Nothing quite that blunt this time. Today he writes only that "the representative principle steals from individuals the ultimate responsibility for their values, beliefs, and actions" (p. 145), is "incompatible with freedom" (p. 146), "precludes the evolution of a participating public in which the idea of justice might take root" (p. 146), and produces "distrustful, passive" citizens (p. 219). That's it for representative democracy. Barber does not later qualify the words *steals*, *incompatible*, and *precludes*. He does not write an aside granting that every citizen wants and needs representation for some purposes at some times. He gives no hint, let alone discussion, of the kinds of public talk that representatives encourage and that their election inspires. This approach makes it hard to learn much about representative democracy from Barber's analysis.

Anarchism, another old enemy, receives similar treatment. Barber pulls one strand out of a complex (even inchoate and internally contradictory) set of ideas and concludes that "the anarchist perspective is the perspective of the radically isolated self for whom the world is only what it can see with its own eyes. . . . The anarchist reads the world as the realm of One [Self], where the existence of other Ones is scarcely perceived and never felt" (p. 37). And so on. Not a word on the communitarian anarchist tradition.

Although Barber deplores the present-day adversary tradition in political discourse, his own pen has been tempered in that forge. His style thus undercuts his own purpose. Rhetorical overstatement and denigrating the enemy may be required to infuse public debate with a new vision as important as this one. But the style is not compatible with the respect for others required by "strong democratic talk."

JANE MANSBRIDGE

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Book Reviews: Political Theory

Reciprocity. By Lawrence C. Becker (New York: Routledge & Kegan, 1986. 436p. \$32.50).

Although the death of political philosophy was widely predicted a generation ago, the renewed vigor of the field is epitomized by Lawrence Becker's *Reciprocity*. While many political theorists may disagree with the mode of approach, few would deny that it reflects the spreading interest in philosophical analysis to determine the foundations of political virtue.

Becker writes in the neo-Kantian tradition of "a priori moral theory," based on what he describes as "the general conception of morality." In this view, "moral judgments are judgments about what rational agents ought to do or be, period" (p. 17)—and such judgments are determined by discovering "determinable rules of participation, teleology, deontology, valuation, procedure, transformation and recognition" that satisfy the "conditions for logical validity and soundness" determined by contemporary philosophers (p. 36).

While the abstract quality of this fundamental approach may strike many political scientists as difficult, Becker seeks to provide concreteness through an unusual series of devices. Part 3 is a scholium, with lengthy bibliographic essays on a wide range of scholarly fields. Interspersed in his text are many novelistic epigraphs "meant to bring vividly to mind the intricate, subtle, and particular lives within which, and for which, moral philosophy is done"; while "nothing in the argument formally depends on them," these epigraphs are also a means of providing concreteness (p. xi).

Becker's main thesis is that the abstract logical criteria for a "general theory of morality" lead to a "virtue theory" based on reciprocity. His definition of reciprocity, however, differs from that in most social psychology and game theory (pp. 354–68). For Becker, reciprocity must be the "disposition" to reciprocate for benefits received, whether they have been requested or not; such reciprocation need not be narrowly "in kind," but it does rest on an asymmetry between good and evil. According to Becker, while "good received should be returned with good," maxims concerning evil are quite different: "Evil received should not be returned with evil" and "Evil received should be resisted" (p. 74).

In part 2, Becker spells out the implications of the resulting view of morality for families, for future generations, and for the law. Throughout, he addresses philosophic literature from the ancients to such contemporaries as Nozick and Rawls. In short, Becker's *Reciprocity* is a serious attempt to confront contemporary ethical issues from the neo-Kantian perspective, focusing on the need to return to the kind of theory of dispositions or virtues traditionally associated with Aristotle and more recently emphasized by Alasdair MacIntyre.

For me, such an approach encounters the fundamental difficulty inherent in most theories supposedly based on a priori logic. According to Becker, one of the first requisites of "well-defined" rules of any activity is that the participants need to be specified. Since "a rule is a moral rule if it describes what rational agents ought to do or be, all things considered," "participants are therefore also specified: moral action guidance is addressed to rational agents, *from* rational agents" (p. 30).

Since this "participation rule does not limit the class of rational agents to human beings, nor does it necessarily include them all," Becker himself admits that "the boundaries of the concept of agency are somewhat vague (with respect to the status of young children, for example, and some animals)" (p. 31). Further reflection leads to the realization that this admission calls into question Becker's entire theoretical apparatus.

On the one hand, it is manifest—both from the philosophical tradition and from observation—that many animals satisfy the criteria of purposive action and self-consciousness that are used by Becker to define "rational agency." To cite the most obvious example, Jane Goodall and Frans de Waal have given us incontrovertible evidence of such putatively "human" traits in chimpanzees.

Unless the use of verbal speech is formally required, an a priori theory of rational agency cannot be limited to humans. If so, the web of reciprocity should, on Becker's own grounds, extend to other animals—with consequences that he does not seem to draw concerning the relationship of humans to individual animals and the ecological systems of which they are a part. If not, the theory cannot claim to be a priori, since it is based on the concrete characteristics of *human* behavior, combining as it

does feelings and emotions as well as verbal speech and social behaviors derived from millions of years of hominid evolution.

At the other extreme, of course, one could argue that "rational agency" only applies to those who reason "in the silence of the passions." Here, however, Becker must confront the opposite difficulty, perhaps best stated by Rousseau. In the *Second Discourse*, after describing the golden rule as "cette maxime sublime de justice raisonnée," Rousseau adds, "Quoi qu'il puisse, appartenir à Socrate, et aux Esprits de sa trempe, d'acquiescer de la vertu par raison, il y a longtemps que le Genre-humain ne seroit plus, si sa conservation n'eût dépendu que des raisonnemens de ceux qui le composent" (*Oeuvres Complètes*, vol. 3, Bibliothèque de la Pleiade no. 169 [Paris: Éditions Gallimard, 1964], pp. 156-57).

In short, either Becker's theory applies to many animals as well as to human infants or it is only applicable to "Socrates and minds of his calibre." Such an approach therefore proves either too much or too little, leading to the suspicion that purely a priori approaches to "virtue theory" are inferior to the observation and reflection characteristic of Aristotle. No longer does it seem reasonable to ignore the role of emotion and feeling in the phenomena known as morality; no longer can an account of human obligations to the family, to future generations, or to the law be based on abstract logic. It is time to admit that, like behaviorism, philosophic a priori-ism has been rendered obsolete by contemporary research in the life sciences. As Robert Axelrod has already shown, evolutionary biology is a better guide to human reciprocity than attempts to deduce obligations from the kind of "reasoned theorizing" (p. 31) used by many contemporary philosophers.

ROGER D. MASTERS

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Research in Political Sociology, Vol. 2. Edited by Richard G. and Margaret M. Braungart (Greenwich, CT: JAI, 1986. xviii, 332p. \$52.50).

This book is the second volume in an annual series of original contributions revolving around a dominant theme. The previous year's

issue dealt with the development of nation-states, whereas this 1986 issue considers a germane topic: effectiveness and legitimacy. Twelve articles of relatively equal length are grouped together under four headings: *political structure and change*, *cultural politics*, *youth politics*, and *social and political conflict*. Within these generous boundaries, each contribution remains autonomous and distinct in terms of the approach and topic chosen.

The series opens with what I would call a theoretical hors d'oeuvre. I cannot say whether it simply stands as an abstract endeavor meant to ennoble (so to speak) what is to follow or whether it is the outline of more elaborate future developments. In any case, Jonathan H. Turner and Charles H. Powers submit no less than twelve propositions under four captions. Borrowing from such classics as Marx, Durkheim, and Weber together with episodic imports from Simmel, Spencer, and Pareto, the authors manage to outline converging or complementary principles of political organization. Right from the start, Turner and Powers are aware that such an attempt may elicit more than reticent comments. Marx, Durkheim, and Weber do not always mix well. But despite incompatibilities deemed by them only apparent, the authors rely on common denominators in the name of a to-be-developed "natural science of society." The principles are not necessarily meant to be original and some of them verge on the obvious. The intention is mainly to bridge a gap between the classics and contemporaries.

The articles coming after this theoretical opening belong to different genres of writing; some may be considered full-fledged research articles, others tend towards the essay type, and some stand in between.

Robert J. Antonio expounds on the limits of Max Weber's model of authoritarian bureaucracy, which he considers too monolithic and fatalistic. In contrast, Antonio tries to demonstrate from the ancient Roman Empire—which had been Weber's point of reference—that instead of being destined for a state of permanence, any such regime is likely, by its own action, to generate countervailing pressures from the ruled and to have to submit to changes in the long run. The phenomenon of clientelism is put in evidence, underlining a feature common to both ancient and modern modes of authoritarian bureaucracy. The

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whole argument counts on the existence of an inherent dialectic in the relationship between rulers and ruled.

In the line of systematic research, Ronald Inglehart pursues his postmaterialist saga and furnishes up-to-date confirmations of a trend that should, I imagine, one day lead to this postmaterialist minority becoming the majority devoted to the promotion of the values of self-expression, a better environment, and social solidarity. Inglehart's mode of argumentation is sufficiently flexible to take into consideration the ups and downs of the postmaterialist trend, and therefore more likely to convince skeptical readers. The argument, as we know, revolves around the hypothesis that people reach levels of material satisfaction that drive them to strive for more sublime values thereafter. Just as Marx bound society to the imperative of material needs, this approach resorts to a psychological reading of human social development. In direct affiliation with Maslow, Inglehart expounds a psychological historicism that sets goals close to the ultimate values of the Western world. Scarcity and abundance are more or less given an absolute character.

From data collected in France in the late 1970s, David Swartz takes a stand against the "managerial-revolution" school and opts for a "class-organization" perspective. The study suggests that different classes in France control different types of organizations but that organizational constraints also require members of the managerial elite to conform to specific educational credentials. Social closure is therefore exercised through the recognition of a double standard of selection: economic and scholastic capital (using Bourdieu's terminology). In order to modify the frequent impression of an overwhelming technocratic elite topping both private and public spheres, Swartz establishes a dividing line between a public technocracy—which comes from professional family backgrounds and the highly selective channel of the *grande écoles*—and a still impressive part of the private sphere reserved for an elite based on family wealth.

On cultural matters Donald Granbert and Sören Holmberg, using a social-psychology approach, gauge the degree of subjective ideological congruence of individuals with the party they prefer. Comparing Swedish with U.S. citizens, the study reports a stronger

ideological correspondence between the Swedes and the political party of their choice than is the case for their U.S. counterparts. Such a disparity in ideological attachment would be largely due to the way the political competition takes place—the presidential contest, for instance, lending itself to personalized rather than ideological campaigns.

As to youth politics, Joyce M. Mushaben compares the "no future" generation of the 1980s in (West) Germany with the youth movements of that same country between the two world wars. The paper focuses on their respective contributions to German political culture while denying that the present condition is necessarily conducive to a Nazi form of regime. In opposition to Inglehart, Mushaben emphasizes the feeling of insecurity deriving from economic uncertainty, unemployment, environmental deterioration, nuclear policies, and the like. Claiming that today's German youth are excluded from the benefits of postmaterialism, the author wonders if the present policies of integration into society are adequate. Indeed, the essay is normative, at least in its conclusions. In contrast, there is the study by Vladimir Shlapentokh of attitudes and behavior of Soviet youth in the 1970s and 1980s. Seemingly well documented (I am not a specialist in the field), the paper comes close to the type of good survey a reader might read in *The Economist*, including, of course, the same underlying message.

The book concludes with monographs on social and political conflicts. After a disproportionately long historical background to the "process of class struggle" in Hawaii, J. A. Geschwender and R. F. Levine sum up in a few pages how, after World War II, the Democratic party was prevented by capital's "red-baiting" campaign from becoming a prolabor party. Next are Carol L. Schmid's considerations on Quebec. Having posited that to exist as a submerged nation, a group has to conform to a number of conditions regarding its identity and political will, the author arrives at the foregone conclusion that Quebec only partially fulfills these requisites. The book closes on E. Walsh and S. Cable's case study of movements engendered by the Three Mile Island accident. The authors discuss the complexity involved in any analysis of social-movement litigation.

The idea of an annual review of this kind on

research in political sociology is excellent, even though the treatment of themes still opens the way to a dispersion of topics.

ANDRÉ J. BELANGER

University of Montreal

Politics, Self, and Society: A Theme and Variations. By Heinz Eulau (Cambridge: Harvard University Press, 1986. x, 567p. \$39.95).

This is a splendid book. It brings together 20 of Heinz Eulau's principal papers—the earliest from 1958, 10 from the sixties, 8 more from the seventies, and 1 published here for the first time. As such, it is a most valuable reference. Harvard University Press has done the profession a service by assembling these scattered papers.

The theme of the subtitle is that "self and society are inextricably intertwined in political life" (p. 2). Working with an understanding that "it is the function of science to understand and interpret the world, not to change it" (p. 22), Eulau's analysis of his theme has been guided by explicit epistemological considerations. His principal concern has been with the problem of simultaneity. This results from a larger unit and its smaller subunits being simultaneously involved in any political action. Neither the larger unit nor the smaller units are ever free of each other. The larger unit—whether a group, organization, or nation—does not have any independent status apart from the conduct of individuals who comprise it. But in the real world of politics, it is the collectivity and not its individual members that is the effective decision maker.

In order to collect data and analyze the simultaneously acting units, Eulau distinguishes between the subject unit—by which he means the observational unit, the unit whose behavior is observed—and the object unit—the *explicandum*, the unit whose behavior is to be explained. This distinction is crucial because proper analysis requires bringing all of the object unit's properties to the same level. This may involve either reduction (in which case properties on a more inclusive level are treated as contextual) or construction (with properties on a less inclusive level being treated as dis-

tributive—or relational or structural—properties of the object unit).

"Unless the properties of collective actors are obtained through reduction and construction," Eulau rightly warns, "analysis of large units will inevitably remain literary, discursive, and speculative" (p. 95). But if there are perils in not adopting this research strategy, there are substantial rewards for doing so. "Macrolevel construction can go a long way in making microanalytic, psychology-based models of decision-making behavior relevant to political institutions and processes" (p. 450). Just as self and society are inextricably intertwined, so is analysis of behavior and institutions. Political behavior is not a separate field of investigation but an approach to the full gamut of political phenomena.

An ability to move easily across levels of analysis—to transform data gathered on one level for proper use on another—also confers an ease in movement along the micro-macro continuum. As one moves toward the micro end, empirical data are added that make for greater validity and reliability. As one moves toward the macro end, more significant questions can be addressed. But it is not necessary, Eulau says, to give up validity and reliability in order to achieve significance. An understanding of construction and reduction of data allows an analyst to have validity *and* significance on whatever level. Many arguments in political science result from a failure to grasp this crucial point.

Both of Eulau's most extensive research projects have been informed by a levels-of-analysis perspective. In the four-state legislative study (chap. 6) the data were gathered on the individual level and much of the analysis was also on that level. But the legislature was conceptualized on the institutional level as a determinate role structure that "linked the legislature as a subsystem with other [political] subsystems" and "institutionalized and resolved the social, economic, and political conflicts" (p. 203) with which the legislature had to deal. The levels-of-analysis perspective was even more explicit in the Bay Area study of 82 city councils (chaps. 10, 11, 14, and 15). Here again, data were gathered on the individual level, but the analysis was all on the group level, dealing with integral properties (such as the size of council), relational properties (such as cosponsorship, oppositional activ-

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ity, affect, respect, and expertise), structural properties (such as bipolar, unipolar, and non-polar councils) and contextual properties (such as city size).

In *Politics, Self, and Society* Eulau also expounds configurative analysis and developmental analysis. A configuration is an emergent whole. "Emergent wholes such as 'personality,' 'society,' or 'culture' are units that are something more than the sum of the items, elements, or traits that constitute them individually" (p. 249). The task of the analyst, then, is less one of tracing cause and effect among the elements than of discovering the particular configuration that emerges from the combination of elements. Thus Eulau is interested, for example, in the response style that results from setting a city council in a particular political matrix. And in his most recent analysis, Eulau traces life space and social networks, discovering differences between day dwellers and night dwellers in their use of neighborhoods.

Developmental analysis is based on Lasswell's distinction between goal thinking (analysis and selection of values toward which decisions are directed), trend thinking (analysis of past tendencies and future probabilities), and scientific thinking (analysis of limiting conditions). Therefore, says Eulau, "the methodological problem is nothing less than to connect statements of value or preference, statements of fact, and statements of expectation" (p. 228). *Politics, Self, and Society* includes three developmental analyses: a discussion of policy maps summarizing council members' perceptions of their city's stage of policy development; a quasi-longitudinal, quasi-experimental design for comparative policy analysis; and one of the very few re-analyses of the Miller-Stokes data.

"The behavioral persuasion in politics," Eulau wrote in the early sixties, "has more than one approach, and there are many voices that speak in its name. But they all have in common a commitment to the study of man as the root of things political or, to put it more technically, to the individual person as the empirical unit of analysis" (p. 22). *Politics, Self, and Society* contains the fruit of his personal efforts. There is a little irony to its publication in the late eighties. We are now hearing about a new institutionalism, but if political scientists had paid closer attention to what Eulau was

saying, we would already have a richer institutional analysis. For if Heinz Eulau's subject has been man, his object has always been politics.

JOHN H. KESSEL

Ohio State University

Rational Association. By Fred M. Frohock (Syracuse, NY: Syracuse University Press, 1987. xi, 201p. \$27.50).

New dimensions of the debates over liberalism and its fate are difficult both to isolate and to advance interestingly. This slim volume does both. Frohock introduces the consistency problems of liberalism by assessing them through the lens of collective-choice theory, which simplifies many of the dilemmas confronting formal understanding of democratic "rationality" as treated in the literature. By attending to the nature of rationality throughout the book, Frohock is able to clarify the problems and introduce possible solutions that could revitalize what many think of as the dying theory of liberalism. His primary question deals with the puzzles surrounding the inconsistency "between individual rationality and collective outcomes" (p. ix). His goal is to avoid the discontinuity between *one* and *all* that plagues rational-choice theory without making the major countererror of adopting a holistic solution.

The problem, embedded in treating utility-based rationality as the ground for quantitatively reaching collective action, is the move from the individual to community (set forth in chap. 1). The analysis then proceeds, through assessment of individuals and structures as set forth in the prisoner's dilemma, to establish the importance of isolation of individuals as a background assumption of the puzzles (chap. 2), especially in a concluding contrast with Aristotle's corporate model—giving a hint about where we eventually will end. Frohock continues his arguments about the tensions arising from Arrow's theorem and exchange theory to conclude (chap. 3) that "there is no mediating device to transform the disjointed features of liberalism into the type of moral community liberalism seeks" (p. 67). His assessments of individuals and groups lead him to conclude his criticisms of liberal theory's linkages with the observation that different

liberal theorists (Hobbes, Locke, and Rousseau) had different approaches to the dilemmas (clarified by his analysis of formal theorists), with Rousseau's general-will solution—adopting community will and morality as one's own—providing the “exit routes” for these dilemmas (chap. 4, p. 98).

Frohock's own skillful and easily followed route through formal theory is continued in his assessments of rationality and reasoning (chap. 5), and rational forms (chap. 6). The path, clear enough for undergraduate use even though it advances the literature, provides a framework for pursuing careful assessment of the options for treating the fit between collective-choice theory and moral decisions, for moving from the rational individual to appropriate grounds for collective commitments on specific actions. Perhaps the single most powerful dimension of the book is that it makes clear to both proponents and opponents of rational-choice analysis just what is missing in the effort to deal with liberal and democratic theories from the choice perspective. Traditional theorists troubled by rational analysis will better grasp what is missing in the formal approaches; formal theorists will see what conditions are not resolved within their theories. Defining *reasoning* as deliberation (dealing within the realm of uncertainties)—as isolation of things that count as good reasons—rather than counting reasoners under a democratic motif permits one to move the discussion out of the constraints of formal choice of individuals (which leads to inconsistencies in collectivizing the will) into individual choices within the discourse that provides reasons for collective action that can be shared: “The task of political inquiry, once individuals are accepted explicitly as reasoning agents, is to recognize the juridical forms that are compatible with individual minds. . . . Individuals who are rational on reasons must rely on rules that govern; and the unity of minds can be located in the rules by means of which self-legislation occurs” (p. 159).

In his final chapter (chap. 7), Frohock carefully focuses on the forms of background assumptions that affect collective-choice applications to liberal values: “These three types of connections between individuals and collectives—decisiveness, fairness, and hypothetical generalization argument—introduce collective considerations to individual choice” (p. 169).

The fairness option, however, is different in that it must provide a reason for cooperating. Fairness as a necessary but not a sufficient condition, for a moral association relies upon the giving of reasons: “A reason-giving form of rationality is the substance on which such an association is formed. Reasons, as we know by now, (a) are formed on a network of rules that can organize communities authoritatively, (b) define individuals holistically as members of rational classes, and (c) provide evaluative criteria to address those issues of fairness in the absence of which cooperation has no meaning” (p. 171).

Frohock provides convincing analysis that confirms the notion that reasons for choice within a collective are the key to rational association. Reasons, not just preferences, permit the establishment of rules and morality within the collective (p. 179). Deliberation, giving reasons and counterreasons while the collective works towards standards that can be shared, is an operation of reason that permits one to join a search for common interest in ways impossible under formal choice theory based on isolated preferences. In his concluding advocacy of “juridical democracy” (p. 181), Frohock has carried us back through *reasons* for returning to Aristotle's analysis from within formal theory's own logic—an important journey.

GEORGE J. GRAHAM, JR.

Vanderbilt University

Hermeneutics and the Sociology of Knowledge. By Susan J. Hekman (Notre Dame, IN: University of Notre Dame, 1986. 224p. \$29.95).

Susan J. Hekman's *Hermeneutics and the Sociology of Knowledge* has two main goals: first, to point out the similarities between Hans-Georg Gadamer's hermeneutics and Karl Mannheim's sociology of knowledge and second, to indicate the implications of these similarities for social-scientific method. In her view, Mannheim and Gadamer share three notable insights. First, both emphasize interpretation. While Mannheim is confused on this point he nevertheless advocates a “hermeneutic method” according to which historical phenomena are to be investigated “in

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terms of the understandings and presuppositions of the age in which they are manifest" (pp. 69-70). For its part, Gadamer's *Truth and Method* attempts to show that interpretive understanding is fundamental to all aspects of human life. Second, both emphasize the way in which interpretation is conditioned by the situation of the interpreter and thus suggest the way in which Enlightenment conceptions of truth and objectivity need to be modified. Hekman contends that Mannheim is, again, somewhat confused on this point, calling his views phenomenological positivism. Still, like Gadamer, he shows that there is no ahistorical, unsituated Archimedean point from which to acquire an objective explanation or the truth of human beings or society. Rather, the worth of any interpretation is relative to the circumstances from which it is advanced and to the questions it tries to answer. For this reason, both Mannheim and Gadamer stress the importance of what Hekman calls *self-reflexivity*: interpreters must be aware that their perspectives are "prejudiced" (in Gadamer's words) and hence must be willing to subject them to critical examination. Finally, both Mannheim and Gadamer conceive of interpretive perspectives in "collective" rather than individual terms. In a passage that Gadamer might have written, Mannheim therefore asserts, "Strictly speaking, it is incorrect to say that the single individual thinks. Rather it is more correct to insist that he participates in thinking further what other men have thought before him" (p. 80).

What Hekman sees as the chief difference between Mannheim and Gadamer might be expressed in similar terms: namely, that Gadamer simply extends Mannheim's thoughts in a more radical and consistent manner. Not only is he clearer on the role of interpretation and its implications for Enlightenment views of truth and objectivity, he also clearly "reverses Enlightenment thought on the relationship between the natural and the social sciences" (p. 107). Neither theorist thinks that the methods of the natural sciences can provide any model for those of the social sciences. But Gadamer goes further, suggesting that insofar as the natural sciences disregard the conditions of prejudice, they are themselves flawed. As Hekman continues, "the model of understanding characteristic of the human sciences is the universal process by which we, as human

beings, attain knowledge; that characteristic of the natural sciences, on the other hand, seriously distorts this process, hence calling into question the legitimacy of this mode of understanding" (p. 107). From this analysis, Hekman concludes that Gadamer's hermeneutics provides an important "anti-foundational" basis for the social sciences. Rather than universalizing notions of truth and objectivity developed for the natural sciences and seriously distorted in themselves, a hermeneutic social science focuses on the relations between "human thought and human existence" (p. 11).

As far as I know, Gadamer never charges natural-scientific method with internal flaws. His claim is rather that the mode of experience proper to the natural sciences is *not* proper to other fields of human inquiry and that their project is one rooted in the concerns and commitments of a particular community. Hekman's failure to clarify the point she is making here is indicative of similar failures throughout her book. She covers a great deal of ground, makes numerous distinctions and comparisons and contends with much commentary on, and criticism of, the texts with which she deals. But she does much of this far too quickly and therefore leaves significant gaps in her argument. In the remainder of this review I shall briefly consider two.

First, despite her intention, Hekman fails to spell out the implications of Gadamer's work for social-scientific method. She thinks that Gadamer goes beyond the Wittgensteinians in overcoming Enlightenment-type distinctions between natural and social science; for, although the latter emphasize interpretation, they continue to identify the natural sciences with the search for truth and simply exclude the social sciences from it. Gadamer, however, denies that either attains truth. I think there are misconceptions in this account. Of concern at this point, however, is the relatively superficial character of Hekman's exploration of the issues. If both natural and social science employ interpretations, if both are historically situated, is this all that needs to be said? What of that which Anthony Giddens has called the "double hermeneutic" in the social sciences? If Gadamer thinks that neither natural science nor social science attains "truth," what does he mean by talking about truth and denying the exclusive access of method to it?

This last question is related to another dif-

ficulty in Hekman's argument. Throughout her book she denies that Gadamer has a problem with truth because he defines it in terms of the collective conceptions or shared vocabulary of a community. Indeed, she claims that this is what separates Gadamer from the nihilism of Rorty, Foucault, and Derrida: we can make moral judgments and can distinguish between good and bad interpretations in general because we belong to historical traditions to which we can appeal for our normative standards. But what if our traditions are Fascist, racist, sexist or the like? It is on precisely this point that much of the criticism of Gadamer's work has focused. The question is how we examine our shared understandings or reflect on our prejudices. Hekman touches on this question at two points: first, where she discusses the self-reflexivity she finds in both Gadamer and Mannheim and second, in her discussion of Habermas's critique of Gadamer. In the first instance, however, she merely notes that both theorists demand self-reflection without investigating the conditions of its possibility. The problem here is that if all interpretation is prejudiced then so are the interpretations in which we reflect upon the prejudiced character of our interpretations. Hekman herself makes this point in discussing Gadamer's antifoundationalism. Still, if he is antifoundationalist in this regard, what are the consequences for his demand for self-reflection? If our prejudices are racist, will the vocabulary in which we reflect on them not also be racist? Habermas argues that "objectifying" methods need to be brought into the social sciences at precisely this point to allow distance from our own preconceptions. Hekman dismisses this solution, but in doing so she apparently overlooks the difficulty with her own assessment of Gadamer's work.

The attempt to synthesize Mannheim and Gadamer to provide the social sciences with "both a solid philosophical foundation and a viable research program" (p. 11) is interesting and productive. One wishes that Hekman had had time to explore the issues involved in more depth.

GEORGIA WORNKE

Radcliffe College

The Political Ideas of Marx and Engels, Vol. 2, Classical Marxism, 1850-1985. By Richard N. Hunt (Pittsburgh: Pittsburgh University Press, 1984. xiii, 421p. \$37.95).

This is the successor volume to Hunt's well-received study of the political ideas of the young Marx and Engels, and a fitting memorial, given his untimely death, to its author. In his first volume Hunt assumed that the notion of the "parasite state" hovering above civil society constituted the core of Marx and Engels's early political thought. Here he traces in particular the development of a notion of "democracy without professionals," in which large-scale participation and the frequent election of all important officials were to prevent the growth of any permanent, alien political and bureaucratic force. This, Hunt presumes, was the chief achievement of Marx and Engels's later political thinking, the source for their adoption of the model of the Paris Commune, and the main distinction between their work and more authoritarian forms of twentieth century Marxism. Like his chief competitor, Hal Draper (*Karl Marx's Theory of Revolution* [New York: Monthly Review Press, 1977]), Hunt denies that anything like "totalitarian democracy" (in Jacob Talmon's phrase) was the natural outcome of Marx and Engels's critique of the liberal state. Analytically, Marx and Engels's notions of both the parasitic, or independent, state and the class, or instrumental, state developed in the 1850s through their observations of Bonapartism and studies of "Oriental despotism," the latter being gradually seen as the first instance of the parasitic state. After detailing these anthropological explorations, Hunt treats the development of both state theories up to the 1870s and the Paris Commune, to which two chapters are devoted. After a useful excursus (which will fuel the debate on Marxism and morality) into the problem of the degree to which Marx and Engels recognized individual rights and the question of the tyranny of the majority, Hunt examines the notion of classless society and the disappearance of the state, arguing that both have been misconstrued as completely utopian. His final chapters then detail Marx's debate with the anarchists in the International and the debates after 1870 over the possibility of skipping historical stages to reach socialism (notably relevant for Russia)

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and over the strategy of a legal, peaceful road to socialism in democratic countries. Hunt's is a careful, largely textual and very scholarly study. Abjuring ideology and polemic, he nonetheless rejects many well-known interpretations of areas of the topic (e.g., Wittfogel on Oriental despotism). His aim is not to expose Marx and Engels's weaknesses or heap the sins of their disciples upon them, but inevitably some questions of this type must be faced. Hunt indicates, for example, Marx's failure to foresee the expanded role of managers and bureaucrats in the future (cf. pp. 252-63), but neglects to ascertain why this (obviously serious) weakness in the theory persisted (more attention to Saint-Simon and to the problem of economic administration would have been helpful). Similarly he steers clear of some of the more unpleasant innuendoes associated with "proletarian dictatorship" (better handled by Draper). Nonetheless, this is an excellent study with great strengths, and its subtlety and toleration of the ambiguity of the sources help to raise the level of scholarship in this area considerably.

GREGORY CLAEYS

Washington University

James Madison: Creating the American Constitution. By Neal Riemer (Washington: Congressional Quarterly, 1986. xix, 203p. \$11.95, paper).

This work, though not labeled as such, is apparently a revised edition of the author's earlier (1968) treatment of James Madison's political thought. Its seven chapters focus on concerns that stem from the question Riemer poses in the first sentence of the book: "Is republican government in a large state possible?" (p. 1). These chapters explore four difficulties—"disunion, large size, faction," and the "anti-republican danger"—that Madison confronted in his efforts to "reconcile liberty and large size in the infant American republic" (p. 132). As such, the subtitle is a bit misleading because these concerns take us well beyond Madison's efforts toward a stronger national government or his contributions to the Philadelphia Convention and the ratification struggle. Indeed, as Riemer makes clear at the out-

set, he is concerned with Madison's "evolving thought" throughout "his entire life," particularly his "maturing political conceptions," such as his "bold assertion of civil liberties, the organization of a republican political party, and a strategy of constitutional interpretation . . . that would safeguard union and freedom against anti-republican attack" (p. 3).

A goodly portion of Riemer's treatment is devoted to accounting for Madison's changes of heart regarding the nature of the union and the relative powers of the national government vis-à-vis the states. According to Riemer, Madison's strong nationalism, evident at the convention and in his early political thinking and activities, was modified as he came to realize that ratification of the Constitution required "a more moderate nationalistic interpretation" (p. 89). In Riemer's view, his modification on this and other matters, such as political parties, clearly manifest Madison's prudence—"a practical wisdom," "a quality of mind" concerned with "wisely relating means to ends" (p. 13). Thus, as Riemer would have it, many of Madison's presumed inconsistencies can be attributed to his "consistent prudential approach to politics" which rendered it difficult, if not impossible, "to maintain a superficial consistency" (p. 16). And where, in retrospect, many find Madison to be imprudent, to have taken extreme positions—for instance, his constitutional opposition to the national bank or the underlying theory of his Virginia Resolutions, drafted in response to the Alien and Sedition Acts—Riemer holds that we encounter another basic element of Madison's philosophy: "When the safety and happiness of the people were at stake, Madison's usually high regard for existing legal forms would give way to his insistence upon placing the substance of republican liberty and union ahead of accepted but malfunctioning constitutional or legal forms" (p. 136).

Riemer also explores Madison's extended-republic theory at some length. He notes certain difficulties with this theory that are of more than passing interest to the student of U.S. politics today. For instance, Madison seemed to believe that there is an objective "public interest and that it can be identified," a belief that, Riemer surmises, "is ultimately grounded in his natural law orientation" (p. 124). Yet his failure to define this central concept means, as Riemer remarks, that his theory

"may not be scientifically operational in modern terms" (p. 125). And in this vein, as Riemer notes, his theory is concerned with preventing factious rule—which "limits [its] relevance . . . for those who favor a broad exercise of national power in what they hold to be the national interest" (p. 118).

While noting that Madison "was by no means a systematic, comprehensive, sophisticated student of political theory," Riemer concludes by pointing out the ways in which his thought provides "the germ of a durable, penetrating, and persuasive democratic theory for man today" (p. 158). Though his view of human nature prevented him from even contemplating a heaven on earth, Madison did repose sufficient faith in man to take the "calculated risks" (p. 164) involved in establishing an extensive republican regime under the forms of the Constitution.

GEORGE W. CAREY

Georgetown University

Self and Society: A Study in Gandhian Thought. By Ramashray Roy (Beverly Hills: Sage, 1985 [in collaboration with United Nations University, Tokyo]. 205p. \$25.00).

The author maintains that the predicaments of Western modernity can be overcome through the Gandhian conception of the self and of the relationship of the self to society and nature. According to him, the pathology of modern civilization has its origin in the seventeenth century shift in European thought—and practice—from the conception of man as being an integral part of a larger order to the concept of man as an autonomous, self-defining, self-sufficient and possessive-individualistic ego. In Roy's view, such Western attempts to repair this "broken totality" as expressivism, Hegelianism, Marxism and romanticism do not lead us out of the "satanic civilization," which remains committed to "the multiplication of wants and machinery contrived to supply them." What is required, according to Gandhi, we are told, is not mere "external" or structural changes in social and political organization but the self-transformation, or *swaraj*, of human beings, leading to the realization of "the essential unity of God

and man and for that matter of all that lives." Against the "broken totality" of modern Western civilization, Roy advances Gandhi's peculiarly Hindu philosophy of life, according to which, as noted by Gandhi, "all life (not only human beings, but all sentient beings) is one" (p. 73). Salvation, accordingly, is not to be confined to human beings; it has to extend to *all* God's creatures. This Hindu philosophy of life, Gandhi believed, excludes all exploitation. It defines salvation as the realization, by the self, of Truth, which means "not only truthfulness in word, but truthfulness in thought also, and not only the relative truth of our conception, but the absolute Truth, the Eternal Principle, that is God" (p. 71). This Hindu conception of the integral unity of all life, as interpreted by Gandhi, implies that the process of self-realization, that is, Truth realization, can only be achieved through the moral-political actions of *satyagraha* and *sarvodaya*. *Satyagraha*, by rejoining politics and religion, we are reminded, repairs the broken totality of modern times.

Roy succeeds in bringing out the important contrasts between the modern Western paradigm of politics and the Gandhian conception of the self, society, and nature. The treatment of the distinctive philosophical foundations of those two conceptions or models of social and political life is the chief merit of this book. It suffers, however, from the limitations of the author's self-acknowledged training as a behavioralist. The book could have gained from a greater use of the available literature on Indian philosophy and on Gandhian thought. While it is indeed true that Gandhian thought has its roots in Hindu philosophy, it has also to be recognized that the former was influenced considerably by such strands of non-Hindu thought as Tolstoy, Ruskin, Thoreau, the Bible, and British jurisprudence.

While I would still regard the book as giving important and useful insights into Gandhi's philosophical undermining of the technological or scientific model of modern Western politics, I am somewhat disappointed by the author's inadequate treatment of Gandhi's *satyagraha* mode of liberative political action. How *satyagraha* constitutes a realistic and effective mode of emancipatory action against the technological model of politics remains to be demonstrated. From the author's treatment of the subject, I am left to wonder if Gandhi's

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alternative philosophy of the self and society is simply an utopian blueprint. Also left unresolved is the question of whether or not the Gandhian satyagraha is merely a voluntarist conception of social change.

It seems to me that for a proper understanding of the distinctiveness and relevance of Gandhi's philosophy and method of liberative action by those who suffer from the predicaments of "modern industrial civilization," it is necessary to compare and contrast the Gandhian "project" with both liberalism and Marxism. This has not been the specific objective of Ramashray Roy, but his book will be of great help to anyone undertaking such a larger study.

THOMAS PANTHAM

Maharaja Sayajirao University, Baroda

The Evolution of Rights in Liberal Theory.

By Ian Shapiro (Cambridge: Cambridge University Press, 1986. x, 326p. \$39.50, cloth; \$11.95, paper).

Ian Shapiro's book is both more and less ambitious than its title suggests. It is more ambitious because Professor Shapiro's concern is not only to trace the evolution of liberal theories of rights, but to reveal the ideological force of such theories. As he says in the concluding chapter, "My aim has been to show . . . how conventional liberal values have come to function, and continue to function, conservatively in today's world" (p. 304). As an account of the *evolution* of right in liberal theory, however, it is less ambitious than the title suggests, for Professor Shapiro concentrates his attention on only four "moments" in the development of liberalism, "moments" represented by Thomas Hobbes, John Locke, Robert Nozick, and John Rawls.

Indeed, the author proceeds by fours in this book. In the first chapter he states that a "claim about rights generally involves a four-fold assertion about the *subject* of entitlement, the *substance* of entitlement, the *basis* for entitlement, and the *purpose* of entitlement" (p. 14, Shapiro's emphasis). In the succeeding chapters on the four "moments," then, Professor Shapiro analyzes the theories of Hobbes, Locke, Nozick, and Rawls in terms of their conceptions of the subject, the substance,

the basis, and the purpose of rights. In each case, he follows this analysis with an account of the implications—intended or not—of the philosopher's theory for the development of liberalism as an ideology.

These accounts of ideological implications are to my mind the most interesting parts of a most interesting book. According to Shapiro, for instance, Hobbes's political absolutism was due largely to the fact that he was caught in the "transitional moment" between feudalism and capitalism, a time when the economic and social practices necessary to the survival of the "negative-libertarian" society Hobbes really favored were neither fully nor firmly established. In these circumstances, Shapiro argues, Hobbes had to insist on the need for a "decisive authoritarian power" to enforce "the rules and norms essential for social action" (p. 66). Later, however, when the necessary practices and institutions had come to seem part of the natural order of things, the implications of Hobbes's arguments pointed away from absolutism and toward liberalism. Thus Hobbes's identification of "natural man" with "private man" led him to conclude that the purpose of a public sphere "was merely to facilitate private interaction. This is a move of immense ideological significance because it provides for the germination of the liberal notion that the state . . . is fundamentally undesirable, an illegitimate intruder except where its actions facilitate private interaction" (p. 60).

With regard to Locke, the representative of "the classical moment" of liberalism, intentions and implications again point in somewhat different directions. In arguing for toleration and against conformity in religious matters, for instance, Locke conceived of rights themselves "primarily in terms of toleration, of the spheres of people's lives in which they may be left alone" (p. 117). Such a conception leaves little room for a "doctrine of democratic participation," Shapiro says, and once religious toleration and conformity were no longer at the center of controversy, "the negative libertarian view of freedom Locke embraced had vastly different and more conservative ideological implications" (p. 117).

Both Hobbes and Locke, then, contributed by implication to the "negative-libertarian" view of rights and society—a view shared, the author says, by Nozick and Rawls, represen-

tatives respectively of the "neoclassical" and "Keynesian" moments. The chief difference between the two on Shapiro's analysis is that Nozick advocates the "minimal state" because he clings to the equilibrium theory of the self-regulating market while Rawls, who follows Keynes in rejecting this theory, advocates "the liberal corporatist state" (pp. 262-64). This is the only significant difference between them, though, and in the end we find that they join Hobbes and Locke in regarding "the purpose of political organization generally, and the civil rights constituted by it in particular, as being to preserve a negative libertarian society in which individuals can pursue their different individual goals unimpeded" (p. 283). If one is tempted to say that a political organization ought to serve this purpose, Shapiro is ready to point out that such a vision of politics and rights denigrates political participation and the public sphere, fails to foster a sense of community, and succeeds all too well in legitimizing the dominant interests in capitalist societies.

Professor Shapiro is not the first to bring these charges against liberalism, of course, and perhaps that is why he does not pursue them here. He sees his task, instead, as "historical anthropology" (p. 303), with the particular aim of providing "an evolutionary anthropology of establishment values" (p. 8). Does he succeed in this endeavor? He does if one is willing to count well-informed, closely argued, and provocative readings of Hobbes, Locke, Nozick, and Rawls as the full measure of success. Certainly it is a triumph of sorts for Shapiro to find something fresh and interesting to say about each of these thoroughly scrutinized philosophers. If one takes his notion of an "evolutionary anthropology" seriously, however, then one must conclude that there are some links missing from *The Evolution of Rights in Liberal Theory*.

This is most evident when one considers that Shapiro devotes his attention to two English philosophers of the seventeenth, and two U.S. philosophers of the twentieth, century. Between his pairs of subjects stretches more than 250 years of ideological evolution of which we are told almost nothing. To be sure, Shapiro does refer several times to the importance of the rise of the concept of equilibrium in the eighteenth century, and Bentham, Mill, and Sidgwick do appear briefly, but there is no

attempt to trace shifting conceptions of rights or developments in liberalism through the eighteenth and nineteenth centuries. Nor is there any attempt to explain why so many influential philosophers of the nineteenth century, both within and without the Anglo-American tradition, were suspicious of, or even hostile to, the natural-rights theories associated with Hobbes and Locke. A second kind of missing link is more a matter of method than content. Through the use of such evolutionary metaphors as "lethal mutations" (p. 5) Professor Shapiro suggests that the fate of a theory rests in large part on its ability to adapt—or, more precisely, to prove adaptable—to a changing environment. Hobbes did not intend to justify the capitalist market, for instance, but his "negative libertarianism survived and achieved the preeminent status it did in the dominant ideology because of its affinity with these emerging economic and social relations" (p. 63). Clearly there is some relationship between ideas and institutions here (or *super-structure* and *structure* in another idiom) but what is it? Shapiro rejects any sort of simple deterministic explanation of the relationship, certainly, but aside from invoking the evolutionary metaphor, there is no clue as to what he would put in its place.

There is only so much that one can do in a book, however, and I am inclined to think that Ian Shapiro has done quite a lot in this one. There is much to admire in *The Evolution of Rights in Liberal Theory*, and much to argue with, too, which is one of the reasons for admiring it. Professor Shapiro writes clearly, argues vigorously, and, for an author intent on unmasking the ideological functions of rights theories, remains remarkably free himself of ideological dogma. If he does not tell us all we might wish to know in this book, then we may well wish that he will tell us more in another.

RICHARD DAGGER

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Book Reviews: Political Theory

The Longing for Total Revolution: Philosophic Sources of Social Discontent from Rousseau to Marx and Nietzsche. By Bernard Yack (Princeton: Princeton University Press, 1986. xvii, 390p. \$35.00).

Bernard Yack has both historical and critical projects in this book. The historical project locates the heritage of German idealism within revolutionary thought from Schiller and the Left Hegelians through Marx and Nietzsche. These thinkers are often viewed as inheriting and secularizing Christian ideals. Yack argues, however, that their ideals are distinctively modern, reflecting notions of human dignity and freedom introduced by Rousseau and philosophically expressed by Kant. According to Kant, human dignity stems from capacities for freely determined rational judgments, that is, judgments not determined by natural contingencies. Yack is especially concerned with the social conclusions various thinkers drew from this polarity of freedom and determination. Kant and Hegel saw in it a more or less inevitable conflict between inner freedom and social contingency. In contrast, their leftist successors held that freedom should and could be realized within social relations. To the extent that social relations do not express freedom, they are dehumanizing and ought to be transformed.

Yack's historical treatment of this theme is wide ranging, scholarly, fluid, and often insightful. His view that German revolutionary thought can be seen as providing answers to "left Kantian" questions is an extremely useful context of interpretation. The book's critical project, however, will prove controversial. Yack's targets are Marx and Nietzsche, who, he argues, both fail to understand that extending Kant's notion of freedom to social relations is self-contradictory. For Kant, freedom is an inner, "noumenal" quality. Social existence is "phenomenal," and hence subject to causal contingencies. By demanding that human dignity qua freedom be realized within economic and cultural relations, Marx and Nietzsche misunderstand the possibility of extending Kantian conceptions of freedom to the realm of natural contingencies. Because they retain Kant's conception of human dignity while rejecting its dualism, they fall into self-contradiction. This in turn reflects Marx's and Nietzsche's failure to understand that

social contingency will always exist, and that therefore "total revolution" will always fail. Hegel, on the other hand, recognized these limits to freedom, and was therefore able to conceive of a society in which inner freedom and external social contingencies might co-exist. Thus Yack's philosophic aim is to show that a Hegelian critique of Marx and Nietzsche is more plausible than their own critiques of Hegel. His political aim is to show that revolutionary demands are misplaced, the product of wishful thinking based on self-contradictory premises.

Perhaps because Yack presents his critical thesis in genetic terms—through a history of ideas—it lacks the philosophic argument one might wish, especially given its sweeping qualities. Key arguments are often presented as if they were self-evident. For example, the most important claim for Yack's thesis is that Kant understood that inner freedom could not be expressed in social relations (Chap. 3). Yack asserts that the "left Kantian" demand that freedom be realized in social relations rests on a very obvious category error: the noumenal realm of freedom is incommensurable with the phenomenal world of social relations. But Yack surely misses the form of Kant's questioning. Kant assumed that moral and rational experiences exist and that they already have an imperfect reality in the social world. His critical thought had to do with identifying what we must presuppose (such as our freedom) for our moral experiences to be intelligible. The duality is an epistemological, not a cosmological, one. This makes a difference for political conclusions: although, as Yack points out, Kant was no revolutionary, he did see reforms toward legal, parliamentary regimes as conditions of extending and universalizing moral experience (see *The Metaphysics of Morals*). He even had a theory of the transformation process (see his *Idea for a Universal History*). Without the benefit of argument, it is difficult to see how Yack can sever Kant's notion of inner freedom so cleanly from social relations. Yet this assumption is what allows him to take for granted that left-Kantian demands for realizing freedom in social relations are misplaced.

A second limitation is that too much of the argument is carried by rhetorical attributions of totalistic goals. Of Marx and Nietzsche, for example, Yack writes that "by providing a

single focus for all our dissatisfaction with the world, the longing for total revolution raises great hopes for the transfiguration, however defined, of the human condition" (p. 368). There is certainly plenty in Marx and Nietzsche to support Yack's many statements to this effect. But there is also much more. Because he carries out his own analysis in such sweeping terms, one feels Yack has missed the often finely textured and subtle ways Marx and Nietzsche deal with the problems he so often acutely identifies.

Finally, Yack's project itself is likely to be controversial. By focusing on "philosophical sources of social discontent," he in effect brackets what many will see as equally important issues, namely, whether Marx, Nietzsche, and others provide new insights into rapidly changing institutions. Yack is certainly correct in arguing that demands for change can result from new ideas about what is desirable and about what can and cannot be changed. And he claims that his type of analysis does not pre-

clude others (e.g., p. 254). Nonetheless, Yack's critical conclusions are hard to separate from his approach. Because he investigates only conceptual sources of discontent—in isolation from both practices and institutional contexts—he adopts a perspective in which the individual is simply a posited internal conceptual state, whose qualities are explained only by inherited ideas. Likewise, institutions appear as undifferentiated, external "obstacles" to individual demands. Yack continually writes of institutions only in the abstract, as if they had the same status as any natural contingency in terms of their independence of human will. In spite of Yack's protestations to the contrary (p. 368–69), one cannot stay within his project and avoid the neoconservative conclusion that the best solutions to social problems are lowered expectations.

MARK WARREN

Northwestern University

AMERICAN POLITICS

Justice Downwind: America's Atomic Testing Program in the 1950s. By Howard Ball (New York: Oxford University Press, 1986. xviii, 280p. \$21.95).

The Whale and the Reactor: A Search for Limits in an Age of High Technology. By Langdon Winner (Chicago: University of Chicago Press, 1986. xiv, 200p. \$17.50).

The study of the social, economic, and political impacts of technology upon human society and its institutions has attracted the attention of numerous scholars in recent decades. Faith in the utopian conviction that technological innovation would lead to previously unimaginable improvements in the quality of life has been replaced by skepticism and ambivalence. The two volumes reviewed here use very different approaches to explore the unintended effects of new technologies.

Howard Ball's *Justice Downwind* is an absorbing case study of the aftermath of atomic-weapons tests conducted by the U.S.

Atomic Energy Commission (AEC) at its Nevada test site between 1951 and 1958. In the Cold War atmosphere of the 1950s, the goal of maintaining U.S. nuclear superiority over the Soviet Union took precedence over the AEC's obligations to protect thousands of residents near the test site from radioactive fallout. While the patriotic, fervently anti-Communist Mormons of Southern Utah initially viewed the tests as necessary for national defense, such acceptance gradually gave way to a growing uneasiness. The AEC responded with public relations gestures and suppression of research data that cast doubt on its public reassurances. Increasingly cases of leukemia in areas directly in the path of the fallout eventually led a group of "downwinders" to sue for damages in a federal district court in 1979. Ball does a masterful job of discussing the legal obstacles faced by the plaintiffs under the 1946 Federal Tort Claims Act, the strategies and arguments used by attorneys for both sides, and the judge's 1984 ruling that AEC negligence was

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"more probably than not" responsible for the injuries of 10 of the 24 plaintiffs (p. 138). He argues, however, that judicial remedies may not be the most appropriate ones in such cases because of the time required for litigation and the difficulty of establishing causation in situations where the adverse health effects of toxic substances may not appear until many years after exposure. A better remedy, in Ball's view, would be some form of legislative compensation. Efforts in that direction, however, have been stalled by the uncertain costs involved, congressional indifference, and opposition from the nuclear industry, insurance companies, and executive-branch agencies.

Justice Downwind's strongest points are its solid historical documentation and clear explanation of relevant points of law and bureaucratic politics. Several chapters are marred, however, by repetitious writing that could have been made leaner and more concise by better editing.

More ambitious in its aims and more theoretical in its approach, Langdon Winner's *The Whale and the Reactor* is a call to develop a "political philosophy of technology": "Just as Plato and Aristotle posed the question, What is the best form of political society? so also an age of high technology ought to ask, What forms of technology are compatible with the kind of society we want to build?" (p. 52). The scientists, engineers, bureaucrats, business executives, and politicians who make technological choices rarely pause to consider what values or ends are served by the technological marvels they create. Winner contends that certain technologies "constitute political phenomena in their own right" (p. 21) because their use encourages or requires particular kinds of political relationships—"centralized or decentralized, egalitarian or inegalitarian, repressive or liberating" (p. 29). He discourses on a wide variety of topics, ranging from the computer "revolution" to the politics of language and differing conceptions of "nature" in the writings of economists, philosophers, and ecologists. Conventional approaches to understanding technology, such as those offered by risk assessment, technological determinism, "appropriate" technology, Marxism, liberalism, or technology assessment, are seen as inadequate foundations on which to construct a philosophy of technology because they fail to ask or answer the most profound questions concern-

ing the social and political ends of technological developments.

The Whale and the Reactor is more readable, coherently argued, and accessible to a wider audience than the author's earlier *Autonomous Technology* (1977). Its greatest strengths are its elegant writing and reasoning and its skillful exposition of the broader issues lying beneath the surface of contemporary discussions of technology. The book offers only vague generalities, however, in tackling the problem of reconciling technical expertise and political democracy. While Winner suggests that technocrats and ordinary citizens alike join in a dialogue to discover which technologies will best promote social justice, equality, and other important political values, he acknowledges that such an enterprise would require "qualities of judiciousness in the populace" that are now rare, as well as the radical transformation of many existing institutions (p. 55). The reader is left wondering about the feasibility and effectiveness of such proposed solutions to the problems he rightly deplores.

The flaws in both books are minor, however, and both are valuable additions to the literature on science and technology policy. Students of bureaucracy and the law of torts may find the Ball book of particular interest, while those inclined to ponder deeper and more fundamental questions concerning relationships between humans and machines may turn to Winner.

JAMES R. TEMPLES

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Science, Technology, and Public Policy. By Richard Barke (Washington: Congressional Quarterly, 1986. ix, 245p. \$10.95, paper).

Science and technology policy, as a field, confronts two large problems. First, the subject matter is exceptionally complex and wide-ranging. While there are discrete policies for science and technology, there are also many less-clear relations between science and technology and nonscience policies. This is because science and technology is a part—generally the "front-end" or new part—of most other policy functions, such as defense, energy, transportation, and so on. Second, the field is inherently

fast moving, a fact that means that the subject matter changes so quickly that books are soon outdated.

Hence, those who teach in the field have difficulty finding readings that overcome these problems. There is a need for material that covers a great deal of territory and at the same time uses illustrative examples that are timely.

Richard Barke's *Science, Technology, and Public Policy* attempts to address these issues. It is successful in that it has much fresh information which will make it especially helpful in teaching. It is not as successful in aggregating and synthesizing the disparate elements of the field.

Barke uses a framework of four types of constraints to investigate general characteristics of all science and technology policies. These are law, knowledge, coordination, and politics. Using this framework, Barke essentially takes the reader through the interactions of science and technology with various institutions of U.S. government (president, Congress, bureaucracy, courts). He also discusses the public and scientists as participants in the science-and-technology policy process. He includes chapter-length case studies of science in policy (hazardous waste) and policy for science (the space telescope). A concluding chapter sums up the author's view of the problems associated with science-and-technology policy and of what, if anything, can be done about them.

What is wrong appears to be a problem specific less to science-and-technology policy than to contradictions in democratic government generally. While Barke does not propose any grand reforms, he does say that whatever reforms in the system are considered must deal with the sheer "complexity of the science and technology policy process." This complexity is manifested in three ways: "fragmentation of decision making, the inseparability of facts and values, and the balancing of considerations" (p. 213).

Barke has written a useful book. However, his framework does not really bring about the coherence he surely sought. Each chapter contains a sequence of topics that do not always flow together. There is a certain choppiness, and this characteristic is exacerbated by an overuse of quick examples.

The author seems aware that the chapters contain a lot of trees in search of a forest. He

points out that he hopes the two cases at the end of the book will help the reader "to see how the pieces fit" (p. vi). The cases do help. But they are late in the book, and might have been integrated with earlier material to advantage. Hence, the framework of constraints is only a partial solution to the dilemma cited earlier, namely, a field that is extremely disparate and wide-ranging.

These reservations notwithstanding, I would expect many teachers of science-and-technology policy courses to adopt this book. It helps to fill a genuine need. It is up-to-date and interesting—and its very limitations in conceptual approach may stimulate others to try alternative methods. The challenge is large but, as Barke has shown, involves a subject that is worth the effort.

W. HENRY LAMBRIGHT

Syracuse University

American Women and Political Participation: The Impacts of Work, Generation, and Feminism. By Karen Beckwith (Westport, CT: Greenwood, 1986. xiv, 185p. \$29.95).

Karen Beckwith has written a straightforward and useful book that uses mass-level data from the Michigan presidential election-year surveys to explore the extent and roots of gender differences in political participation over the period from 1952 to 1976. She proceeds thoroughly and systematically to investigate not only the bivariate relationship between gender and political activity but also the effects upon political involvement of several variables—among them, work-force participation; membership in a cohort that comes of age in a decade of feminist protest; commitment to a feminist ideology; class; and race.

The major findings are hardly astonishing. First, with the single exception of the greater likelihood that women will agree that politics is too complicated to be comprehensible to the average person, any differences between men and women are minimal. Second, employment status, political generation, and feminist ideology are less potent predictors of political activity than is socioeconomic status. Along the

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way, various interesting tidbits appear—such as the fact that the subjective evaluations of respondent's level of political information made by interviewers (which give men the edge) are not borne out by the actual replies to questions testing political knowledge (which show no consistent relationship between gender and information).

Both the data and the analysis have some weaknesses that bear mention, however. First, as Beckwith herself points out, she is confined by the limitations of the National Election Study—which, given its emphasis upon electoral activity, is a less than perfect source for a study of the multiple ways in which U.S. citizens take part in politics. In addition, the data are a bit stale. Beckwith argues that “by ending our examination of women's political participation in 1976, we allow ourselves the advantage of avoiding a consideration of the rise of an intense, antifeminist minority and the confounding effects of the conservative Reagan administration and its opposition to women's rights” (p. 5). However, this seems to be making virtue of convenience, especially when recent Michigan surveys contain many items of relevance to her subject.

With respect to data analysis, there is a problem with data reduction. Even when Beckwith creates several activity scales (some of which, of necessity, mix attitudinal items with measures of political participation), there are six indicators of political activity and seven presidential-election years. Naturally, the results for 42 data points contain lots of noise. Happily, Beckwith eschews the temptation to invent an explanation for each random blip in the data. However, the end result is a certain mushiness. Finally the great care with which the analysis proceeds—on occasion the book's greatest asset—is something of a mixed blessing. For example, Beckwith devotes several pages (pp. 35–39) to a well-documented discussion of women's employment. Taken on its own terms, the argument proceeds quite reasonably. However, readers of this volume probably do not need to spend several pages learning that women have traditionally taken responsibility for child care and household management or that they are paid less than men when they enter the work force.

In spite of these shortcomings, Karen Beckwith has given us a sensible and useful volume that makes an addition to the library of any-

one concerned with citizen political behavior or women in politics.

KAY LEHMAN SCHLOZMAN

Boston College

Administering the New Federalism. Edited by Lewis G. Bender and James A. Stever (Boulder, CO: Westview, 1986. ix, 369p. \$39.95).

This is an edited book which seeks to examine the implications of President Reagan's domestic program, called the New Federalism, for state and local governments and for selected policy and management issues. For the most part, the focus is on the first four years of the Reagan administration, although some attention is given to the more recent period. In the introduction, editors Lewis Bender and James Stever attempt to draw some parallels between large organizations and the U.S. federal system. They argue that many administrative federal issues are but a subset of similar issues facing any large organization. These include those dealing with questions of leadership, coordination, regulation and accountability, authority, and efficiency. The book's first section features articles by Timothy Conlan and Richard Williamson who discuss the origins of the New Federalism agenda and some of the competing values apparent in the Reagan program.

Section 2 focuses on the implications of New Federalism for state and local governments. Irene Rothenberg and George Gordon in their chapter compare some of the administrative guidelines issued during the Reagan years with those of previous presidencies. They conclude that the major effect of New Federalism for local governments “has been to interject another government between themselves and funding agencies” (p. 90). In one of the book's best chapters, Paul Posner argues that our traditional distinctions between categorical and block grants may no longer be as useful as other methods of classifying grant programs. He suggests degree of state-management authority to be the most important variable. In an equally reflective article, Susan MacManus, Robert Stein, and V. Howard Savage assess the impacts of New Federalism for Texas cities

in the 1981–1983 period, focusing especially on Houston. They find little “short-term” impact of the Reagan initiatives, citing Texas’s changing economic and demographic conditions as more important. Focusing her essay on small cities in North Carolina, Beverly Cigler finds by contrast, “major impacts on North Carolina’s small communities, influencing their ability to undertake their responsibilities” (p. 176). Examining the situation in Tulsa, Raymond Rosenfeld and Alan Frankle in their chapter suggest that the potential hardships of the New Federalism agenda prompted Tulsa voters to be more supportive of local taxes and higher service fee, thus blunting some of the negative aspects of the administration’s program.

Part 3 looks at the implications of New Federalism in various policy areas. Charles Moore and David Sink examine housing policy in the Reagan years with special attention given to Birmingham, Alabama. They find the situation in Birmingham to be “grim” (p. 221). The Reagan program, they say, is contributing to a “halt of federal efforts to provide additional decent housing for those residents who cannot secure it on their own” (p. 221). Pinky Wassenberg examines state response to reductions of federal funds for water-pollution control programs and the administration’s attempt to return responsibility for establishing and enforcing pollution control standards to the states. The result, Wassenberg argues, is to deprive states “of the increased federal assistance they [need] to increase the administrative and technical capacity of state programs” (p. 243). Dale Krane in his article examines the Community Development Block Grant (CDBG) program in Mississippi and state takeover of that program resulting from the Reagan initiatives. He concludes that earlier “fears” of state takeover “appear to be unfounded. . . . Mississippi’s track record to date is positive” (pp. 265–66). In a similar vein, Susan MacManus finds little negative impact of CDBG restructuring in Houston. By contrast, she finds the new guidelines “offered the new mayor and the CD director the opportunity to get the program under control” (p. 289).

The final two chapters of the book offer something of a point-counterpoint exchange concerning the merits of New Federalism. Robert Agranoff and Valerie Rinkle suggest

that New Federalism may represent a major opportunity for opening new channels of communication—and thus new structures for planning and problem solving—between state and local governments. David Walker, in the book’s concluding chapter, observes that the weakened nature of state governments in the U.S. federal system poses significant obstacles to any domestic program that rests for its success largely on the will and the capacity of sub-national governments for significant policy innovation and implementation.

One of this book’s strengths is also one of its weaknesses. To their credit, the editors attempt to cover the full spectrum of issues surrounding the New Federalism program, including its history, philosophical background, administrative and programmatic impact, policy implications, and historic role in the U.S. federal system. Students will appreciate having one volume that attempts to cover all these topics; but, at the same time, the book cannot give adequate and complete attention to all these important issues. Perhaps a focus on only one or two major issues would have been advisable.

Further, the provocative framework established by the editors in their opening chapter is virtually ignored in the remaining chapters. The reader is left to draw his or her own conclusion as to how the points made by contributors relate or do not relate to the editors’ opening arguments. Finally, the book would benefit considerably from a concluding chapter drawing together the various themes and findings suggested in the previous chapters. Here the editors might have discussed some of the contradictory findings of contributing authors, provided an assessment of New Federalism in all areas covered (is New Federalism working or not?), and might have placed these findings in the context of the themes they assert at the outset.

In spite of these limitations—and the problems apparent here do not differ markedly from those in other studies of the New Federalism—this will be accepted as a valuable addition to the growing literature assessing the implications of the Reagan presidency for domestic policy and politics. The editors and contributors bring us “up-to-date” on current assessments of New Federalism and, while those assessments ultimately may change, this volume has considerably advanced both for

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the lay and the scholarly reader our understanding of the Reagan years.

RICHARD L. COLE

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Intergovernmental Relations and Public Policy. Edited by J. Edwin Benton and David R. Morgan (Westport, CT: Greenwood, 1986. viii, 224p. \$35.00).

With the editors' assertion that "a wedding of IGR [intergovernmental relations] and public policy was inevitable" (p. 5), I tend to agree. Certainly it is an appropriate focus for an anthology. Unfortunately, it apparently proved easier to assert the theme than to find authors with ready papers truly combining the two elements. In consequence, this volume gives us several competent pieces on IGR as such (e.g., on the Berger court's view of IGR and on metropolitan regional associations), but rather fewer that really tie the two together.

Among those that impressed me (such judgments always involve partly subjective criteria of interest) are Thompson and Scicchitano's comparing state with federal Occupational Safety and Health Administration enforcement in a seven-year period ending in 1983; Hedge and Menzel's similar focus, but in the Surface Mining program; Ervin and Watson's on state-local patterns in Community Development Grant administration; and one on changes in fiscal centralization in the states, 1957-83, by Stonecash. Each of these substantially and successfully blends the two foci of substance and process.

Other topics covered include federal-aid cutbacks, environmental policy, a comparative piece on local finance in West Germany, and an interesting brief look into the crystal ball by Beam and Benton. This last has a couple of thoughtfully provocative pages (pp. 209-10) on a reverse twist: "the localization of national programs."

It should be noted that earlier versions of one-third of the papers in this volume appeared in *Policy Studies Journal* (vol. 13 [March 1985]).

Stimulating as some of the individual empirical papers and essays are, the thought occurs to me that anthologies need more than a com-

mon nominal topic to constitute coherent volumes. If editors can arrange for authors to work within some common framework of hypotheses to be addressed, of method, or of values to be explored, we readers would be most grateful. In the book covered here, the closest to a common thread is that most pieces use statistical analysis. Most are also stronger in data than in suggesting ideas and propositions about the nature of the reciprocal relationship between the two variables contained in the volume's title.

MICHAEL D. REAGAN

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Re-electing the Governor: The 1982 Elections.

Edited by Thad L. Beyle (Lanham, MD: University Press of America, 1986. xli, 336p. \$15.75, paper).

Re-electing the Governor: The 1982 Elections is the result of a larger effort to understand the determinants and consequences of gubernatorial elections. In 1982, a network of state analysts was established to study two different phenomena: in the 36 states holding gubernatorial elections the successful reelection of incumbent governors and, in cases where power changed hands, the gubernatorial transition process. This volume, edited by Thad Beyle, focuses on the reelections of 16 of the 19 governors who were successful in their bids for another term. The book begins with an introductory chapter by Beyle that quite nicely summarizes some of the important aspects of gubernatorial incumbency, both in elections and in the operation of government. This introduction is followed by two interesting chapters on the role of money in gubernatorial elections, the first written by Malcolm Jewell and the second, again, by Beyle. Unfortunately the book goes downhill rapidly from this point. What comes next are 16 separate state "analyses" of the reelection triumphs of incumbent governors in 1982.

I have serious reservations concerning the utility of this book for social scientists interested in the determinants of gubernatorial election outcomes. The strategy of inquiry pursued in this book is not well suited to answering this or any other general research question. The 16 state studies are almost without excep-

tion simply descriptive accounts of the political environment surrounding the election, the election itself, and the political changes that followed the reelection. The one exception to this approach is the chapter by McDonald and Morgan, which complements the descriptive account of the reelection of Oklahoma's George Nigh with a county-level analysis of socioeconomic and political variables as well as some descriptive evidence from survey data. Even if all other chapters had pursued this type of within-state systematic analysis, the book would still come up short. The problem with this book is not just its descriptive nature (although this is certainly a major problem) but also its lack of a comparative focus. Instead of coming away from this book with a general explanation of successful gubernatorial reelection bids in 1982, what the reader gets is 16 separate descriptions of gubernatorial elections.

Even if such idiosyncratic descriptions could be melded into a more general explanation it would be of little utility due to problems of case selection. This book includes "analyses" of only 16 of the 19 successful bids for reelection. The problem here is not just that three successful reelection bids are excluded but also that the six unsuccessful bids and the eleven cases where no incumbent ran are overlooked. To explain successful gubernatorial reelection bids, you cannot limit yourself to examining only those cases where governors are re-elected. Surely the different conditions surrounding successful and unsuccessful bids for reelection, as well as the races not involving an incumbent, are important to an explanation of reelection successes. In short, to explain successful bids for gubernatorial reelection adequately you must be able to explain the determinants of gubernatorial elections in general. This is not accomplished by relying on descriptive accounts of a subset of elections "analyzed" one at a time.

The idea of postelection studies has a good deal of merit. My recommendation, however, is that the methodology be comparative and systematic rather than idiosyncratic and descriptive. This book is of the latter type and, therefore, is of little utility in a search for answers to general questions such as why some incumbent governors are successful in their bids for reelection.

As a final matter, I feel compelled to com-

ment on the poor quality of craftsmanship on the part of the publisher. The most glaring example of this is that the footnotes at the end of the introductory chapter bear no relationship whatsoever to the footnote marks in the text. As near as I can determine, the footnotes at the end of the chapter belong at the end of a chapter from the companion volume on gubernatorial transition. Other examples are of the more nitpicky variety such as misspellings, and asterisks in the tables with no corresponding explanation.

THOMAS M. HOLBROOK

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Presidential Transitions: Eisenhower through Reagan. By Carl M. Brauer (New York: Oxford University Press, 1986. xvii, 310p. \$22.95).

Presidential transitions, especially since receiving federal subsidies in 1968, have become something of a growth industry. Eisenhower's transition team was small, consisting of a few intimates; Reagan's, by contrast, was an elaborate effort with over one thousand salaried staff and numerous volunteers. There has, however, been no comprehensive book on the topic since Laurin Henry's *Presidential Transitions*, which appeared in 1960. Historian Carl Brauer, currently associated with the Center for Business and Government at the JFK School, has undertaken to update the story.

Brauer writes for a general, rather than scholarly, audience as he examines the basic dynamics of the Eisenhower, Kennedy, Nixon, Carter, and Reagan transitions. His treatment is broader than just the interregnum period (from election to inauguration) and spans the time from preelection transition planning to "established incumbency"—generally through the first year of the administration.

Each chapter-length case study of the five transitions explores a variety of themes, including the new president's interpretation of his mandate, his relations with the outgoing president, his stance vis-à-vis the Congress and the development of a legislative program, his relationship with the media, his appointment of White House office staff and cabinet and subcabinet officials, and a *tour d'horizon* of

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the various policy issues—foreign, military, domestic, and economic—that faced his administration.

Brauer's research is thorough, relying on memoirs, interviews with key participants, secondary reportage, and, in the cases of Eisenhower and Kennedy, presidential archives. The lack of archival material pertaining to the three most recent presidencies, though unavoidable, is regrettable. As scholars of the presidency know well, there are often considerable discrepancies between what is remembered by participants—who, as in the case of the Nixon administration, may have an ax to grind—and what is demonstrated by the historical record. (As Brauer volunteers, this renders his discussion, especially of the Nixon transition, "relatively tentative and speculative" (p. 122). Also disturbing is Brauer's apparent neglect of standard comparative analyses of several of his major themes, for example Calvin MacKenzie's work on presidential appointments (*The Politics of Presidential Appointments* [New York: Free Press, 1981]) and Richard T. Johnson's on White House organization (*Managing the White House* [New York: Harper & Row, 1974]). The result is a treatment which, at least for political scientists, is rather long on description and short on comparative analysis.

Brauer does extract some salient patterns from the transitions he studied. First, he finds, newly elected presidents fail to learn from the mistakes of their predecessors. They pay little heed to the incumbent's advice, for the latter, after all, has just been repudiated at the polls. In the postelection euphoria, "hope, sometimes arrogance, hubris, and a sense of infallibility run high. . . . This atmosphere . . . largely explains why new Presidents have been unable or unwilling to learn from their predecessors" (p. 258). Moreover, new presidents tend to overreact to perceived flaws in the man and administration they follow. Ike was too anti-political, Kennedy too antiorganizational, Carter too antiimperial.

Second, new presidents are overly prone to become enamored of organizational reform and to bow to the false God of "cabinet government." All save Kennedy entered office proclaiming the virtues of the cabinet and underestimating the importance of the White House staff. Later, they were forced to recant.

Third, in spite of campaign rhetoric promis-

ing dramatic changes in style and policy, basic continuity between one administration and the next, Brauer argues, is the rule rather than the exception. Real policy change is basically incremental, occurring over the life of several administrations. "Symbol and rhetoric tend to change much more than does the substance of policy" (p. 266).

Lastly, and somewhat ironically, the caliber of the transition itself—though it may help the new president hit the ground running—may not have all that much influence on the overall quality of the administration. Carter's transition effort, which he subsidized with his own campaign funds, was among the most comprehensive and carefully planned. Yet in Brauer's view, Carter's administration was among the least successful. In spite of federal funding for transition efforts, Brauer observes, "a newly elected President's most fundamental problems are far more attributable to his own shortcomings" (p. 183).

Brauer makes several suggestions he urges new presidents to heed. Noting that Carter, a stranger to Washington, surrounded himself with Georgians and that Reagan, unfamiliar with foreign policy, made little effort to remedy his ignorance, Brauer counsels the victors to "take a good hard look at themselves and determine how to compensate for their weaknesses—their strengths just naturally assert themselves" (p. 257). They should also examine more closely the errors made by their predecessors, give up utopian notions about cabinet government, pay closer attention to selecting their senior White House aides and subcabinet appointees, and stop bashing the career bureaucracy—with whom they will need to establish effective working relationships.

It is much easier to criticize books than to write them. I wish, however, that Brauer had spent more time on the organizational detail and dynamics of the crucial interregnum period. Further, though he alludes to the influence of the president's personality and beliefs on his transition and early incumbency, this theme remains largely undeveloped. In spite of its shortcomings, however, Brauer's study is informative, well written, and a useful addition to the literature.

RICHARD L. SCHOTT

University of Texas, Austin

Managing the Presidency: Carter, Reagan, and the Search for Executive Harmony. By Colin Campbell (Pittsburgh: University of Pittsburgh Press, 1986. xx, 310p. \$19.95).

Colin Campbell's major new book concerns itself with the institutionalized presidency, the performance of presidents, and presidential underutilization of the state apparatus. Campbell feels that writers on the presidency have created a false dichotomy between bureaucratic neutral competence and responsiveness. To Campbell, neutral competence need not lead to bureaucratic insularity and lack of responsiveness to the president. Neither does emphasis on creating a bureaucracy responsive to presidents mean that it will be incompetent. Rather, Campbell suggests that a mix between the two is possible and desirable, a situation that he terms "policy competence." Campbell's theory, then, is built on the interaction between neutral competence and responsive competence. A resulting typology occurs such that a bureaucracy that is both competent and responsive is the desirably "policy competent" bureaucracy. If it is competent but unresponsive, then neutral competence results; if incompetent though responsive, political incompetence arises; and lastly, if it is both incompetent and unresponsive, nonpolitical incompetence results. According to Campbell, presidents have some control over this mix.

In successive chapters, Campbell discusses the historical evolution of the cabinet, compares the U.S. cabinet experience to the British, looks at presidential organization of the White House, economic policy-making, and budgeting and management issues (chaps. 2-6). The chapters are presented as comparative case studies of the Carter and Reagan experiences, though a wealth of other materials are brought into the analysis, including British practices and presidential developments from FDR through Ford. Campbell's breadth and depth of knowledge is impressive, and he uses this knowledge to inform his case studies. Further, these case studies are presented within the analytic framework already mentioned and further developed as appropriate to each topic. As such, these chapters are not only informative but theoretical and analytical and therefore should serve as a model for the comparative-case approach.

These chapters provide a wealth of illum-

inating information and insights as well. Some that struck me include the illusion of decentralization and cabinet consultation in the Carter administration. Rather, Carter's penchant for detail and his reliance on his Georgian aides centralized decision making. Secondly, Reagan's mixed collegial-hierarchical approach, using the troika of Meese-Baker-Deaver early on, not only suited Reagan's personal style, but may have been an effective device to ensure both collegiality and discussion. Further, Campbell argues that economic pressures on the budget of the 1970s and 1980s, which led to budgetary retraction, have undermined the Office of Management and Budget (OMB) as much, if not more, than its politicization in the years since Nixon. When budget expansion reappears, the neutral competence and institutional strength of OMB may reassert itself.

Chapter 7 is a quantitative discussion of career bureaucrats and political appointees in the Carter and Reagan years. These are based on interviews; the interview instrument is presented in the appendix. It is to be hoped that future scholars will rely on that instrument by repeating it, expanding and refining it. Thus, Campbell's work, along with Aberbach and Rockman's and Kessel's, should be seen as a replicable data base upon which to build.

I strongly recommend this book. Though long, it is well written and well reasoned. Its theoretical arguments are strong and usually persuasive. A range of knowledge is brought to bear. Hence, this should be regarded as a model for studying the presidency. Lastly, we should thank the University of Pittsburgh Press for issuing it so economically. The book should enjoy a wide readership, one that extends beyond presidency scholars to those interested in bureaucracy, public policy, and U.S. politics as well.

JEFFREY E. COHEN

University of Illinois, Urbana

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Presidents and Foreign Policy Making: From FDR to Reagan. By Cecil V. Crabb, Jr. and Kevin V. Mulcahy (Baton Rouge: Louisiana State University Press, 1986. xiv, 359p. \$15.95).

Among the many points of interest in this book, there is the quite remarkable circumstance of its timing. The authors indeed could hardly have asked for a more perfect example of life imitating art. The basic purpose of their analysis is to examine "the challenge presented to the State Department's traditionally paramount position in the field of external policy by a growing number of executive departments and agencies—a phenomenon that has become pervasive and progressively acute since World War II" (p. 42). For academics and the general public alike, bemused by revelations concerning the activities of Colonel Oliver North, Admiral Poindexter, and others on the National Security Council staff in the "Iran-gate" affair, this book provides an intellectual and historical perspective from which to view the recent events.

The authors remark early on that the existence of the National Security Council has not really solved any of the basic problems of coherence and direction in U.S. foreign-policy making. Indeed, the NSC "may have aggravated the problem by adding another layer to an already complex foreign policy hierarchy [and] by generating confusion about who is really in charge of American foreign policy" (p. 81). To this judgment, Secretary of State George Shultz presumably could offer only a heartfelt *amen*. The basic difficulty that Crabb and Mulcahy face, however, is in offering evidence or even hope that the current disarray in U.S. policy-making might somehow be put right with appropriate changes in the institutional framework of the decisional process. The authors have done a very good job of describing the disease. Whether they may be judged equally successful in suggesting cures is another matter.

The book essentially falls into three parts: an initial overview of basic problems in the U.S. foreign-policy process, a series of case studies of various administrations from Roosevelt to Reagan, and a concluding chapter offering some potential solutions to the disarray they have so effectively described. The tale told is a familiar but still depressing one: the

increasing fracturing of authority within the executive branch on foreign-policy matters, with resultant confusion and lack of direction on fundamental issues. A particular emphasis, as noted above, is on the gradual withering of the primacy of the State Department in the policy process, and the rise of competing centers of advice and even execution, particularly within the White House itself. The authors quote with approval the plaintive comment of former Senator Arthur Vandenberg that Congress could "only cooperate with one Secretary of State at a time." Unfortunately, there seems little prospect that the increasing babel of different voices on foreign policy will be replaced by a greater sense of order. Crabb and Mulcahy do offer some specific ideas for reform, such as giving the vice-president a much more substantial role in policy-making and of course restoring some of the perquisites of the State Department. They do concede that ideas for reforming the policy process have been something of a cottage industry over the last 40 years, and they are quick to admit that they offer no panacea of their own to the prevailing difficulties.

A rather depressing but perhaps also realistic conclusion from all this is that Tocqueville's famous analysis in *Democracy in America* (1835) concerning the *inherent* tendency toward confusion in democratic foreign policy may be a permanently operating factor. Certainly nothing written or done during the last several decades holds out much promise that his gloomy analysis can be refuted. The fact that this admirably researched and well-written analysis itself fails to promise any enduring solutions is perhaps the best testament to this fact.

STEPHEN A. GARRETT

Monterey Institute of International Studies

Cities of the Prairie Revisited: The Closing of the Metropolitan Frontier. By Daniel J. Elazar, with Rozann Rothman, Stephen L. Schechter, Mauren Allan Stein, and Joseph Zikmund II (Lincoln: University of Nebraska Press, 1986. 288p. \$17.75).

This book examines the far-reaching effects of Lyndon Johnson's Great Society and Richard Nixon's New Federalism on local gov-

ernment in the United States or at least in some of the small metropolitan areas of the Midwest. The study is a second look at the metropolitan areas, first studied in 1970 by Elazar, and is one of the few comparative urban studies conducted over an extended period of time. It is contended by the authors that other studies of urban political power give only snapshots of single cities at single points in their history.

Elazar and his coauthors return after an absence of 15 years to Champaign-Urbana, Decatur, Joliet, Moline, Peoria, Rockford, Rock Island, and Springfield, Illinois; Davenport, Iowa; Duluth, Minnesota; and Pueblo, Colorado. In the first book, Elazar examined the political and social changes in these cities from the end of World War II to the beginning of the Kennedy administration. In this second visitation, an attempt is made to consider how the areas adapted to the changes brought about by the Great Society, the Vietnam War, and the Nixon years. An attempt is also made to look at the efforts of the federal and state governments to impose new and different standards upon the local governmental units. The authors claim to analyze the struggle between federalism and managerialism in the local political arenas; and they correctly assert that the cities by and large were able to assimilate external pressure and adapt it to local mores.

This book may well be the forerunner of many. It may bring to life more studies of community programs revisited after a period of years. Indeed, it may be necessary to revisit these cities of the prairie again *after* the full effects of the Reagan years have been felt upon their local revenue patterns.

The issue of continuity and change in the local power system is one of the central questions in community studies, one that the "snapshot" approach cannot answer. Examining the question over time, as is done in this study, does enable one to draw "tentative" conclusions. As the authors contend, it would appear that the more institutionalized the broad base, the more the continuity. Similarly, the least continuity is found in the activities of the "reformers" who enter for a specific change in the formal constitutional structures.

The authors make an interesting comparison between the 1972 Federal Revenue Sharing Act and the Morrill Act of 1862. Their basic argu-

ment, however, is that the solution to the real problems of U.S. cities and metropolitan areas is not in simply increasing federal largesse but in recreating the civil community.

Another contention is probably equally valid, that the problem of growth is a problem of accommodating not just numbers but newness and transience—the conditions that have always been present in the U.S. frontier.

RUSSELL M. ROSS

University of Iowa

Corrigible Corporations and Unruly Law.

Edited by Brent Fisse and Peter A. French
(San Antonio: Trinity University Press,
1985. 235p. \$30.00, cloth; \$14.95, paper).

This is a valuable addition to the libraries of people interested in the control of corporate behavior. Its interdisciplinary perspective could have been even more valuable with stronger contributions on a few topics. The volume contains the leading and most innovative thinking about its subject; unfortunately, it also occasionally lapses into some less well-honed analysis.

The chapters aggregate to a complete inventory of ideas for promoting compliance with law. Not surprisingly, Christopher Stone both leads off and leads with provocative proposals. He reflects on the limits of what the editors characterize as "the bold interventionist theories" he advanced in *Where the Law Ends*, and describes new strategies such as committees that would collect in-house data on problems related to company activity, problems insufficiently understood to merit a full rule promulgation by a regulatory agency. John Braithwaite suggests lodging with enforcement agencies a clearly defined set of principles to indicate who will be held responsible for specified types of noncompliance and, most radically in this country, discouraging the use of lobbyists, "professional purveyors of conflict." Fisse and French review alternatives to the traditional fine and both discuss the community service order and forced negative publicity.

The term "idea" to characterize this inventory is consciously chosen. Some of the chapters state explicitly and others suggest that

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much of the analysis of corporate compliance is at the level of thoughtful speculation by intelligent and experienced scholars or extrapolation from very limited data. As Fisse and French say in their useful introduction, "the interdisciplinary study of corporate activities crawls in its infancy." Braithwaite, comparing the public-interest director unfavorably with his notion of creating tension in the suites and tainting the chief executive with knowledge of illegalities, recognizes that the former strategy has not been found lacking on the basis of testing and empirical work. The volume communicates this message in a less satisfying way by including a couple of chapters that seem driven by ideological conclusions and read like sermons on the evils of capitalism and bureaucracy. For example, a slap at the agency bureaucrat seems misplaced in a scholarly treatment of a complex topic. Certainly, there is variability in competence and professionalism among public employees, but that should be a starting point for an organizational analysis of compliance rather than a self-evident principle explaining law violations.

Geis's thoughtful chapter notes that even careful empirical work may not be conclusive from a policy perspective. His is a review of recent research with some surprises on who is doing the major work. His skepticism about some of the more modern approaches for studying corporations is explicit: "We will have further detailed empirical work, with its precision purchased at the price of reducing complex matters to operational terms" (p. 80). (He feels that approaches that combine legal and social-science perspectives hold promise.) The reader joins in the skepticism that stems from certain results Geis reports including the findings that "sentences are generally tougher for college-educated offenders than for persons in other categories . . . [and that] . . . the possibility of imprisonment rose with an increase in the occupational status of the offender."

Another of the book's strengths is its identification of sources of corporate noncompliance. Stone informs us that some theory holds that legislative fines may be so designed to encourage noncompliance for a greater social good. Braithwaite recognizes the importance of individuals in an arena that is often analyzed with systems ideas and abstractions. These can lead us and enforcers to forget that identifiable people occupy significant and highly influential roles in even the largest cor-

porations. Executives set a tone, which may make them culpable and responsible. The Australian criminologist notes that those at the top can create unreasonable pressure that fosters unethical and noncomplying behavior. In a report on his study of the coal-mining industry Braithwaite describes the opposite phenomenon. Safety leaders in the industry had several things in common but the most interesting and generalizable was "a corporate philosophy of commitment to safety and communication of the message that top management did not perceive cutting corners on safety to achieve production goals as in the interest of the corporation." He concludes that "if we wish to maximize the compliance impact of additional investment in enforcement, the most productive targets will be chief executives."

A particularly strong chapter is Byrne and Hoffman's which uncovers the religion that sometimes masquerades as scientifically tight analysis of organizational behavior. This is a careful and comprehensive critique of the law and economics school that has dominated studies of organizations in recent years. Special attention is given to antitrust, but the chapter has broader implications and employs a wide literature.

The breadth and coverage of the volume are appropriate even if difficult to circumscribe conceptually. There is, for example, a chapter on factors that influence the exercise of prosecutorial discretion, written by a former U.S. attorney. This is a lawyer's article but it is written clearly and may be of general interest to those who wish to explore within the black box of the enforcement variable. Rakoff educates us well on constitution-based limitations, as well as on ethical and eye-opening practical considerations in the exercise of discretion, including the prosecutor's estimate of the propriety of the motives of the complainant.

Fisse and French have assembled some very good thinking by a leading group. The book could have been somewhat more fully edited (the same few cases and anecdotes are used throughout); nonetheless it should be welcomed by all students of corporate compliance. It moves the interdisciplinary understanding of the corporation's reaction to law a large step forward.

JOSEPH DiMENTO

University of California, Irvine

Controlling Bureaucracies: Dilemmas in Democratic Governance. By Judith E. Gruber (Berkeley: University of California Press, 1987. xi, 232p. \$29.95).

Try ordering a bureaucrat around, Judith Gruber argues, and the result is likely to be trouble. The effectiveness of an agency's functions will suffer, and enforcement of controls can consume vast resources. Understand how the agency goes about its work and how the bureaucrat views his or her job, on the other hand, and things can be different, for bureaucrats see some controls on their behavior as acceptable, or even useful, and will accept them in exchange for the important resources of outsiders.

Gruber's focus is the classic question of how unelected officials can be democratically controlled. Her book begins with an analysis of the options for controlling bureaucracies. It continues by linking various forms of control to theories of democracy, and by specifying their costs. Then comes evidence, gathered from interviews with housing, school, and fire-department employees in a "moderate-sized northeastern city," on the ways bureaucrats view themselves and their work. A theoretical chapter follows, on the differing meanings and forms of control in differing policy areas; and then the interviews are reintroduced in order to show how bureaucrats perceive such control efforts within their own domains. A final chapter returns to basic questions and begins to develop the notion of "control as exchange."

This is a successful book. It pulls together, in a disciplined way, much of what we know about bureaucracies and the ways we have tried to control them. And instead of relying for evidence upon hypothetical cases or anecdotes from policy battles long gone by, Gruber uses her interviews to construct a much more dynamic picture of the bureaucrat's world than we are typically given. The result is an understanding of the generally dismal record of bureaucratic-control efforts, and a surprisingly hopeful conclusion that we can indeed do better.

"Doing better," however, will require a sophisticated understanding of the bureaucrat's world, and of the variations to be found in different policy areas. This book offers the beginning of such an understanding, but much remains to be done. The notion of control

through exchange is presented and briefly discussed at the end of the book. I wished for a full chapter offering a more complete development of the idea, perhaps showing how it might be applied to the three agencies under study. Another issue underlying the whole analysis—that of whether or not the people and or their elected representatives are capable of democratic self-government—also needs more discussion than it gets. And while an organization's environment is identified—along with its core technology—as a factor defining important variations among policy fields, the idea seems quickly to lose out to technology as a focus.

But these complaints amount mostly to a wish to see the analysis extended further. The book is well argued and is rigorous at the theoretical level while remaining sensitive to the variations to be found among public agencies and types of policy. By talking to the bureaucrats, listening with a sensitive ear, and carefully presenting their views, Gruber has avoided the aridity which characterizes too many discussions of bureaucracy. And her conclusions on control-as-exchange remind us that, while our elected representatives often turn over significant funds and powers to unelected officials in the pursuit of goals they have only vaguely defined, bureaucrats still need the resources and backing of outsiders. Within their agencies, everyday concerns may well be defined in administrative terms, not in the language and concepts of democratic theory; but politics still matters, and therein lie opportunities to reconcile bureaucracy with democratic control.

MICHAEL JOHNSTON

Colgate University

Going Public: New Strategies of Presidential Leadership. By Samuel Kernell (Washington: Congressional Quarterly, 1986. xi, 251p. \$16.95, cloth; \$11.95, paper).

Over the past few years, the number of studies examining the presidential leadership strategy termed *going public* has mushroomed. Undoubtedly spurred in large part by Ronald Reagan's frequent use of the strategy, these studies grapple with what constitutes *going public*, why it is on the rise, and how to deter-

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mine if it is successful. Samuel Kernell's book, *Going Public: New Strategies of Presidential Leadership*, is the latest of these works and is certainly among the best. Still, Kernell's effort like those before, leaves the inquisitive presidential scholar with many lingering questions.

The book defines *going public* as a president's promoting "himself and his policies in Washington by appealing to the American public for support" (p. 1). Such appeals are on the increase, according to Kernell, because institutionalized pluralism is being replaced by individualized pluralism; the latter characterized by individuals who have few group loyalties and prefer immediate payoffs. In short, as the modus operandi of the Washington political community has shifted, Neustadt's bargaining president has been replaced by a public president. One is left wondering, though, how completely going public has actually supplanted bargaining. After all, an administration pursues hundreds of policy initiatives during its tenure, far more than its president could publicly push.

More troubling, however, is Kernell's reliance on individual pluralism as an explanation for the rise in going public. A more complete job needs to be done of disentangling the complex relationship between the shift from institutionalized to individualized pluralism and recent advances in communications and transportation. Kernell correctly recognizes that going public would be virtually impossible without telecommunications and high-speed air travel, but he is less clear about where these technologies fit in the causal chain culminating in the public presidency. It seems quite plausible that a president with access to modern technology would often opt to go public even if bargaining reigned supreme in Washington. Put differently, the relationship between individualized pluralism and going public may prove spurious when controlling for advances in technology.

Also intertwined among these forces is the Washington press bureau. Kernell does a superb job of summarizing the changing relationship between the president and his press corps, but his treatment of the role of the press in the dynamic process of going public is less complete. Does the aggressive contemporary Washington bureau force a president to go public, perhaps even "create" a public president? Or does going public shape the press,

perhaps making it part of the new individualized pluralism? The reciprocal relationships between these and other key forces need to be fleshed out, first in theory and then in empirical analysis.

The book excels as a chronology of going public. A good number of examples are described in detail, such as the Truman Doctrine speech and Reagan's budget crusades. *Going Public* would be strengthened, though, if a comprehensive scorecard were kept. How often have presidents' public appeals met with success? Also useful would be a more complete breakdown of going public by administration, party, policies (e.g. domestic or foreign), medium used, and whether the appeal was national or group-specific.

The strongest section reexamines an old question, What determines presidential popularity? Kernell argues that public approval is the "keystone" of going public; that without it neither the populace nor the Washington elite will likely be persuaded by presidential appeals. Maintaining a high popularity rating is crucial, then, to the success of the new leadership strategy. And through some nifty original analysis, the author demonstrates conclusively that high popularity and healthy economic conditions go hand in hand.

Going Public addresses many of the tough questions raised by the emergence of the public presidency. While its answers sometimes fall short, the book successfully frames these questions, thus paving the way for much fruitful research in the future. For this, Kernell deserves substantial praise: bringing some order to such a complex phenomenon as going public is no easy task.

TOM RICE

University of Vermont

Taking Reform Seriously: Perspectives on Public Interest Liberalism. By Michael W. McCann (Ithaca, NY: Cornell University Press, 1986. 345p. \$29.95).

It has been roughly 20 years since the liberal public-interest movement began to gain widespread attention for its advocacy in Washington. During this time there has been little critical evaluation of the movement's goals and political ideology. This is the task that Michael

McCann sets for himself in his new book, *Taking Reform Seriously*.

In the first half, McCann offers a careful and detailed analysis of the political and social philosophy of the liberal public-interest movement. This was no easy task, as there is no collection of great works by public-interest activists to consult nor any commonly accepted manifestos to review. To describe the world view of the public-interest movement, he had to piece together the work of a wide variety of scholars and activists from a highly diverse set of sources.

McCann's synthesis of the political ideology of the liberal groups alone makes this book an important contribution to the literature. But he goes much farther to provide a thoughtful and provocative argument that the philosophy of the movement has in many ways worked to undermine its long-term viability. He emphasizes how the movement's consumer and environmental proposals ignore the very real concerns of working-class citizens. McCann views the liberal groups as the political arm of an elitist class, far removed from the mainstream of U.S. life and thus all the more vulnerable to the vicissitudes of the political environment.

McCann's harshest criticism is directed at the movement's failure to help build a true participatory democracy. He notes that "the reformers have managed to mobilize voluntary support from millions of card-carrying members," but at the same time "they have actually engaged very few of these members as genuinely active participants in political life" (p. 170). Although it is one of their pronounced goals, Washington-based public-interest lobbies have generally done little to nurture and revitalize citizenship in the United States.

McCann sees the tensions and contradictions of public-interest liberalism as a cause of its downfall. After many important victories in its early years, the movement is said to have faltered badly as opposition to its goals has mounted. In this vein, McCann makes a surprising judgment about the performance of public-interest groups during the Carter years. He holds that the movement's serious decline began at this point as public-interest groups found the Carter administration to be "a bitter disappointment" (p. 127). There were some disappointments to be sure, such as the defeat of the proposed Consumer Protection Agency.

Yet day in, day out, public-interest lobbyists had generous access to agency policymakers throughout the administration, and they played an important role in developing public policy during those years. At the USDA Food and Nutrition Service, for example, former public-interest lobbyists ran the whole shop. The open access that hunger lobbyists enjoyed at this agency would make a Tommy Boggs or Charls Walker jealous.

McCann's view of the Carter years is important because it goes to the heart of his argument that the movement has suffered a fundamental decline because of its unsound philosophical foundation rather than a more transitory downturn because of a turn to the right that came with the election of Ronald Reagan. There's room for disagreement on this question, but McCann weakens his own case by not offering clear criteria for evaluating the relative level of success for these groups.

Although I wasn't convinced on all counts, I found *Taking Reform Seriously* to be an original and enlightening book. McCann's work is valuable reading for all those interested in contemporary interest-group politics.

JEFFREY M. BERRY

Tufts University

Thinking in Time: The Uses of History for Decision Makers. By Richard E. Neustadt and Ernest R. May (New York: Free Press, 1986. 329p. \$19.95).

Few would quarrel with Machiavelli's observation that in order to know what is going to happen, one must know what has happened. But the lessons of history can be obscuring and misleading as well as illuminating and orienting. Clio is an enigmatic muse, and her analogies are Delphic. Today, we would hardly share young Ranke's optimism when, in a fervent address to Goethe in 1817, he spoke of "the solid ground of history."

This collaborative work by Neustadt and May, which is based in large part on their jointly taught course at Harvard on the pitfalls of decision making, returns to the major theme of a previous book by May on the (misapplied) "lessons" of history (*"Lessons" of the Past* [Oxford University Press, 1973]). The present

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study is replete with examples and case studies, mostly drawn from the U.S. experience, that illustrate the dangers of being guided by historical analogies that turn out to be inappropriate for the contemporary context to which they are applied. The central theme of the book is that policymakers often use history badly and that they must learn to use it more discriminatingly. Neustadt and May recommend that decision makers divide the elements of a decision situation into three categories—*known*, *unclear*, and *presumed*—and that they carefully examine historical analogies to determine if and how they are applicable to the case at hand. Issues should be identified clearly, their “time lines” should be connected with considerations of feasibility, and events should generally be seen within the “stream of time.” The authors argue that even marginal improvements in decision making are worth seeking, and that they are obtainable if one would only raise the helpful journalistic questions *who*, *what*, *when*, *why* and *how*.

The “catalog of issues” developed by Neustadt and May is drawn from both domestic and foreign-policy issues, it includes both successes and failures, and it is extensive. The case studies are examined in some detail and are grouped under the following headings: “Drawing Analogies” (Vietnam, the onset of the Cold War, Korea, the 1976 swine-flu scare, Verdun, and Versailles); “Inspecting Issue History” (the Mayaguez incident, the Pittsburgh political machine, Social Security, and wage and price controls); “Probing Presumptions” (the Intelligence Process, the Bay of Pigs, and U.S. aims in the Korean war); “Placing People” (Mary Anderson and the Women’s Bureau, J. W. Fulbright, Martin Luther King, Malcolm X, Frances Perkins, and Vietnam advisers); “Placing Organizations” (the CIA and the United Mine Workers of America); “Noticing Patterns” (economic growth, Keynes and Friedman, U.S. analyses of the Soviet economy, and other cases); and “Judging Change” (the Great Crash, the Civil War secession, Prohibition, George Marshall, and George Washington).

This book is full of common sense and the wisdom that its persistent application engenders. It is a remarkable achievement, rich in analytical insights, written in a lucid and relaxed style, measured and fair in its political judgments. And yet there is something

curiously unsatisfying about this enterprise, perhaps because its central premise does not fully correspond to what we experience in our daily lives. Surely we (as well as decision makers) make our choices not only by considering possibly appropriate analogies that come to us from the past, but also by interpreting those that are offered to us by the present. We apply the lessons we obtain currently in one context to the decisions we need to make in another context. We may see ourselves in a diachronic stream of time, but we also occupy a place in a synchronic range of space. Our use of “history” is not only linear and vertical but lateral and horizontal. Indeed, there is ample evidence that decision makers are guided in their actions by present as well as past experience—and both may be misapplied. For example, our early involvement in Vietnam in the fifties stemmed both from misplaced historical analogies of the past and from the seductive contemporary “lesson” that dividing Vietnam could, like the concurrent cases of Germany and Korea, serve as the basis for an accommodation with our opponents. We sought to apply the principles and assumptions of our containment policies, which were taking hold in Europe, to other parts of the world, where they obtained little leverage.

By placing such a heavy and exclusive emphasis on the dangers of improper thinking in *time*, Neustadt and May ignore the equally compelling and perilous analogies that come from thinking in *space*.

WOLFRAM F. HANRIEDER

University of California, Santa Barbara

Liberalism at Work: The Rise and Fall of OSHA. By Charles Noble (Philadelphia: Temple University Press, 1986. x, 292p. \$29.95).

Liberal reforms such as the worker protection regulation implemented by the Occupational Safety and Health Administration (OSHA) are unlikely to succeed in the United States without strong grass-roots organization of beneficiaries—in this case workers—and greater willingness on the part of the state to intervene more directly into the investment decisions of the corporate United States. Noble’s underlying argument in this book is

not simply that OSHA failed to have much impact on worker safety and health but that reformers missed a major opportunity to use this legislative initiative in order to introduce structural changes to alter power relationships at the plant level, where dominant corporate interests work against safety and health improvements.

During the past century of capitalist development, market pressures have led management to two strategies that ultimately lead to underinvestment in worker safety and health, one emphasizing full control over the production process and the other emphasizing ways to minimize costs of production. Factors such as wage premiums, production losses attributable to accidents, and the need to conserve labor may partially offset investment disincentives, but Noble notes the problems with each of these factors and concludes that they are not enough. Liberal statist reforms developed to counter recurrent problems of workplace safety, primarily workers-compensation schemes and factory legislation, are also ineffective because weak government agencies were never given sufficient authority and enforcement resources to challenge the dominance of business interests seriously, particularly in matters affecting the production process. Only workers organized at the factory level could have provided the government agency with sufficient resources for effective intervention in production processes, but the one likely source of this organized effort, the industrial unions, had chosen to seek economic rewards rather than challenge management's strategy of controlling the production process. In combination, reforms favored by unions and liberal reformers, according to Noble, are doomed to failure unless the need for structural changes are recognized.

Compared with the half dozen existing political-science studies about OSHA, Noble's broad historical account of capital-labor-state relations in the United States as they impinge on safety and health issues in the workplace uses more radical terms to analyze government interventions in "industrial capitalist economies." Yet the tale Noble tells provides a skilled analysis of familiar themes of regulatory capture. Increasing workplace problems during the 1960s discredited existing health and safety policies and indicated rank-and-file discontent. A number of fortuitous circumstances

led to the passage of OSHA, including analytic groundwork by a few legislative activists, the political entrepreneurship of the Johnson White House intent on appealing to union workers, the support among Democrats in Congress for protective social legislation, and the disarray of business lobbies. Noble's detailed account of the origins of OSHA, based on interviews and memos from administrative officials, is one of the most interesting contributions of the book to understanding why a major legislative bill emerged despite the disinterest of unions (the presumed beneficiary). Factors increasing the impracticabilities of policy processes are well portrayed, particularly in the amusing account of how business allies inadvertently strengthened the bill with suggestions intended to weaken it.

The rest of the story is familiar but not as convincingly told. Business groups organized to oppose the burgeoning social regulations, while labor failed to take initiative. With the help of a declining economy and a White House review process responsive to economic concerns of business and academic allies, businesses succeeded in rehabilitating the values of market capitalism. OSHA's activities were systematically curtailed. "By the end of 1984, social regulation was dead in the water" (p. 161). But the accounts of changes in OSHA's actions, particularly under Carter, point out the variety of forces behind these changes, many of which counter the main theme of inevitable decline. OSHA is found to have some effect but not enough. Noble does an excellent job of documenting the political forces aligned against "liberal" reforms based on regulatory agencies but says little about how his preferred strategy of increasing worker controls at the factory level and more extensive state controls over investments is likely to bring about greater effects in the same political setting. Similar strategies to mobilize new groups in the war on poverty politically, failed for predictable reasons. Nonetheless, Noble's broad perspective, extensive scholarship, clear presentation, and reformist sympathies make this book a delightful change from the normal market perspective currently dominating discussions of social regulation.

JOHN SCHOLZ

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Alternative Americas: A Reading of Antebellum American Culture. By Anne Norton (Chicago: University of Chicago Press, 1986. ix, 363p. \$24.95).

This book follows the tradition of Tocqueville, Louis Hartz, Sacvan Bercovitch, and others who have attempted to explain the United States' cultural tradition. In *Alternative Americas*, I think we have a valuable, although perhaps controversial, addition to that corpus. Although her narrative encompasses antebellum culture in the U.S. West as well as in the North and South, her primary focus is on the North and South, and her major contribution lies there.

Norton argues that U.S. political culture prior to the Civil War was a political culture marked by an increasing separation between the North and the South. This separation reflected a difference between the regions that arose not because of a disagreement over slavery, or between different views of federalism, or even between the existence of an increasingly commercial North and a feudal South. These arguments have been made by others. Norton argues that such views of the South "ignore the language of the era" (p. 4). Instead, she argues that the distinction between the two regions is one that is fundamentally and explicitly sexual in nature: "Throughout America, throughout the antebellum period, the North was identified as masculine and the South as feminine, in an elaborate constellation of metaphors" (p. 4). These perceptions of the South and the North are revealed in the relationship that developed between white Anglo-Saxon Protestant males and groups which were marginal, or liminal, to these societies. In the North, the liminal groups were immigrants—especially the Irish—and women. Slaves, women, and Indians were the liminal groups in the South. In the case of both regions, however, the relationship between liminal and dominant group was perceived in sexual terms. Thus, Norton argues that in the North, the needs of industrialization resulted in a valuation of "masculine traits," for instance, rationality, hierarchy, and community defined by law. The South, with its primarily agrarian economy, tended to see itself primarily in "feminine" terms, valuing self-expressiveness, individual autonomy, and emotionality.

The different development of North and South has important consequences for the relationships between the dominant and liminal groups in each region. In the North, feminine characteristics were ascribed to liminal groups, and resulted in attempts to "reform" them. The goal here was to assimilate these groups into society not as equals but as part of a docile labor class. Liminal groups were to perform as docile "wives" to a masculine paternalistic industrial elite. Thus, the North proposed a paternal state that would use its power both to control and to mollify. The South chose a different path, appropriating the characteristics of liminal groups and identifying with them. Thus, because they identified with women and their maternity, southerners chose to adopt a model of social relations that mimicked familial ones. For example, Norton argues that southerners identified themselves with the Indian and that that identification led them to describe the supplanting of the Indian by the white man as a fraternal conflict, like that between Cain and Abel. Southerners saw that the Indians "must be torn from the breast to make room for the encroaching whites" (p. 145). The South rejected the model of state paternalism embraced by the North but exhibited a plantation life that was itself paternalistic. In both cases, the relationship between dominant and liminal groups was one that ultimately denied the equality of the members of the liminal groups.

This very short synopsis does great violence to the richness of Norton's argument. However, rich as the argument is, it is not entirely convincing in its discussion of the South. The basis of the South's feminine character is grounded by Norton in the agrarian nature of the region. The continued reliance on the earth, and the resultant ties to natural process of growth and season, she argues, results in the image of the South as "motherland." Two points should be noted here, though. The first is that at bottom, I think it might well be argued that the nature of the relationship between the southerner and the land is not harmonious, but instead domineering. It is plausible that the southerner perceived land as something to exploit, particularly given the historical accounts describing the wastefulness of Southern agricultural methods. This would make their attitude toward nature no more salutary than that of their northern counter-

parts. Second, although the southerner might well have appropriated "feminine" traits to define his self-image, it seems to me that this led to a redefinition of masculinity—the image of the indulgent, sensual, violent, and independent Southern male is that of the typical "good ole boy," who would quail at the notion that he was "feminine" in any way—rather than a true identification.

Finally, I found it interesting that Norton makes an association between agrarianism, femininity, and a republican political culture. Certainly, Rousseau, that old advocate of republicanism, would find the association of femininity with republican government surprising. One might well ask why this association would take place in the United States, but that goes beyond the scope of this work.

These comments are hardly meant to be criticisms. This book is thought-provoking and well argued and will be of great interest to those who work in political theory, women's studies, Afro-American studies, and U.S. politics. It is a fine book well worth a first taste and, like a fine wine, a second as well.

LIANE C. KOSAKI

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Making Cancer Policy. By Mark E. Rushefsky (Albany, NY: State University of New York Press, 1986. xiii, 257p. \$39.50, cloth; \$12.95, paper).

The sudden proliferation of federal health, safety, and environmental regulatory programs in the 1970s is one of the major events of recent U.S. history and governance, yet one whose substance has received surprisingly little attention from political scientists. For more than 10 years now, a primary objective of these programs has been the control of chemicals that may cause cancer and to that end the elaboration of detailed policies—"cancer risk-assessment guidelines"—setting standards of evidence for use in regulatory proceedings.

Mark Rushefsky's *Making Cancer Policy* provides an excellent analysis of the interpenetration of facts and values, of science and politics, that he argues is inevitable in such a process. Unlike many other books on environ-

mental politics and policy, it is neither an advocacy tract nor merely an account of the interest-group politics surrounding these issues, but a penetrating and insightful synthesis of theory, history, and substantive analysis of a complex and technical policy issue. It should be valuable not only to readers interested in cancer policy, but also to those interested in risk assessment as a form of policy analysis and in the politics of science and regulation more generally.

Four themes run through this material: the problem of imperfect knowledge and scientific uncertainty; the existence of controversy within both the scientific community and the larger society; the inevitability of mixing political judgments and assumptions into ostensibly scientific assessments supporting regulation; and the political uses of science itself by advocates of particular policies. A central principle of recent Environmental Protection Agency (EPA) policy, for instance, is that "risk assessment" is a purely scientific enterprise, to be clearly distinguished from "risk management," in which political, economic, and other value considerations are added in and regulatory decisions made. Rushefsky argues persuasively that this distinction is untenable, that policy judgments and value-laden assumptions are inevitably commingled with regulatory uses of science even in the assessments, and that regulations are therefore more appropriately developed through such mechanisms as "consensus dialogues"—providing for open debate over what assumptions are to be used—than through the pretense of scientific neutrality.

Rushefsky develops these themes first by describing the regulatory use of science to produce risk assessments, then by documenting the evolution of cancer regulation and related policies (with particular emphasis on the period since 1970), and finally by critically analyzing the issues that must be addressed in cancer regulatory policy today and the arguments offered by other writers on the subject. For a theoretical framework he uses two sets of concepts: John Kingdon's characterization of policy agenda setting as growing out of the interactive evolution of problems, policies, and political events (*Agendas, Alternatives and Public Policies* [Boston: Little, Brown, 1984]) and to a lesser degree Patrick Hamlet's characterization of technological policy-making as occurring in distinct "arenas" or

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"estates," each subject to its own distinct incentives, influences, pressures, constraints, and goals ("Understanding Technological Development," in *Science, Technology and Human Values* 9 [Summer, 1984]: 33-46).

Particular strong points of Ruchefsky's book include its mature treatment of all sides in what has become a shrill and contentious policy issue, and its frequent use of concrete cases to illustrate its points. Its title notwithstanding, it does not address all aspects of cancer policy (the politics of cancer research funding, for instance, or the politics of tobacco subsidies); nor does it pursue some questions as far as one might wish. One key question left unanswered, for instance, is whether ideology or science was the primary driving force in the policy changes Rushefsky describes. Were EPA officials (such as John Todhunter, for instance) simply political ideologues who used scientific assumptions selectively to justify their preconceived positions, or were they good scientists articulating changes in scientific understanding whose implications happened to be consonant with the Reagan administration's agenda? Both the reputations of the individuals and our understanding of subject turn on the answers to such questions, yet Rushefsky stops short of answering them.

Notwithstanding these minor criticisms, Rushefsky's book provides a solid and readable scholarly treatment of the regulation of environmental carcinogens and a valuable contribution to the literature.

RICHARD N. L. ANDREWS

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Disenchanted Realists: Political Science and the American Crisis, 1884-1984. By Raymond Seidman and Edward J. Harpham (New York: State University of New York, 1985. vii, 295p. \$39.50, cloth; \$12.95, paper).

The authors of this volume argue two basic theses: First, from the beginnings of U.S. political science there can be traced a lineage of practitioners who constitute a "third tradition." These individuals, all characterized as "liberals," believed that the problems of our U.S. society could be solved by "linking objective professional political study with political

reforms." Convinced that a scientific political science could lead to whatever changes were required to make the U.S. democratic system function effectively, they "blended scholarship and political advocacy, a science of politics with a science 'for' politics" (p. 2).

Second, the reformist proposals espoused by the third tradition (from Lester Ward in earlier years to Theodore Lowi and Walter Dean Burnham today) have been not only manifestly but inescapably unsuccessful. Consequently, if U.S. political science is meaningfully to address our current political, social, and economic ills, it must abandon the "centrist liberalism" of the third tradition and move drastically to the right or to the left.

These two propositions, it is important to note, have no inherent logical relationship. That is, the truth or falsity of one has no necessary implications for the truth or falsity of the other—a view, I suspect, with which the authors might not agree. In any event, I will deal with them in order.

According to Seidman and Harpham, there are two long-established traditions in U.S. political thought (a term sometimes employed as synonymous with *political science*)—the *institutionalistic* tradition and the *radical democratic* tradition. The institutionalists (they point to the "American Founders") are skeptical about human political capacities, inclined to put their faith in the Constitution, and utilize "a political vocabulary of system, mechanism, control, realism, skepticism and 'facts'" (p. 5). In contrast, the radical democrats (e.g., Tom Paine) trust popular virtue and sentiment and favor "continuous experimentation and impromptu forms of popular power associated with equalitarian democracy" (p. 6).

The newly discovered third tradition, however, seeks to go "beyond mechanics and spontaneity." It is less a synthesis of the other two, according to Theodore Lowi's Foreword, than "an organized effort virtually to deny that these are separate and inconsistent streams of political thought and values" (p. xiii).

What do they have in common, these eponymous "disenchanted realists" (a phrase that appears so rarely in the text that one senses the publisher's helpful hand) who constitute the third tradition? They are all "liberals" (a term never defined and used with remarkable elasticity); they all believed in a "science of poli-

tics" or a "scientific politics" (terms used interchangeably); they regard the state as essentially benign; they do not think that "citizens are by nature greedy, self-seeking and irredeemable"; they have great faith in the possibilities of civic education and, as previously mentioned, of political reform. "Relevance" appears as a specific requirement only in the discussion of postbehavioralism.

The book's opening chapter briefly describes the institutionalistic and radical democratic traditions and then, at somewhat greater length, delineates the beliefs common to the third tradition. Chapter 2 traces the emergence of a professional U.S. political science and the concomitant appearance of the third tradition in the persons of Lester Ward and Woodrow Wilson. Chapters 3-6 carry the story down to the present: chapter 3 focuses on Bentley and Beard, chapter 4 on Merriam and Lasswell, chapter 5 on V. O. Key and David Truman, and chapter 6 on Lowi and Burnham. The treatment is essentially the same in each case: the ideas of each of the aforementioned individuals are analyzed to show how they can be fitted, if occasionally in Procrustean fashion, into the third-tradition mold. What we have here, then, are eight intellectual biographies, running some 10-20 pages in length, interestingly written, and providing a useful, if perhaps sometimes arcane, reading and assessment of eight of our leading practitioners, past and present.

The key question, of course, is whether the authors make a convincing case for their thesis. There are, I must say, some difficulties. As indicated above, key terms are often undefined and loosely used. Not only are the first and second traditions treated in sketchy fashion but, more troublesome, the authors fail to identify a single political scientist associated with either of these traditions, despite their claim that "modern political science has been understood as a simple extension of one or the other of them" (p. 3). This omission makes any meaningful comparison among the three traditions extremely difficult.

That difficulty is further compounded by the professional and political commitments that, the authors contend, uniquely characterize the third tradition—namely, aspirations toward a more scientific politics; a positive attitude toward the state; and faith in human nature, education, and reform. These (and possibly

even "relevance") have been the values, I would suggest, of many, if not most, U.S. political scientists rather than of any select "subfield within a minority" (p. 191).

In short, I remain dubious that the eight political scientists singled out in the volume really constitute an intellectually homogeneous set of "disenchanted realists," or that they represent a truly separate third tradition in either the practice of U.S. political science or the history of U.S. political thought. Try as I might, I cannot escape the sense that there is an essentially contrived quality to the thesis. In fact, the authors themselves tell of the slow metamorphosis of a "semester-long reading project among three Cornell graduate students" into "a longish paper, a short monograph, and finally into three or four versions of the present work" (p. xix).

Chapter 7, "Conclusion: The End of the Third Tradition," advances two propositions. The first is that liberalism generally, and especially that variant exemplified by the third tradition, is hopelessly incapable either of correctly diagnosing, or of proposing useful solutions for, the ills of contemporary U.S. democratic capitalism. The liberals cannot do so because, given their inherent ideological limitations, they are unable to comprehend the "political proclivities of American capitalism," the gravity of the problems with which we must deal, or the ineluctable deficiencies of our system of governance.

The second proposition follows from the first. Since liberalism has come to the end of the road, U.S. political scientists *must* move to the right or to the left if they are to have any voice in the reconstruction of U.S. society. If and only if they do so can they hope to play a significant role in the debate between the "advocates of democratic socialism and state conservatism" that "may finally form the contours of future American politics" (p. 240).

Must we finally abjure "centrist liberalism"? Is there nowhere to go but to the statist-conservative Right or to the democratic-socialist Left? I do not know. But I do recall hearing, many years ago, the shattering news that God was dead and, in a probably unrelated communiqué, that we had come to the "end of ideology." Both pronouncements were, I think it fair to say, somewhat premature. So, too, it may be, with the alleged demise of liberalism.

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In their Acknowledgment, the authors give special thanks to Theodore Lowi without whose "charm, wit, tolerance, critical abilities and sheer enthusiasm . . . this book would never have been written." As indicated earlier, Professor Lowi has also contributed a Foreword in which, *inter alia*, he endorses the notion of a third tradition. He contends that "whether by coincidence, common causation or plan, modern social science and modern government have common origins in liberal thought" (p. xxix); and, coming to the immediate point, describes *Disenchanted Realists* as a "splendid and provocative book."

Although *splendid* is not a term that would have occurred to me, I do agree that the book is certainly provocative, both in its strengths and its weaknesses. The entire volume should be read by those concerned with the history of our profession; the final two chapters by those who, if less interested in where we have been, are curious as to where we now are—or might be going.

ALBERT SOMIT

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Citizen Demand-Making in the Urban Context. By Elaine B. Sharp (University: University of Alabama Press, 1986. xii, 211p. \$25.95).

Citizen contacts with municipal governments have both intrigued and puzzled political scientists in recent years. These contacts are intriguing as an apparently increasingly common form of political participation, but they are puzzling because they seldom follow the patterns observed on other forms of participation. Socioeconomic status, for example, cannot be counted on to have the same positive effect on contacting propensity that it has on other participation.

Prominent among those who have tried to solve the puzzle is Elaine Sharp of the University of Kansas. Now, as the culmination of a lengthy NSF-supported project, she has produced the best analysis to date of the causes and consequences of these contacts—or *demands*, as she terms them.

This is a carefully crafted, meticulous piece of research. One by one, Sharp analyzes each question about these contacts, translates the

question to operational terms, and presents and interprets original data she collected to answer the question. The manner of presentation is almost always clear and well written, thereby making complex arguments understandable.

Sharp is at her best when addressing the causes of these demands. She assesses the role of many possible causes, ranging from the standard variables of socioeconomic status, need, and awareness to the usually overlooked influences of centralized complaint units and neighborhood organizations. In the process, she suggests resolutions to many previously conflicting conclusions about the relative roles of these factors.

Perhaps the most interesting question Sharp attempts to answer is why political science should be concerned with these contacts at all. It could be argued, after all, that contacts focused on mostly minor service-delivery problems are politically trivial. In two concluding chapters, Sharp attempts to show why that is not the case.

She suggests, first, that these contacts can influence whether citizens stay in the city or move away. Evidence presented in the book indicates that dissatisfaction with the municipal response to contacts does significantly, if modestly, increase citizen inclinations to move out of the city.

Sharp is somewhat less persuasive in arguing, second, that these contacts may be important in a negative sense as a component of the supposed "demand overload" on urban governments. This speculation raises a number of interesting empirical questions. Is the increase in these contacts sufficient to constitute a dramatically increased demand? Has the increase translated to an increased demand on municipal resources? These questions are ignored in the book, however, in favor of theoretical speculation founded on a directed literature review. Although interesting, this speculation is less convincing than the book's data-based arguments.

That is a minor problem, however. The only major problem for the book may be finding an audience. Written in the manner of a sequence of journal articles, with each step in the research carefully explained and documented, the book represents a significant research contribution but not a contribution accessible to a broad audience, even within political science.

This is not the sort of book, for example, likely to be used in an undergraduate class, even as a supplementary text for advanced undergraduates. The book is important reading, on the other hand, for any faculty or graduate students who want to understand how citizens interact with local government.

JOHN CLAYTON THOMAS

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Back of the Yards: The Making of a Local Democracy. By Robert A. Slayton (Chicago: University of Chicago Press, 1986. xiv, 277p. \$22.00).

This book treats a largely immigrant, early industrial, working-class community that emerged in one of the world's most unpromising settings—the Back of the Yards. In this area, the workers in Chicago's meatpacking industry lived and worked from approximately the end of the Civil War until the 1950s when the industry began to move out of Chicago, culminating in the official closing of the Stockyards in the early 1970s. Meatpacking as a large-scale industry was one of the earliest and largest fully to rationalize the factory system. And within the industry, the Chicago plants were the earliest and largest to do so. By 1900, if not before, Armour and Company could boast, truthfully, that they used every part of the hog except the squeal.

But the minute division of labor that makes such rationalization possible (and profitable) extracted a high toll among those who labored there, particularly so where the labor force was relatively defenseless to begin with, as this one was. From Marx on, sociologists of quite different theoretical persuasions have documented the social pathologies attendant upon early, rapid, large-scale, industrialization. Of all these ills, the one that lies at the crux is social disorganization and anomic behavior, or, in the author's words, "the concept of community lost."

It is this theoretical convergence of both the Marxists and the non-Marxists and their agreement that the loss of community was and is the defining characteristic of a neighborhood like the Back of the Yards that supplies the author with his leitmotiv. He disagrees, and sets out to show "that communities did exist in urban

industrial immigrant districts, that bonds between people did form, and that institutions did persevere," "that residents of these inner city areas did form *communities* even by a rustic definition of that term" (emphasis original) (p. 8).

The book is, of course, a case study and the author is conventionally modest about the typicality of the Back of the Yards and consequently about the generalizability of his findings. He is too modest. The point to be asked about any empirical study is not, primarily, how the evidence was generated but rather whether the findings are theoretically interesting and relevant. At this level, there is no essential difference between a case study and any other mode of inquiry. And there can be little doubt that this study satisfies on that score. There are other advantages to the choice of this site as well. Many of the changes in which the author was interested have occurred within living memory and, accordingly, the text is enriched by a number of lengthy interviews (79, by my count). Because of the location and the time, there is a wealth of publicly available data that studies of early industrialization in other parts of the world have often had to do without. Finally, the Back of the Yards has been the object of scholarly scrutiny for a long time; most notably by the social scientists of the nearby (geographically if not socially) University of Chicago, who, as early as the turn of the century, were already treating the neighborhood as their own private laboratory.

The author makes his case that a community—or rather communities—in the full sociological sense of the concept of *community*, did emerge in these unlikely environs—and he makes it persuasively. Very early on, the immigrant workers recreated as best they could the social conditions and institutions they had left behind in Europe (the book concentrates on the middle period from roughly 1880 to 1920 when the immigration was overwhelmingly East European). Primarily these were based on church and family and therefore faithfully recreated as well the ethnic balkanization of Eastern Europe. Shared language and belief and very often even shared villages or districts of origin in the homeland facilitated the development of a sense of community in a strange and hostile environment. In addition, a network of other forms of association such as

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sport clubs or social clubs soon sprang into being. The churches were fertile sources of such activity. The author at one point counted 18 different associations with no direct religious purpose sponsored by one church. Neighborhood taverns and shops provided daily foci for socializing and socialization. In short, a rich mosaic of associational life that provided the social and psychological support for the people to assert their autonomy and establish control over crucial aspects of their lives soon emerged. Their struggles were not, of course, uniformly successful. In particular, the attempt to establish some degree of autonomy in a central aspect—the workplace—through unionization was, on balance, a failure until the late 1930s, in part because the packers manipulated the very ethnic divisions that in other ways served to facilitate the development of community.

There is, however, a parallel thesis to the one concerning the establishment of community. That is that the creation of community simultaneously involved the creation of democracy. Indeed the book is subtitled, *The Making of a Local Democracy*. And here caution is advised. For one thing, at the conceptual level community and democracy are by no means the same thing. Some—Burke, for example—have even seen them as antithetical. Yet the author at times confounds the two. We can agree with the author that the Back of the Yards sought, and in no small measure achieved, stability, predictability, and autonomy in the sense of freedom from external interference in many areas of their daily lives. But nomic, as opposed to anomic, behavior or autonomy, as opposed to dependency, can be realized quite apart from democracy. It must be shown, not assumed, that the creation of community entails the creation of democracy as well.

That demonstration is particularly necessary here. The two major institutions responsible for the development of community—church and family—are late nineteenth century, East European, and working-class. Rightly or wrongly, such structures have most often been characterized as strongly authoritarian and fundamentally antidemocratic. Second, two of the major external actors the author cites as helping to foster democracy, the unions and, somewhat later, the local Democratic party machine were not and are not internally demo-

cratic. The author says that the union, for example, extended democracy, "by organizing industrially, by company, rather than by craft." But how, one asks, does this extend local democracy? And the local political machine may have imbued the residents with the sense that "downtown" can be manipulated, but clearly it did so in the framework of a patron-client relationship rather than democracy.

Finally, one must remember that all of this takes place within a larger picture. And that larger picture is the overwhelming dependence of the community on the meatpacking industry with all the misery and degradation that entailed together with the overall failure of the workers to establish order and autonomy in this central aspect of life; failure, that is, until 1939, nearly 80 years after the community was founded and less than 20 years before the industry started to disappear from Chicago. Summing up, the book more than amply shows the emergence of a community in a setting where one would think it unlikely. It does not successfully demonstrate the emergence of local democracy in an equally unlikely setting, in part because the author assumes what needs to be shown—that community and democracy go together.

DON R. BOWEN

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Public Opinion and Collective Action: The Boston School Desegregation Conflict. By D. Garth Taylor (Chicago: University of Chicago Press, 1986. xiii, 241. \$29.00).

Reading this book brought back a lot of memories, many of them unpleasant: violence in the streets and schools of Boston; tawdry attempts by Louise Day Hicks, "Pixie" Paladino and John Kerrigan to exploit the desegregation controversy to advance their political agendas and careers; timid leadership by Mayor Kevin White, concerned about his own political skin; ineffective leadership by Cardinal Medeiros as he attempted to rally his flock, in this most Catholic of the United States' great cities, behind peaceful support of desegregation. Garth Taylor, I should hasten to add, does not attempt to exploit the sensational aspects of the Boston school-desegrega-

tion controversy. This book is a careful and skillful piece of work, written in the straightforward style suitable for reporting the results of social-science research. For anyone interested in how a city, once the cradle of liberty, nearly became its coffin, this book is most instructive.

Taylor begins with a review of racial ideology and civil-rights progress in the United States. By the time of *Brown v. Board of Education*, he concludes, U.S. citizens were prepared to jettison racially based doctrines of inferiority, social inequality, and mandatory segregation. Over the next 10 years, Taylor argues, there was widespread acceptance of the right of black students to attend desegregated public schools. In this process, the focus of debate shifted from whether mandatory segregation was wrong to whether mandatory desegregation was right. People were prepared to abandon the former but not necessarily to accept the latter. Orientations toward racial injustice came to be governed by what Taylor calls the "Doctrine of Voluntary Compliance," a belief that the wrongs of mandatory segregation are remedied best by voluntary desegregation. Taylor traces the roots of this doctrine back to Reconstruction and demonstrates how it can still operate as a powerful deterrent to mandatory desegregation.

The analysis is based mainly on five waves of survey data from a panel of 631 white adult residents of six neighborhoods in the city of Boston. The interviews were conducted over a period of approximately two years, beginning several months before Judge Arthur Garrity's order to desegregate the schools, and ending after the implementation of the first phase of the plan.

Taylor shows that opposition to busing and willingness to protest against mandatory desegregation are not merely the product of racial prejudice and intolerance. Adherence to the doctrine of voluntary compliance provided a frame of reference for citizens to view the insistence on mandatory desegregation as contrary to U.S. principles of popular consent and thus as unfair and unjust and potentially harmful in terms of safety and educational quality. Equipped with these "rationalizations" rooted in a pervasive popular belief, citizens became prime targets for collective mobilization at the hands of antibusing leaders whose own political prosperity was linked to the stridency and

duration of public protest. In telling the story of how a city with liberal and abolitionist traditions came to symbolize bitter and protracted opposition to school desegregation, Taylor shows how all the pieces of the puzzle came together: widespread belief in the unfairness and potential harm of the desegregation orders; aggressive antibusing leadership; a strong and resourceful organizational base for protest and a belief in its efficacy; limited resources for support, enfeebling the initial efforts of those charged with enforcing compliance with Judge Garrity's orders.

Although published in 1986, this book, based on research conducted in the 1970s, belongs very much to that decade. Like much research of that time period, its main focus is on white resistance to desegregation, and Taylor does an able job of relating his work to the research of that period. Perhaps in recognition of the somewhat dated quality of the work, Taylor attempts to draw some contemporary significance from the Boston case. In the final few pages of the book he notes that the federal-court desegregation orders produced significant changes for the better in the Boston public schools, improving management and curriculum, raising average daily attendance, and boosting dramatically the proportion of high-school graduates going on to post-secondary education. A few years after the disruptions that accompanied the first phase of implementation, the situation changed dramatically. Hicks, Paladino, and Kerrigan all suffered electoral defeat, the Boston school board came under the control of blacks and moderate whites, and, in 1985, the federal district court closed the case. While Taylor (reminiscent of the poet John Donne) seems to feel that the old order of mandatory segregation is dead but that the new order of a desegregated society is yet powerless to be born, an opposite conclusion can be drawn. A new order of school desegregation has indeed been born in Boston, and its benefits are being reaped by those who chose not to flee in protest but to remain in voluntary compliance.

EVERETT F. CATALDO

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Book Reviews: American Politics

Reagan's America: Innocents at Home. By Garry Wills (Garden City, NY: Doubleday, 1987. 472p. \$19.95).

Most political scientists who study U.S. politics and virtually all who study the presidency are likely to have read at least parts of one or more of the previous books in which Garry Wills combines history, biography, literary imagination, facile prose and self-assured personal judgements in accounts of such U.S. presidents as Washington (*Cincinnatus* [Garden City, NY: Doubleday, 1984]), Jefferson (*Inventing America* [London: Athlone, 1980]), Madison (*Explaining America* [Garden City, NY: Doubleday, 1978]), Kennedy (*The Kennedy Imprisonment* [Boston: Little, Brown, 1982]) and Nixon (*Nixon Agonistes* [New York: New American Library, 1979]).

Wills's latest foray is only partly about the current chief executive. Rather, as the possessive in the title indicates, it also comments on aspects of U.S. politics, society, and history that nurtured Ronald Reagan—for example, the small-town Midwest, the ethos of the Disciples of Christ (Reagan's mother's religious denomination), life in the Depression, and the myths and mores of Hollywood films and the film colony. But the book also delves extensively into Reagan and his career, refracting both Reagan and his America through the imagery of Mark Twain (hence the variant on *Innocents Abroad* in Wills's subtitle).

Any reader of this book who has not read Wills's previous work and expects to encounter meat-and-potatoes journalism of the sort to be found, for example, in Lou Cannon's *Reagan*, will be disabused before finishing the first paragraph of *Reagan's America*. Wills pronounces that Reagan must be understood as a *synecdoche* (a figure of speech in which a whole is described by naming an instance of it, e.g., "He is a Rockefeller" for "He is a millionaire"). Indeed, the political scientist who is unfamiliar with the Willsian mode is likely to dismiss the book as hopeless before completing that paragraph with its succession of fuzzy assertions of paradox: "The geriatric 'juvenile lead,' even as President, Ronald Reagan is old and young—an actor, but with only one role. Because he acts himself, we know he is authentic. A professional, he is always the amateur. He is the great American synecdoche, not only

a part of our past but a part of our multiple pasts. That is what makes so many of the questions asked about him pointless. Is he bright, shallow, complex, simple, instinctively shrewd, plain dumb? He is all of these things and more" (p. 1).

Why then should a reviewer ignore the book-review editor's instruction not to submit a review "if, after examining the book, you conclude it is not appropriate for *APSR* treatment"? For this reader, the reason is that by the end of this sometimes maddeningly digressive book (and in spite of its reiteration of the familiar theme that Reagan is a font of rhetoric, with little if any impulse to confront his rhetoric with evidence), I had a visceral sense of the character and roots of the fortieth president that I heretofore lacked—a sense of Reagan as a man, a sense not easily specified but that goes beyond what I learned from Cannon's scrupulously detailed biography.

Some of this sense has a cognitive basis. Wills has unearthed new information about Reagan's hometown. He interviewed people who knew Reagan in his early years, mined the accounts of him in 1940s film magazines and Hollywood gossip columns, and reassessed his participation in the Screen Actor's Guild and the biographical record of this film-actor-turned-politician.

Much of what led me to continue reading, however, was the book's imagery and its author's undocumentable interpretive flights. In the mode of a novelist or essayist, Wills stretches the reader's imagination. Although he would deride such social science usages as "heuristic," the fertility of Wills's conceits, along with the archness of his prose, provides this distinctly non political science-like analyst with his political-science following.

Moreover, some readers may have reason to believe that even when Wills's assertions are unsupported, they may be on the mark. Long ago I read and dismissed as clever fantasy Wills's account in *Nixon Agonistes* of a tough-minded, politically shrewd Dwight Eisenhower. Later I found myself opening the newly declassified personal papers in archives of the Eisenhower Library. What Wills put forward as unquestioned truth on the basis of no evidence was on the mark and he had been spared my own molelike burrowings through mountains of documents.

This book is vintage Wills. It is not as care-

fully assembled and closely presented as some of his earlier work. It is hard to say much new about Ronald Reagan at this point, and Wills's two purposes of discussing Reagan and discussing Reagan's America get tangled with one another. Still it is a safe prediction that just as many political scientists have found Wills's earlier books of interest, many will read or dip into this one. They will not find political

science but are likely to emerge with a more nuanced perspective on a president who needs to be deciphered in any effort to take stock of the continuities and variations in presidential leadership.

FRED I. GREENSTEIN

Princeton University

COMPARATIVE POLITICS

Capitalism and Public Policy in the UK. By Tom Burden and Mike Campbell (London: Croom Helm, 1985. vii, 269p. \$29.00).

In the authors' own words, this book "aims to provide an analysis of the nature, development and impact of public policy in the UK" (p. vii), and it attempts to do so from within an explicitly Marxist framework. Ambitiously, nine topics are covered, ranging from the more or less expected economic policy and "health and welfare" through the less studied "cultural policy" and "law and order," concluding with a look at "foreign and defence policy." Each of these chapters is clearly organized, beginning with a section outlining the relevant theoretical concerns, after which the course of British public policy in that area is outlined. *Policy* is generally taken to mean "legislation," with special reference to the post-World War II period. Finally, the chapters conclude with a brief section that attempts to link the pattern of acts back to the Marxian model. In very general terms, the argument presented is a familiar one: the state pursues policies (passes laws) that are in the interests of the capitalist class, and particularly the financial (city) interests of that class. The succession of acts, then, can be understood as a means by which such processes as "accumulation" and "legitimation" are made more secure for capital.

Despite the fact that there is little (if anything) that is new in this argument, such an approach does come as an antidote to the pluralist conceptions of the state, which often view the state as a neutral arbiter of interest-group demands. At the very least, a Marxist

approach gives pause for thought to the pluralist "state-as-referee" idea.

Valuable though this insight may be, however, its worth hinges on an ability to demonstrate evidence for the argument, a point especially critical when there are important problems of falsifiability, as is often the case with Marxist approaches to public policy. Unfortunately, Burden and Campbell, while able to present a redescription of many pieces of legislation in terms of their capitalist interest, do not tackle, but only court, these problems. The "capitalist interest" seems to cover many eventualities and aspects, and can be defined and redefined in such ways as to ensure that every government act, without exception, was in the interests of capital in at least one sense: if not accumulation, then legitimation; if not in the short term, then the long term; if not for capital as a whole, then for some "fractions" of capital. The latter device does open up some interesting room for discussion. After all, why should successive British governments be so slavishly devoted to financial interests at the expense of (job- and vote-providing) business interests? Rather than being a starting point for analysis, however, such questions are an end point: they are not pursued and are rarely in any form amenable to hypothesis testing.

At some points the authors do present opinions in what may be termed *testable* form. For example, "The worse the condition for profitable accumulation, the greater the 'needs' of capital, and the more coherent their articulation, then the less the strength of labour and the less is the need to buy consent" (p. 73).

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Sadly, these all too rare moments do not result in any attempts to test out the hypotheses. Here the book's content does not follow its systematic form, and in general there is no direct demonstration of the links between policy outcomes and capital, only the repeated assertion that such and such a policy is consistent with some flexible definition of the capitalist interest. More detailed and careful studies of the relationship between business and policy, such as those conducted by Moran, Grant, or Marsh tend to reveal that the situation is usually much more complicated and uncertain. Often there is little evidence to support the view that the strings of politicians are being directly pulled by businessmen.

All in all, readers will find very little here of immediate use in studying British public policy; more praiseworthy and useful studies do exist in which at least some of the same conclusions may be reached with a much more parsimonious theoretical underpinning. Having said that, however, the emphasis on the importance of capitalist interests and of a state structure that is not politically anemic are important points. At the very least, they demand some kind of response from those students of policy who rely on such diverse analytical foundations as electoral incentives, the importance of trade unions or the impact of national styles. Despite the fact that many political scientists would agree with Lindblom's assertion that there is a "privileged position" for business, all too few seem to wish to see how this works out in practice and in relation to other, competing, explanations.

SHAUN BOWLER

Washington University

National Politics and Community in Canada.

Edited by R. Kenneth Carty and W. Peter Ward (Vancouver: University of British Columbia Press, 1986. 200p. \$14.00).

Six historians and four political scientists combine in this collection of essays to argue for a reinterpretation of the evolution of Canadian politics and the Canadian political community—or, perhaps more precisely, to call for a return to an older tradition. Two themes dominate. The first is substantive. Canadian scholars, the editors argue, have

been too preoccupied with "the part and not the whole" (p. 1)—with the weakness of the national political community and its institutions, with regional and linguistic cleavages, and with "province building" and the decentralized character of Canadian federalism.

Such preoccupations, they argue, underestimate the "primordial" strength of the national political community and the role of institutions in fashioning and maintaining it out of "the pieces of Britain's North American Empire" (p. 1).

The methodological theme, therefore, is "state centered," emphasizing the role of political parties and their leaders in using the resources of the state, primarily those of patronage, to build networks of interest and loyalty: "The national community was explicitly political in early Canada, where party constituted the principal tool of pragmatic nation-builders" (p. 7).

This nation- and state-centered approach is indeed an important corrective to recent trends of interpretation in Canada. Several of the authors elaborate it persuasively—Gordon Stewart with an excellent analysis of the foundations of elite culture and style in pre-Confederation Canada; the editors in their analysis of the changing definitions of citizenship through manipulation of electoral laws; P. B. Waite in a lively description of the role of spoils and jobs in reconciling the reluctant entrants Nova Scotia and New Brunswick to Confederation; Margaret Prang in an insightful analysis of the development of a national "public culture" through the emergence of nationwide business and religious elites; David Smith on the nation building and integrative strategies of the Liberal party in alliance with the national bureaucracy; and John Courtney on the role of national party conventions.

But the nation-centered thesis turns out to be hard to sustain and is indeed contradicted in many essays. As Stewart concludes, Sir John A. Macdonald's "calculating, comprehensive and thoroughly partisan" use of patronage to strengthen the national government ultimately failed because it did not foresee that "the new provinces could act as counterweights to the seemingly limitless power of the party that controlled the Commons" and because Canada "was too extensive geographically, too diverse ethnically, too fragmented economically, and . . . had no national ruling class to sustain

Macdonald's vision of central power" (pp. 42-43). This theme recurs throughout the essays. Thus, David Smith traces a sharp decline in the integrative capacity of political parties in the face of province building, the rise of policy concerns in provincial jurisdiction, and the importance of intergovernmental relations. Christopher Armstrong elaborates on the last point with an analysis of the growth of federal-provincial conferences as regional brokerage mechanisms displacing parties. Donald Blake shows how sharply divided national and provincial party systems are in British Columbia, while calling for a more aggressive assertion of federal power into the provincial-policy arena. John English shows that one integrative mechanism—the role of "Quebec lieutenant" for an English-speaking prime minister—has rarely been successful. Prang concludes that the importance of provincial governments, increased ethnic diversity, and increasing Canadian linkages outside the country have eroded the networks that earlier contributed to the "nationalizing of sentiment." Carty and Ward themselves conclude their analysis of franchise laws by noting the "conflicts about the essence of Canadianness that lie at the heart of the political system" and the "lack of agreement on what constitutes a Canadian" (p. 77).

Thus, the book may be read less as a celebration of the success of nation building and more as a commentary on its relative weakness. Perhaps the key is that state building on the basis of the efforts of pragmatic, vote-maximizing, elitist politicians deploying the patronage instruments of the modern state is insufficient. It requires other qualities as well—and more attention than these authors pay to underlying social, economic, and ideological forces. Nevertheless, collectively, this is a stimulating, well-written, and useful book.

RICHARD SIMEON

Queens College

Corporatism and Political Theory. By Alan Cawson (Oxford: Basil Blackwell, 1986. viii, 174p. \$45.00).

Twelve years after the publication of Schmitter's article on the "century of corporatism," his approach seems to have become

something like the "orthodox" conceptualization of *corporatism*. As a synthesis of the recent debate, Cawson's book is, before all, a systematic exposition of this orthodoxy and a defense against the counterattack of the "pluralists." Cawson points out that *corporatism* does not necessarily supplant the pluralist "paradigm" but rather supplements it. This involves a restrictive definition of *pluralism* as an ideal type with a limited scope of applicability.

The other challenge to the emerging "orthodoxy" of corporatist theory is, of course, the structuralist version of neo-Marxism. Here the central issue is the concept of the state. Cawson places the corporatist concept of the state clearly into the Weberian tradition. In this perspective, in fields of welfare-state policies where the power base of the bureaucracy proves not sufficiently effective, state agencies enter into exchange relationships with "organizations representing monopolistic functional interests."

Compared to earlier writings of the corporatist school, the major shift in emphasis in Cawson's synthesis emerges in his distinction of three levels—macro-, meso-, and micro—of corporatism, according to the "scope of interest organizations" involved. Macrocorporatism intermediates interests aggregated on a class basis in peak organizations, and the level of state organizations involved is the central government of the nation-state, which has broad economic management as its "policy focus." Under this rubric, Cawson reports some of the cross-national studies of the impact of corporatism on policy outcomes and discusses Austria as an "ideal type." He presumes "that macro-corporatism is characteristic only of a few small countries, and that it has flourished especially in the context of economic growth in the post-war period" (p. 104). This, however, does not warrant the conclusion that the concept of corporatism is only of limited interest. But the specific category of *macrocorporatism*, if I understand the author's intentions, is of little use as an analytical category.

For Cawson, the potential is greater for *meso-* and *microcorporatism*. The latter he defines as "collaborative policy-making between a single firm and a state agency" (p. 76), distinguished from "agency capture" through industrial interests by a greater degree

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of "autonomy" of the agency (p. 121). The subsumption of such collaboration under the concept of *corporatism* is justified by the argument that large firms "aggregate" interests as well as organizations do. But how can this be reconciled with Cawson's definition of *corporatism* in terms of "organizations representing monopolistic functional interests" (p. 38). And whether there is anything on theoretical grounds distinctive about this subcategory remains obscure.

Obviously, the author's main emphasis is on *mesocorporatism* where, as he points out, organizations represent "sectoral as opposed to class interests" (p. 72). Cawson does not single out any countries as being specifically "mesocorporatist"—although he indicates that Britain has meso-, but only weak macro-corporatism (p. 136). The organizations at the level of sector—"be it industrial sector, public sector, consumption sector or whatever" (p. 73)—have restricted domains. Again, the author's ideas about what is theoretically distinctive in mesocorporatism are somewhat elusive. The "levels" of corporatism appear to be characterized by the predominance of specific policies. What is discussed in relation to the mesolevel is social and welfare policy on the one hand (p. 115) and industrial policy on the other (p. 113).

The latter is said to be "sectoral by nature" and to arise "when macro-economic instruments . . . fail to discriminate sufficiently between the different needs of various branches of industry" (p. 113). Thus, we see here the emergence of industrial restructuring as "negotiated sectoral policy" (p. 114). An important point seems to be that, whereas class-based macrocorporatism tends to be based upon "growth coalitions" depending on favorable economic circumstances, mesocorporatism is the form of "state intervention in industrial restructuring under crisis conditions" (p. 87).

The author thus seems to suggest that "macrolevel" corporatism is, first, a "fair-weather phenomenon" and, second, operates mainly through macroeconomic policy, whereas mesocorporatism is a crisis phenomenon and emphasizes sectoral industrial restructuring. Now it is certainly true that in some of the earlier contributions to the debate, corporatism was indeed discussed on the national (macro-) level and mainly associated

to macroeconomic policies, in particular to incomes policy. (This was also the earlier perspective of this reviewer.) However, since then we have learned that in countries like Austria and Sweden, industrial policy and labor-market policy were central issues in the process of nationwide (and "class-based") "concertation" of peak organizations (and this already under conditions of economic growth). Hence, *macrocorporatism* should not be equated with negotiated "macroeconomic" policy. On the other hand, it is difficult to see that sectoral industrial policy is something specific to crisis. After all, the sectoral Economic Development Councils ("Little Neddies") in the UK had been established already in the sixties. The author wants to analyze "the impact of economic changes on corporatism" and to demonstrate that "what is involved is a shift in the level of corporatist practice, rather than an increase or decrease of corporatism as a whole" (p. 125). This may be an interesting hypothesis and a plausible one, at least as far as Britain is concerned. But it would be more convincing if the alleged correspondence were demonstrated with the aid of measurement (of "economic changes" or "shifts in level") and tested for cross-national validity.

GERHARD LEHMBRUCH

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The Faces of Justice and State Authority: A Comparative Approach to the Legal Process. By Mirjan R. Damaska (New Haven: Yale University Press, 1986. xi, 247p. \$26.00).

Legal systems are often classified under rubrics like *Anglo-American* and *Continental* or *adversarial* and *inquisitorial*. Mirjan R. Damaska, a professor of comparative law at Yale Law School, reveals the inadequacy of such labels for cross-cultural study of the legal process. He offers instead an analytic framework that not only illuminates the world's diverse judicial systems but, more important for political scientists, demonstrates the impact of political ideas and institutions on legal structures and practices.

Damaska examines the administration of justice in terms of both the structure and the function of government. His structural focus

produces two competing "ideals of officialdom." The *hierarchical* ideal, similar to the classic model of bureaucracy, reflects the centralized authority of Europe; whereas the *coordinate* ideal resembles the decentralized authority of the United States. These models help shape a nation's judicial process. In hierarchical regimes, the legal system entails specialized stages. Information is collected gradually and elaborate files are developed as cases ascend the procedural ladder. Authorities control proceedings strictly, allowing little role for litigants and their attorneys. Appellate review is routine before a decision is final. By contrast, legal proceedings in coordinate states are less integrated, testimony is oral rather than written, authorities delegate considerable procedural responsibility to actors outside the organization, and review from above is limited.

Governmental functions likewise affect the administration of justice. In *reactive* states, where government seeks only to maintain a stable order in which individuals are free to manage their own affairs, the legal system mainly resolves conflicts between competing parties. *Activist* states, on the other hand, limit individual freedom in order to mold society to a particular vision of the best order. Here justice facilitates implementation of state policies. While *conflict-solving* and *policy-implementing justice*, like *coordinate* and *hierarchical authority*, sound like sophisticated names for adversarial and inquisitorial proceedings, Damaska shows that they are independent of this traditional dichotomy because they influence both kinds of proceedings.

Combining these visions of authority and justice, the author surveys past and present legal systems to illustrate "the ways in which procedural features attributable to different goals of justice interact with features rooted in the character of officialdom" (p. 181). His conceptual scheme is particularly useful for understanding recent changes in the U.S. judicial process, such as the rise of public-interest litigation. "Much as the *functions* of the contemporary American state are a mixture of activist and reactive impulses," Damaska argues, "so the actual *structure* of the state apparatus—especially the administration of justice—is a hybrid of bureaucratic and pre-bureaucratic forms" (p. 234).

Nevertheless, he understates how bureaucratic our legal system is (pp. 44–46, 231–34). In terms of decision making as well as administration, the U.S. legal process has become a large-scale, complex organization displaying many of the hallmarks of Max Weber's model of bureaucracy: shared values, beliefs, and expectations; loyalty to the organization and its goals; consensual decision making by mutually dependent and supportive work groups; routinized operating procedures; and secrecy and hostility toward outsiders. A good example of this bureaucratization is the collaboration among police, prosecutors, defense attorneys, and judges in criminal cases.

Although the book makes surprisingly little use of political-science research, given its recognition of the importance of politics in understanding law, it is nonetheless a novel exploration of the connection between justice and state authority. It places much of what we already know about the administration of justice in a fresher perspective while identifying patterns and suggesting meanings not appreciated before. Damaska's elegantly written and flawlessly organized work is therefore a valuable contribution to comparative analyses of the legal process, showing that study of the relationship between law and politics is far from exhausted.

GERARD J. FITZPATRICK

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Political Socialization in the Arab States.

Edited by Tawfic E. Farah and Yasumasa Kuroda (Boulder, CO: Lynne Rienner, 1987. xiv, 215p. \$25.00).

Jo-Ann Hart's valuable introduction to this collection of survey essays ends with Roberta Sigel's self-criticism that *political socialization* "is a misnomer for what we study because we study what children have learnt . . . not how they have learnt it" (p. 15). The great majority of these essays have not fallen into that trap. This leaves the reader with both an interesting and relevant source, because in the Middle East the function of political-socialization agencies is more that of system creation than of system maintenance. However, in the Middle East, system creation and political legitimacy are inherently linked to the ongoing

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effort to create a common communal association and national identity. These communal associations have taken two "modern" forms since the collapse of the Ottoman Empire, those of state and Arab nationalism. "Pan-Arabism," as much a socioeconomic rallying point for the disenfranchised under the old regimes as it was an antiimperialist and anti-Israeli nationalist movement, threatened "parochial" state nationalism under much of Egyptian president Nasser's rule but faltered from the mid-1960s onward until it was debilitated by the debacle of the 1967 war and Nasser's death in 1970. The Arab state, sometimes no more than a dynastically and or bureaucratically led collection of religious and ethnic groups, families, and tribes, survived much of the Arab system, but since 1979 it has come under a new ideological assault from Shiite Islamic fundamentalism.

Identity, personal or national, is a dynamic process and the history of a people's self-identification is the history of both their objective situation and their self-conception within that situation. The best articles in this collection attempt to identify the most relevant socializing agents in Arab society and, at the same time, place the respondents, families, schools, peers, and national institutions within the region's political context. These include two focused on Kuwait: one by Tawfiq Farah and the second coauthored by Farah and Faisal S. A. Al-Salem. These examine the issue of efficacy and succeed in decoupling the theoretical link between interpersonal trust and trust in an individual's government and political system. Trust in a system, they persuasively argue, is not a subjective or psychologically induced trait but emerges, when it does, from experience and successful interaction with the system. While traditional social units, such as the family, frequently provide the required access to the material and status benefits that the government provides in Kuwait, the result is that for male Kuwaiti citizens of all classes, the system is responsive due to the small size of the population and the wealth and the welfare-nature of the system.

This "instrumental" approach to political socialization is also found in Mark Tessler and Linda Hawkins's study of Tunisia. Tessler and Hawkins find some of Joel Migdal's earlier conclusions (*Peasants, Politics, and Revolution* [Princeton: Princeton University Press, 1974],

12, 20) an aid in understanding the oft-found contradictions in surveys regarding modernization. One is that "individuals often change some patterns and commitments, but not others" and the other is that "self-interest largely determines whether an individual accepts or rejects new cultural patterns to which he is exposed" (p. 108). The results of the Tunisian survey lead the authors to the conclusion that "If acculturative experiences [including education] are unaccompanied by increased socio-economic status, they do not lead to variations in psychological or political outlook" (pp. 117-18). However, these findings tend to differ from the results of the studies in the Gulf States in one important respect. While the respondents in Tunisia who experienced greater socioeconomic mobility changed their political attitudes in the direction of greater "participant citizenship," the surveys of the Gulf States found that the family and religion—and not the state—remain the prime foci of loyalty even for upwardly mobile males.

This forces Ahmed Dhaher to conclude from his survey in "Culture and Politics in the Arab Gulf States" that while the material and ecological setting has been rapidly changing, the Arab mind has not "kept pace": "The students in the sample have come to expect modernity in their materialistic needs while maintaining a traditional mental outlook, apparently unaware of the contradiction" (p. 76). However, young Gulf women and a majority of respondents of both genders from Bahrain tended to be much more highly politicized in general and more closely identified with the "liberation of Palestine" than the aggregate, leading Dhaher and Maria Al-Salem (in "Women in the Gulf") to speculate that those individuals who see themselves as most repressed in their own social setting have had their consciousness raised to the point of at least verbalizing a commitment to the liberation of other Arabs.

The two surveys on Palestinian identity and political loyalty indicate that traditional social structures have the potential of serving as the vehicle for modern political mobilization and activity. Tawfiq Farah's "Learning to Support the PLO" and Rosemary Sayigh's "Sources of Palestinian Nationalism" should be read together. While very different in approach and representative sample, each tends to find the family as the major socializing agent for

Palestinian consciousness and identity among the young, rather than the PLO-run schools or the respondents' peer groups. One might theorize that due to the stateless nature of the Palestinian people, Yasir Arafat (the personification of the national expression), the family, and the "homeland" have become linked in the Palestinian psyche.

One overall criticism of this collection is that many if not most of these surveys took place at the earliest stages of, or prior to, the 1979 revolution in Iran. This event was riddled with nationalist, religious, and socioeconomic dimensions and has greatly influenced the altogether unresolved issues of identity and legitimacy throughout significant parts of the Arab world. An updated series of surveys can easily rectify this shortcoming.

STEWART REISER

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Decolonization and the State in Kenya. By

David F. Gordon. Special Studies on Africa (Boulder, CO: Westview, 1986. 266p. \$21.00).

In strictly economic terms Kenya has to date been among the most successful African countries during the first postcolonial generation. At the same time, after having survived one of the more traumatic colonial experiences among African former colonies, Kenya has been among the most open to foreign and domestic private investment of any country on the continent. The conventional wisdom among "mainstream" economists has been to see Kenya's economic progress as a direct consequence of its policies and to say to other economically struggling African countries, "Go thou and do likewise." Political economists of a more radical persuasion have seen Kenya's fate as the result of the changing character of international capitalism, disagreeing on whether this has (Colin Leys, *Underdevelopment in Kenya* [Berkeley: University of California Press, 1975]) or has not (Nicola Swainson, *Development of Corporate Capitalism in Kenya* [Berkeley: University of California Press, 1980]) produced dependency. They agreed in challenging earlier analyses emphasizing the salience of major local political developments and responses to them (e.g.,

the Mau Mau Emergency) in shaping Kenya's future course (e.g., Carl Rosberg and John Nottingham, *Myth of "Mau Mau"* [New York: Praeger, 1966]; George Bennett, *Kenya, a Political History* [London: Oxford University Press, 1963]; and George Bennett and Carl Rosberg, *Kenya Election* [New York: Oxford University Press, 1961]). Those centering on the conjunction of land-transfer schemes and political decolonization differ over whether local European-settler economic interests did (John Harbeson, *Nation-Building in Kenya* [Evanston, IL: Northwestern University Press, 1973]) or did not (Gary Wasserman, *Politics of Decolonization* [New York: Cambridge University Press, 1976]) dominate the transfer of power and significantly shape Kenya's future. Underlying these debates is the question of how much relative autonomy the colonial regime enjoyed and which constituencies were most effective in limiting that autonomy in their own interests.

David Gordon's *Decolonization and the State in Kenya* is a refreshing new contribution to this ongoing debate. His thorough and balanced review of the rise and demise of the Kenya colonial order facilitates, if it does not resolve, the foregoing debates. Gordon's argument is that the colonial "state," if that's what it was, was too "diverse and ambiguous [a] collection of parts" to have dominated, or been dominated by, any single interest or underlying economic trends or to have dominated the new regime after its formal demise. There is ample room, he believes, for the play and interplay of political strategy in policy formation and execution.

Gordon builds a strong case for the importance of the political that should be taken to heart by economic and structural determinists in our midst. Yet the "indigenization of control," both economic and political, does not by itself, in my view, disprove the Kenya-as-neocolony case. The problem centers on the question of "control," the many complexities of which it is beyond the scope of Gordon's book to explore fully. Neocolonialism is not incompatible with such processes, nor is it incapable of reappearing in new forms as societies appear to change. At the same time, Gordon's long historical view of the interplay of political economic developments is important. That same perspective applied to the first quarter century of Kenya's independence will

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show, I think, that neocolonialism is not forever present, a point that Kenyans have begun and will continue to demonstrate.

JOHN W. HARBESON

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Forming Economic Policy: The Case of Energy in Canada and Mexico. By Fen Hampson (New York: St. Martin's Press, 1986. x, 161p. \$27.50).

Hampson uses energy policy in Canada and Mexico to demonstrate how and when governments act in the long-term national interest, a behavior contrary to the pluralist view that elites respond to "micropolitical" pressures. According to the "statist" model, perceptions of crisis mobilize leaders to create policies meant to preserve stability and legitimacy at the risk of alienating important societal groups.

The price and availability of energy was perceived widely as a crisis in the 1970s. Canada and Mexico joined other governments in seeking policies to cope with problems of security and equity. Both faced the trade-offs between blessings and costs of expanding domestic production in response to rising world oil prices. They shared the opportunities and problems of land borders with the United States and its oil deficits. Was there "politics as usual" in these two countries? Did the state act autonomously to formulate policies to reverse the damaging effects of past concessions to micropolitical pressures?

In Mexico, the problem with oil lay in the rapid acceleration of production that was meant to capitalize on the rising world price of oil. While success in oil production and exports fostered economic growth, there were the destabilizing effects of drawing scarce capital, increasing the foreign-debt service, milking resources from other sectors, and fueling inflation. President Lopez Portillo capped PEMEX production in 1979 in order to reverse these trends. The effect of this policy was short-lived, as world oil prices fell in the 1980s.

Unlike Mexico, Canada was incapable of being a net exporter of oil. Conflict centered on eastern Canada's vulnerability to rising imported-oil prices and western Canada's frustration over being denied the producer's right

to sell at market prices. In the National Energy Program (NEP) of 1980, the Liberals ended an oil-price freeze, forcing higher gasoline prices on eastern consumers, and also imposed a wellhead production tax to capture revenues for a strapped federal government. The "made-in-Canada" price fell victim in the 1980s to the downward reversal in oil prices, making a judgment on the NEP difficult.

Acute crises resulted, the author argues, from concessions to parochial interests. In Mexico, decisions to expand production and shelter prices were meant to hold consensus among the dominant elements—labor, business, agrarian—of the revolutionary coalition. In Canada, the Liberals wanted to avoid alienating pivotal constituencies in central Canada by controlling oil and gasoline prices.

These two cases show, Hampson argues, that pluralist and statist theories are actually complementary. Parochial interests usually prevail, but under conditions of widely perceived crisis, elites rise above the pressures to secure the national interest. The author acknowledges a weakness of this approach. Retrospective analysis allows the observer to define a crisis and describe cause-and-effect circumstances.

The paradigm also does not allow the reader to independently categorize policy that is merely the constellation of private, parochial interests and policy that meets an unequivocal definition of the national interest. In the case of the NEP, some critics have argued that its publicized features of security, fairness, and efficiency were transparent as energy policy and that it was little more than a confirmation of the Liberals' view of the virtue of economic centralization. "Canadianization" of the oil industry was intended to help secure the Liberals' electoral base in central Canada. In Mexico as well, the same arguments about national salvation made in defense of Lopez Portillo's decision to cap PEMEX production could as easily be applied to the previous decision to mobilize production—the action that supposedly produced the crisis.

The statist model does not, in practice, provide a useful distinction between the objectives of the "state" as the embodiment of sovereign interests and those of the government as the resultant vector of the society's dominant interests. In Mexico, what distinguishes the preservation of the interests of the state from

the interests of the revolutionary coalition? In Canada, to what extent was the NEP the ideological vision of senior civil servants, and to what extent pure power politics?

Notwithstanding these reservations, Hampson's book is a good effort that calls attention to the need for more studies to explore the convergence of statist and pluralist interpretations.

LAUREN S. MCKINSEY

Montana State University

Guyana: Politics and Development in an Emergent Socialist State. By Kemp Ronald Hope (New York: Mosaic, 1985. 136p. \$19.95, cloth; \$12.95, paper).

Trinidad and Tobago: Democracy and Development in the Caribbean. By Scott B. MacDonald (New York: Praeger, 1986. ix, 231p. \$31.95).

Revolutionary Grenada: A Study in Political Economy. By Frederic L. Pryor (New York: Praeger, 1986. xx, 395p. \$45.00).

Militarization in the Non-Hispanic Caribbean. Edited by Alma H. Young and Dion E. Phillips (Boulder, CO: Lynne Rienner, 1986. ix, 178p. \$20.00).

The case studies of Guyana, Trinidad and Tobago, and Grenada by Kemp Hope, Scott MacDonald, and Frederic Pryor, respectively, and the contributions in the volume on militarization edited by Alma Young and Dion Phillips provide a variety of assessments on the nature of the problems faced by the non-Hispanic Caribbean since the attainment of independence and how different governments have responded to them. In one form or another, the four books focus upon the forces, both external and internal, that affect the chances for countries in the region to preserve and maintain democratic practices while at the same time formulating and implementing effective programs of economic development.

There is a general consensus by all the authors as to a growing militarization in the region as a whole and in the individual countries in response to domestic political concerns for order, stability, and regime survival and as a result of the intensification of international Cold War contentions in the Caribbean and Central America. There is general agreement

that militarization can have a profound negative effect upon the chances for sustaining democratic practices or of achieving self-sustaining economic development.

From Hope's case studies of Guyana and Pryor's of Grenada, the adoption of "Third World socialism" appears to bring with it the certainty of economic crisis and stagnation as well as kill any chances for the preservation and maintenance of democratic political practices. Pryor does not see these outcomes as inherent in socialism per se. Rather, the explanation has to do with how this ideology has become fundamentally and aberrantly altered in its transposition to the Third World. In his fascinating and highly convincing analysis, he concludes, with reference to his own set of economic, political, and socio-cultural criteria, that, like Grenada, most Third World countries identified as Socialist would rank very low on a Socialist scale. The exceptions to these criteria are in their adoption of the vanguard party, in their international support for Socialist nations, and in their ideological rhetoric. These three characteristics, he seems to be saying, constitute the entirety of the phenomenon of Third World socialism.

In his case study of Guyana, Kemp Hope provides an example of how these three elements can easily be employed to ensure and underwrite regime survival. There, the notion of the vanguard party was used to secure the centralization and personalization of power, to underwrite the conversion of the state administration into an institution of patronage, and to justify the development of a system of coercion and control. In addition, both ideological rhetoric and a foreign policy based on international support for Socialist nations were opportunistically employed to elicit economic assistance and strategic political support from the Eastern bloc and from progressive Third World nations.

What occurred in Guyana constitutes the essence of what Pryor calls "foreign-aid socialism" characterized by a complete dependence upon foreign grants and concessionary loans. The intention of those adopting this form of socialism is to employ such loans and grants for promoting economic development and supporting efforts at structural change.

The case of Grenada highlights the problems that inhere in such a strategy. Pryor shows

how the Grenadian leadership became preoccupied with establishing their "Socialist credentials" in the international arena while lacking a fundamental and sophisticated understanding of socialism itself. This invariably leads to the failure of developmental planning, an outcome also observed in Guyana by Hope, who attributes it to ideological dogmatism in the Guyanese government's approach to development. For Pryor, however, such failure has to do with the inability of governments adopting foreign-aid socialism to devise a consistent and integrated development program. This is because of their absolute reliance upon a foreign-assistance package, the contents of which are entirely determined by what foreign donors are willing and able to provide and which constantly change over time. To this is added the problem, endemic to all Third World countries, of the severe shortage of skilled personnel with the requisite levels of training and administrative acumen to implement a program of Socialist economic development. In Grenada, the inevitable developmental failures were hidden by an effective propaganda campaign directed at the domestic population, and more successfully at the international, particularly Socialist, community.

Pryor rejects almost entirely any suggestion of foreign meddling in the affairs of Grenada as an explanation for the political and economic crises of the Peoples Revolutionary Government (PRG). By contrast, the contributors to the volume on militarization, with the exception of the Seduc-Dahlberg article on Surinam, emphasize the realities of U.S. regional hegemony and the intolerance, particularly of its present administration, for any progressive regimes in the region. Such an emphasis is also evident in Hope's case study of Guyana. He suggests that the failure of the Guyanese regime to recognize this reality has distanced it from the "new Caribbean leadership." It is this very failure that explains the economic and political crises affecting that country.

In the volume edited by Young and Phillips, Boodhoo sees the escalation of militarization during the PRG regime in Grenada as the continuation of a trend toward state violence that began in the late 1940s. The dramatic military buildup after the regime came to power had, as its initial cause, the undoubted fear of U.S. military intervention or U.S. covert support for Eric Gairy, the ousted former prime

minister. It was this fear that drove the PRG to make military agreements with the Soviet Union, Cuba, and North Korea thus driving the country much more closely into the arms of the Communist bloc.

The growing militarization in the region is related, by all the contributors to the volume, to the erosion of democratic institutions. The contributions by Darns on Guyana and Seduc-Dahlberg on Surinam both deal with the use of the military as the primary prop to increasingly unpopular regimes. In the case of Grenada, according to Boodhoo, the PRG's program of militarization and its failure to hold elections contributed to its eventual collapse. In the introductory article by Phillips and Young, in the latter's article on Belize, and in the overview article by Watson, the source of the threat to democracy is clear: the efforts by the United States to assert or reassert its hegemony in the region. To this is added the fear of mass radicalization by the current crop of Caribbean leaders in the face of an escalating economic crisis. The renewed willingness of the United States to prop up pro-U.S. regimes and to provide them with military assistance and the threat to what Watson sees as *petit bourgeois* domination are acting in tandem, according to him, to "Central Americanize" the Caribbean. By this he means the proclivity in that region for right-wing, U.S.-sponsored, authoritarian regimes. A variation on this theme is provided in Alma Young's contribution on Belize. There, growing militarization has been sponsored by the United States in view of what it sees as the strategic importance of the country in the politically volatile Central American region.

What are the necessary prerequisites for democracy in the non-Hispanic Caribbean? Scott B. MacDonald attempts to provide an answer by examining the "successful experiment" of democracy in Trinidad and Tobago. This he attributes to the emergence of a dominant and politically unified middle class, which provided the basis for the establishment of a system of state-directed capitalism. MacDonald sees episodes of lower-class mobilization as anathema to democracy if only because they lead to racial fragmentation and are rooted in radical ideology. He traces middle-class moderation to "acceptance of European social attitudes" and, more recently, to the importation of middle-class ideas as a result of

the military presence of the United States in the country between 1941 and 1960. MacDonald thinks that the two challenges to Trinidad's democracy are lower-class radical mobilization and the breakdown of middle-class consensus. These two combined in the early seventies to produce an authoritarian tendency that was turned back in the wake of the post-1973 oil boom that laid the framework for the strengthening and consolidation of "democratic capitalism." The question that needs to be answered is why did middle-class domination result in democratization in Trinidad and Tobago when, from the accounts of all the authors considered, it appears to be the crux of the problem of militarization, underdevelopment, and growing authoritarianism throughout the region?

PERCY C. HINTZEN

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Black Politics and Urban Crisis in Britain. By Brian D. Jacobs (Cambridge: Cambridge University Press, 1986. viii, 227p. \$44.50).

The title of this book promises a comprehensive analysis of the role of Asian and Caribbean people in political action and protest in British cities. Jacobs dismisses on various grounds most extant writing on the political participation in Britain of people of Asian and Caribbean origin. He sets himself the task of mapping the pattern of political activity of, the parameters of political debate amongst, and the impact on public policy (particularly urban questions) of, this section of the British population (p. 6). His central claim is that the leaders of Asian and Caribbean communities have been slowly integrated into various forms of cooperation with local and national governments as the latter have attempted to execute policies intended to reverse the disadvantaged position of Caribbean and Asian people, particularly in Britain's urban areas.

Jacobs makes a virtue of his descriptive intentions but thereby lapses into empiricism. Uncritically, he utilizes the "race relations" problematic and abstracts "black politics" from its class context, this being legitimated post hoc by his claim that he is concerned with policy rather than theoretical issues (e.g., p. 5). It is mistaken to limit the conception of

political activity of Asian and Caribbean people to include only forms of activity within or exclusively on behalf of the Asian and Caribbean communities, although this conception follows from regarding these populations as shaped by, and only concerned with, "race."

Jacobs is therefore silent about Asian and Caribbean electoral behavior, trade union membership and participation, involvement in industrial disputes, and political-party membership and participation. An integrated analysis of these forms of political activity would have allowed him not only to realize his stated objectives but also to comprehend theoretically the process of political integration he describes. There is no reason to doubt that a process of political incorporation intended to achieve, or resulting in, the neutralization of dissent is occurring in Britain, but its scale and significance require a more thorough analysis of Caribbean and Asian political activity than Jacobs offers.

There is an additional empirical limitation. Having argued that Asian and Caribbean political organization has been localized, the only example he cites is that of Wolverhampton, where his focus is almost exclusively upon the Community Relations Council (CRC). Because the formation of CRCs led to the simultaneous creation and incorporation of Asian and Caribbean leaderships, this focus certainly sustains Jacobs's thesis. But I could accept it more easily if Jacobs had demonstrated that this organization was the primary focus for Asian and Caribbean political activity in Wolverhampton (something that could only be done by means of a more systematic and extensive survey of the political activity of Asian and Caribbean people in Wolverhampton, and if a similar pattern were revealed in other towns and cities. Case studies of single cities can be extremely useful, but in the absence of supporting evidence or of an argument and evidence that they can be considered to be typical or representative, one must hesitate over the generalization of the findings to other contexts. Elsewhere in the book, key assertions are not supported with evidence from Wolverhampton or from any other city. For example, we are told that young Caribbean and Asian people have begun to use state and private assistance to establish themselves as part of the growing petite bourgeoisie, but we are offered no evidence or source (p. 167).

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Jacobs's contextualization of his analysis is weak, particularly his historical overview of British state policy regarding immigration. He describes state practice as pragmatic (p. 22) but can only sustain this interpretation by ignoring detailed evidence to the contrary. For example, he ignores the fact that the Labour government of 1945-51 recruited non-British refugees to resolve labor shortages while simultaneously but secretly considering methods by which to prevent British subjects from entering Britain because they were "colored." He ignores the method by which a Labor government withdrew in 1968 the right of British passport holders to enter and settle in Britain if they were "colored" and the formalization of that device by a Conservative government in the Immigration Act of 1971, an act that increased by several million the number of "white" people with the right to enter and settle in Britain. There is a consistency in the actions of the British state on immigration that supports the assertion that it has systematically institutionalized racism.

These weaknesses are not offset by the positive features of the book. It usefully describes central-government initiatives and the emergence of private-company strategies to deal with urban decline and political protest during the 1980s. And the hastily added Postscript describing the political disturbances of 1985 suggests that these various initiatives have not resolved the major structural determinants of political protest and violence. It is also useful to have the recent debate about the role and effect of the Commission for Racial Equality summarized and clearly set out. But the author has not realized his objectives, and in the absence of a more thorough survey of Asian and Caribbean political activism, it is difficult to evaluate the extent and significance of the process of political integration that Jacobs describes.

ROBERT MILES

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Legal Traditions and Systems: An International Handbook. Edited by Alan N. Katz (New York: Greenwood, 1986. viii, 450p. \$55.00).

This is a large and physically impressive volume that treats a limited set of subjects for a large, implicitly worldwide sample of legal systems. It consists of an introductory essay by the editor and 18 substantive chapters (seven contributed by the editor), each dealing with the legal traditions and systems of either a single nation or a group of nations. The very brief (five-page) editor's introduction lays out the outline followed by the authors of the substantive chapters. Each opens with a discussion of the "historical development" and organization of the legal system. Each then turns to the "personnel of the law," especially the "recruitment and training of those entering the profession" and mobility within the profession and its relation to judicial selection and promotion. Finally, the outline calls for each chapter to discuss "the public's perception of the legal system and its personnel and the role played by the courts in the legal system studied" (pp. 2-4).

It is difficult to know what to make of this outline. Aside from an attempt to link the first two elements of the outline to a definition of legal tradition offered by Merryman and Clark, the editor makes no effort to develop a rationale for its use. Perhaps it is seen as an obvious or self-evident organizing tool for the production of "handbook fare." But the potential difficulties that lie in its lack of development are clearly seen in the editor's two-page discussion both of the evidentiary problems accompanying efforts to assess "the public's perceptions of the legal system and its personnel and the role played by the courts" and of the meanings attached to the "role played by the courts." At a minimum, assessment of the latter is said to involve assessment of the judicial independence and exercise of judicial review by the courts studied. The discussion that ends up being mandated by these outline topics is truly global and is inevitably muddled by use of available, selectively chosen evidence documenting the courts' "roles."

Each of the substantive chapters conforms fairly well to the editor's outline, especially given the broad and ambiguous tasks it mandates. In some cases, this leads to a very dry,

formal-legal discussion with no great depth as the chapter authors' attempts to fill in the outline fall back on the evidence of constitutional and statutory provisions in the absence of better quality evidence.

Although there is no explicit statement of the book's intended geographical coverage, the chapters included deal with the vast majority of the world's legal systems in some way or another. Nine chapters deal with the legal traditions and systems of individual nations: the Federal Republic of Germany, France, Italy, Japan, the People's Republic of China, the Soviet Union, Spain, the United Kingdom, and the United States. Four chapters treat small groups of nations that are united by some fairly closely shared historical or cultural backgrounds: the Benelux nations; Canada, Australia, and New Zealand; India, Pakistan, and Bangladesh; and Scandinavia.

The remaining chapters report on the rest of the world considered as more or less standard geographical-cultural divisions: Africa (actually black, sub-Saharan Africa), Eastern Europe, Latin America, and the Middle East. These regions contain from some (Eastern Europe) to considerable (Southeast Asia) variety in political systems, national histories, cultures, languages, religions, and so on.

The "great-powers" bias of the book's composition is striking to any student of contemporary world politics. One might easily accept the presence of separate chapters on the legal systems of China, the Soviet Union, and the United States, three of the world's most populous and powerful nations. But it is less easy to defend including separate chapters on the Federal Republic of Germany, France, Italy, and Spain while lumping India with its undemocratic neighbors, treating Brazil as merely one of six examples of Latin American legal systems, and paying no separate systematic attention to Africa's largest nation, Nigeria. The point is not that the chapters on the Western European nations are uninformative or poorly done; it is that even the larger and potentially more powerful nations in Third World continents get relatively short shrift in the design of *Legal Traditions and Systems*.

Part of the reason for this great-power bias is a short supply of adequate sources for many Third World legal and judicial systems. But that can be only part of the problem: sources

on the Indian legal system and courts are voluminous, for example. To fulfill their assignments within the apparent space limitations, authors of the regional chapters are forced to make some difficult coverage choices. Some, Harvey Feinberg in his chapter on Africa, for example, choose to pitch their discussions at a very high level, seeking broad generalizations and using concrete examples to illustrate their points. Others, Lee Epstein, Karen O'Connor, and Diana Grub in their chapter on the Middle East, for example, choose to concentrate on specific nations (Egypt and Israel, in this case) as regional examples of apparent or demonstrated relevance.

Overall, this work will be of only limited value to political scientists. Its focus on legal traditions and systems moves it away from the mainstream concerns of public law and judicial politics, along with judicial policy-making and judicial behavior. Its coverage, though broad, is not broad enough to make it a comprehensive reference tool. But it is broad enough to make much of the discussion formal and shallow. It is in discussing the roles of the courts that the handbook chapters most frequently touch closely on matters of central concern to political scientists. It is also in these discussions that the gap is greatest between what the authors say and what they can say with confidence because it is based on firm evidence.

C. NEAL TATE

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Politics, Economics, and Welfare Reform. By Leslie Lenkowsky (New York: University Press of America, 1986. vii, 207p. \$12.75).

This book gives an account of the effort to implement versions of negative income tax in Britain and the United States. "The story of FAP, FIS, and the tax-credit plan," we are told, "suggests that whatever the future brings in Britain and the United States will emerge from the interplay of historical, political, economic, and cultural factors in each nation" (p. 188). Dr. Lenkowsky clearly does not like to go "out on a limb." And, barring the intervention of Shirley Maclaine's extraterrestrials, the future is unlikely to let him down. Generally, this

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book is more compelling in its description than in its analysis.

This comparison of attempts to implement negative income tax is presented as an example of "surprising convergence," as a method for explaining the genesis and failure of policy proposals, and as a means of arriving at general conclusions about the two political systems. The book's contributions lie elsewhere, however. They may be found in the discussion of the idea itself as a serious alternative to the present structure of social policy.

Dr. Lenkowsky claims that "negative income tax was an idea that could appeal to liberals and to conservatives, to those who liked the 'welfare state' and to those who valued individualism" (p. 174). The most important reason for the "surprising convergence," he argues, was the central role played by experts in advancing the idea. As a general concept, it is true that negative income tax is ideologically neutral. But as soon as it is given a specific structure of tax rates and benefit rates it reflects the values of policy makers. Targeted towards poverty, negative income tax offers individually tailored help for the "truly needy" that substitutes cash for the complicated network of existing policies, without diminishing work incentives or damaging the operation of the market. These are the advantages Milton Friedman mentions, and from this perspective it is not really surprising that such a policy, consonant with a particular market-oriented vision of the welfare state, should appeal to both Richard Nixon and Edward Heath. (Perhaps more surprising is that the more self-consciously Friedmanite Margaret Thatcher and Ronald Reagan, to an extent, should have neglected the idea.) It had become a right-wing idea, associated with right-wing experts, and so appealed to right-wing governments. Actually, even as a general concept, there is an ideological content to negative income tax insofar as it is viewed as a complete solution to welfare problems, without regard for health policy or full-employment policy, for example.

Aside from his argument about negative income tax being a rational and attractive idea propagated by experts, Lenkowsky suggests that both countries share similar welfare states, even in health policy (p. 171), and that these similarities in policies and values "led Britain and the United States toward the negative

income tax" (p. 175). I am not aware of any comparative work on the welfare state that would support this claim of similarity.

From this comparison of welfare states we move to the equally shaky ground of a comparison of whole political systems. British government deals badly with complex issues, due to the "often unrecognized restraints of Parliamentary government" (p. 177). Briefly, a Prime Minister, "before making substantial changes," needs a consensual manner and the cooperation of ministers, backbenchers, civil servants, the opposition, and relevant interest groups. Lenkowsky should tell this to Mrs. Thatcher.

The book is better when describing the progress of the policies. While saying that the negative income tax failed because it was too sophisticated, Lenkowsky also provides evidence that the idea had its difficulties. In his account of the work of the Senate Finance Committee, he describes how Senator Williams pointed out the "notch effects," where in some circumstances a rise in earnings would result in a loss of income as a result of the proposed policy. In addition to similar objections, in Britain there was also the problem of the low take-up rate. Those eligible for the Family Income Supplement did not always receive it. Lenkowsky apparently relies on government figures in discussing this problem. Curiously, while Peter Townsend appears in the impressive interview list at the end of the book, his *Poverty in the United Kingdom* (Berkeley: University of California Press, 1979), which comes up with significantly higher figures, is not cited. Also omitted from this book is an index.

It does have on its cover, however, very favorable comments from two participants in the policy-making described, Daniel P. Moynihan and George P. Shultz. Moynihan says that Lenkowsky "has mastered that most elusive art of comparative politics." His art has eluded this reviewer.

NEIL J. MITCHELL

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Sozialdemokratische Verfassungstheorie in der Weimarer Republik. By Wolfgang Luthardt (Opladen: Westdeutscher Verlag, 1986. viii, 194p. 32 marks).

Luthardt's short book on social democratic theories of the Weimar constitution epitomizes a decade during which younger German social scientists have rediscovered and reassessed the achievements of some previously neglected German exiles from Nazism—a number of Social Democratic thinkers trained in the law, including Hugo Sinzheimer, Herman Heller, Hans Kelsen, Ernst Fraenkel, Franz L. Neumann, and Otto Kirchheimer. Many U.S. political scientists also will find a lot to think about in Luthardt's analysis and assessment of these sophisticated attempts to interrelate a sociologically realistic democratic constitutionalism with a design for radical social change based on labor and proceeding through law. The most recent turn in much contemporary discussion, moreover, means that U.S. political scientists will not be discouraged by the fact that this political theorizing has a marked juristic component. The political sources, uses, and limitations of law have become central themes in present-day inquiries into the supposed "crisis" of the welfare state, as they were in Weimar.

Luthardt states the central question confronting almost all of his protagonists as one of how Socialists, who consider a progressive dismantling of capitalism a necessary condition for the maintenance and extension of democracy, can subordinate their conduct to a legality defined by a parliamentary democratic constitution that guarantees their opponents ample chances for making political use of their massive social power. This formulation connects with the predominant theme in the self-criticism by Neumann and others after 1933, that the Social Democratic strategy was vitiated by excessive "legalism." According to Luthardt, the response towards which his theorists tended is best summarized in the conception of "collective democracy" adumbrated by Ernst Fraenkel in 1929, at the time of the last Socialist-led coalition regime and before the impact of the Great Depression.

Collective democracy is a theory of the Weimar constitution that stresses the intrinsic value of its democratic political forms as an alternative to absolutist violence, that affirms

the indispensability of compromise, and that locates the dynamics for progressive social change in the legislative and social development legitimated (and in part initiated) by the constitutional provision for parliamentary sovereignty and social rights. The three labor lawyers, especially—Sinzheimer, Neumann, and Fraenkel—elaborated the third of these points. For them the institution of a "parity" constitution, parallel to the political one, was of the highest importance. This was to be a constitution that would give new contents and new impulses to legislative politics, as witnessed by the transformation of property law through labor law (especially through the operations of collective agreements) and the transformation of administrative and judicial agencies through the incorporation of organizationally grounded social judgments and negotiations, counteracting statism and class justice.

Unlike many critics, Luthardt does not belittle the theory of collective democracy when he turns to an analysis of the "confining conditions" that obviated its designs in Weimar, especially under the pressure of economic disaster. After 1930, he points out, the Social Democratic conception was rendered ineffectual. The parliamentary legislative state was confronted by a parallel and superior legitimacy grounded in statist and plebiscitarian elements in the constitution, a legitimacy that the courts used to devise a "legality" fatally limiting the political and social relevance of participation by the labor movement. The non-socialist partners in the political and social processes of compromise abandoned the earlier common ground. The incipient social constitution effectively collapsed. When the democratic labor movement opted for "legality" between 1930 and 1933, according to Luthardt, the substance of legality no longer corresponded with the constitutional design upon which these thinkers had not unreasonably pinned their hopes. All of them recognized this (although Luthardt singles out Kirchheimer as the most astute), but none of them formulated alternatives beyond rhetorical statements of defiance or tragic acceptance of what they could not change. Luthardt's somewhat eccentric inclusion of Kelsen in the study, despite his distance from most of the elements of "collective democracy," clearly signals Luthardt's own resolu-

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tion of the question of what is to be put first if the constitutive question of democratic socialism cannot be successfully answered in a socialist way.

Luthardt does not evade the question whether the failure of the Social Democrats' project in Weimar makes their conceptions more than a deterrent object lesson. Neumann and Kirchheimer thought so themselves. But Luthardt draws on the past generation of remarkable historical scholarship to present a diagnosis of that failure that is far more nuanced than the shocked self-judgments of the defeated participants or the facile stereotypes that grow out of them. The result is a more qualified and open response to the structural and strategic thinking of the reformist Left. He thinks that Weimar may still have something to teach us. Nothing can turn the history of Weimar social-democratic speculations into a success story. But it can be productively demythologized and rendered useful for discriminating comparative study and reflection by political scientists. Luthardt's book is a very useful contribution to the latter undertaking.

DAVID KETTLER

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Supreme Courts and Judicial Law-Making: Constitutional Tribunals and Constitutional Review. By Edward McWhinney (Dordrecht, The Netherlands: Martinus Nijhoff, 1986. xvi, 305p. \$67.00).

Edward McWhinney, prolific writer on world judicial systems, has written another book on the role of courts in the policy-making process. Here, in the context of several national legal systems, he looks at the variables related to the role of *judicial review*, which he defines as "Court practice in applying constitutional norms to scrutinise executive and legislative actions" (p. xiii). Because of the influence of the U.S. judicial system, this is actually a study of the dissemination of the U.S. principle of judicial review into other legal systems of the world.

McWhinney attributes the rise of modern-day judicial review in large part to the prestige and influence of the United States in the post-

war world. He notes that the example provided by U.S. independence from an imperial power also made the United States a likely model for judicial review. Moreover, for former Axis nations, adopting a new constitutional system was seen as a way of embracing democracy and shortening the Allied occupation time.

Supreme courts around the world, namely, the Indian Supreme Court, the United States Supreme Court, the West German Federal Constitutional Court, the French *Conseil constitutionnel*, the Japanese Supreme Court, the Canadian Supreme Court, and, to a more limited extent, the International Court of Justice at the Hague, are the focus of analysis in this book. The underlying purpose is to show how supreme judicial bodies play out the legal-realist drama of judges "making," rather than "finding," law and to expose the "legal myth" (the opposite of the realist approach) in an international context.

The book seems to be divisible into two parts: the first part, consisting of the first four chapters, presents disparate information about the role and selection of chief judges, court organization and structure, appointment by chief executives, terms in office, removal of judges, and measures of descriptive characteristics of justices who serve on these courts. The major problem with the analysis in this beginning section is an insufficient attempt to relate this enormous array of factual information to the theme of the book; that is, to examine how these variables impact on, and are affected by, the role of the selected supreme courts in constitutional review.

Especially disappointing in dealing with this mix of countries is that there is not enough recognition of the differential effects of civil-law and common-law systems on judicial review. The author begins chapter 2 by announcing that he has identified three principles that separate these two types of legal systems, but he does not adequately discuss what would appear to be a major question to legal scholars, namely, whether judicial review appears in different guises because of these two legal traditions. After all, judicial review is based on a principle of judicial supremacy over legislative and executive actions; this is a practice certainly more familiar to common-law states. Surely it is worthy of more notice that such courts as the *Conseil constitutionnel* and the

Federal Constitutional Court, arising out of civil-law traditions, perform roles similar to courts in countries with common-law backgrounds.

The last part of the book, however, dealing with judicial interpretation of constitutional charters, makes it all worthwhile. These chapters offer an excellent discussion of courts in the context of national political and legal arrangements and present comparisons of dimensions of judicial policy-making by identifying types of national legal systems on an activism-restraint policy-making continuum. The major issue of constitutional interpretation, according to McWhinney, is the behavior of supreme courts in adopting a preference for "strict and complete legalism" as opposed to the imposition of judicial value judgments (p. 92). I agree with him that it is necessary to question whether legalism is possible or even desirable in decision making on "the great political and social issues of the day" (p. 92).

McWhinney differentiates primarily between constitutions patterned after a U.S. model, ones with a "lapidarian text, short and succinct" and constitutions with a "detailed social blueprint" (p. 89) commonly found today in Eastern European countries. The discussion from chapter 5 to the end of the book is devoted to explication of the legal-realist premise that judges play active roles in policy-making when they interpret the constitutions of their respective states on such major questions as social policy, federal arrangements, institutional (political) questions, and economic policy. This analysis is especially valuable as a means of comparing the United States with other countries of the world—something that U.S. legal scholars are not often enough given opportunities to do.

McWhinney concludes by observing that he has found that "the same problems tend to recur in all societies, and that the judicially found solutions are often remarkably similar in spite of the seeming disparateness of the constitutional charters involved" (p. 279). All in all, this book plays an important role in bringing us much closer to an understanding of this basic truth.

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Syria under Assad. Edited by Moshe Ma'oz and Avner Yaniv. (New York: St. Martin's, 1986. 273p. \$35.00).

Syrian Intervention in Lebanon. By Naomi Joy Weinberger. (New York: Oxford University Press, 1986. 367p. \$29.95).

Contemporary Syria poses a series of paradoxes to outside observers. Close relatives and comrades of the current president, Hafiz al-Asad, monopolize the most powerful offices within the state and party apparatus, yet the government's broadly populist policies have generated "support from large sectors of the Syrian population that have benefited from the regime or that share the Ba'ath concepts" (Ma'oz and Yaniv, *Syria under Assad*, 34). Efforts to expand the role of private enterprise in the country's commerce and industry produced substantial levels of economic growth during the 1970s but at the cost of rising political discontent and increasing indebtedness both to Western financial and trading concerns and to the Arab oil-producing states (pp. 44-65). The al-Asad regime has devoted greater amounts of scarce resources to the country's armed forces over the last decade or so, while allowing social-welfare programs upon which much of its popular support depends to languish (pp. 72-76). Syria's leadership has, since 1970, adopted a consistent policy of reducing the influence of the Ba'ath party over such key institutions as the armed forces and state administration but legitimizes its actions in terms of promoting Ba'athi socialism and prohibits rival organizations from playing a substantial role in domestic politics (*Syria under Assad*, 29; *Syria Intervention in Lebanon*, 74-77).

These dynamics have a direct impact on Syria's foreign affairs. Ba'athi policies designed to promote socialism at home continue to prevent Syria and Turkey from establishing diplomatic relations that are anything but "markedly chilly" (*Syria under Assad*, 94-97). Domestic opposition made it impossible for the al-Asad regime to implement programs intended to stabilize Lebanon during the years from 1977 to 1981 (pp. 184-85). Rivalry between the more radical Ba'ath associated with Salah Jadid and pragmatists allied to al-Asad laid the groundwork for Syria's abandoning al-Fath and creating competing Palestinian organizations such as al-Sa'iqa and the

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Palestine Liberation Front in the mid-1960s (p. 194). And shifts within the Ba'athi regime largely determined the character of relations between Damascus and Moscow over the last two decades (pp. 227, 230).

But no other event demonstrates the connection between the country's domestic politics and its foreign policy more clearly than the initial phases of Syria's intervention in the Lebanese civil war. As Naomi Weinberger shows, "Syria's military elite" faced growing sectarian and "socioeconomic" discontent in the years after 1973 (pp. 77-79). Whatever stability was present within Syrian society was increasingly a consequence of the regime's use of armed coercion to dominate such challengers as the Muslim Brotherhood (p. 81; cf. *Syria under Assad*, 31-34). Thus, when the situation in Lebanon began to deteriorate in the winter of 1975-76—and persistent disunity within the Arab world precluded effective opposition to Syrian action (*Syrian Intervention in Lebanon*, 265-68)—the al-Asad regime dispatched first the Syrian-sponsored Yarmouk brigade of the Palestine Liberation Army and then regular units of the Syrian armed forces across the border into Lebanon (pp. 180-83, 213-14, 225). In retrospect, it is not clear that this move was worth the cost: Syria was for the most part unable to bring order among Lebanon's warring militias; refugees from the fighting imposed a serious burden upon Syria's already shaky economy (p. 234); and support for the Maronite Christian Kata'ib generated open discontent among Syria's Sunni Muslim community (pp. 236-37). All of these factors prevented the al-Asad regime from achieving its underlying objective in Lebanon—that of restoring some degree of orderliness to Lebanese affairs, if not of recreating the pre-1975 status quo.

Weinberger herself rejects this line of argument, claiming instead that Syria's intervention in Lebanon was related to "multiple miscalculations" on the part of the al-Asad regime (pp. 16, 326-27, 337). These misperceptions were largely predicated upon the regime's vocal support for the Palestinians residing in Lebanon in general and for the mainstream Palestine Liberation Organization in particular (pp. 112-15 and chap. 5). In her view, defending the Palestinians from the Kata'ib and other Lebanese forces, while preventing Yasir Arafat from consolidating a tactical alliance with

Kamal Jumblat's Lebanese National Movement, could have provided Syria with a principled basis for claiming a leadership position in inter-Arab affairs (pp. 14-16, 114, 241). This perspective is congruent with those of Yair Hirschfeld, who argues that the alliance between Damascus and postrevolutionary Tehran serves as a way of enhancing Syrian strength and prestige relative to Jordan and the Arab Gulf states (*Syria under Assad*, 117-21), and Amazia Baram, who explains trends in Syrian-Iraqi relations in terms of successive ideological shifts in each country's wing of the Ba'ath (pp. 128-38).

In the end, this kind of explanation remains both incomplete and unconvincing since it deals only with what Weinberger calls the "motivations" for Syrian foreign policy rather than with the conditions that enable or prevent a regime from carrying out its preferred programs. Although *Syrian Intervention in Lebanon* provides a generally lucid overview of the opening phases of Syria's involvement in the Lebanese conflict, as well as of the pitfalls into which the al-Asad regime has stumbled in the years after 1976, the volume provides no theoretical explanation for why the regime was able to act on its presumed motivations. (On pp. 196-98, Weinberger suggests that Syria completely reoriented its policy toward the conflict when President al-Asad changed his mind about backing Jumblat's forces.) In the absence of a well-developed explanation for Syrian policy that connects internal dynamics to external actions, the structural perspective adopted by Avner Yaniv in his analysis of Syrian-Israeli interaction during the period from 1948 to 1982 (*Syria under Assad*, 157-78) and by Robert Freedman in his essay on Soviet-Syrian relations in 1982-83 (pp. 224-43), as well as by Weinberger in her discussion of Israeli and Soviet-U.S. responses to the Syrian move into Lebanon (chaps. 10 and 11), appears to be the most promising means in the existing literature of unraveling what Ma'oz and Yaniv call "the Syrian puzzle."

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Policy Making in a Three Party System: Committees, Coalitions, and Parliament. By Ian Marsh. (New York: Methuen, 1986. 264p. \$65.00).

The rise of the Liberal-Social Democratic Party (SDP) alliance in the early 1980s has fueled vigorous debate among observers of the British political system about the prospect of a "hung parliament," perhaps marking the end of a very long era of two-party adversarial politics. In February 1987 the Greenwich by-election in southeast London resulted in a spectacular success for the SDP at the expense of both the other main political parties. Clearly, as Ian Marsh says in his Introduction, "A House of Commons in which no single party commands a majority of members can no longer be considered fanciful" (p. 1). But how in practice would such a parliament operate? What impact would a no-majority situation have on the way in which the processes of government and policy-making are conducted? Would the discussion of policy become more open, or would powerful political parties, forced into formal coalitions or less formal pacts, be even more inclined than they are now to hammer out their compromises privately in smoke-filled rooms? Could a hung House of Commons break free of the shackles of executive domination and carve out for itself a more significant and positive role in the ordering of policy priorities and the mounting of initiatives? Would the fragile principles of individual and collective ministerial responsibility, and the bruised conventions of civil-service neutrality—not to mention the mechanistic functioning of constitutional monarchy—survive such a change?

Inevitably, much of the discussion on this subject has been highly speculative, and some of the conclusions reached have wildly exaggerated the likely short-term impact of such a result. Hung parliaments could take many different forms. The outturn of events would depend, among other things, not only upon the party political arithmetic but also, crucially, upon how long the situation lasts. After all, the most probable consequence of a hung election result is the prospect of a rerun in a few months' time, as happened in February and October 1974; but the persistence of a hung parliament for a decade or so, and a lasting im-

provement in the alliance's capacity to translate its votes into seats (buttressed perhaps by the introduction of proportional representation), would almost certainly necessitate a major reappraisal of the way in which British government is conducted.

So far as the House of Commons itself is concerned, it may be tempting to speculate, as Ian Marsh does here, about the potential role of investigative select committees in such circumstances. Westminster's committees inevitably tend to operate on the sidelines of a government-dominated parliamentary system, being concerned with scrutinizing and informing rather than with active decision making, but they have long been seen by optimistic parliamentary reformers as holding the key to redressing a perceived imbalance of power between executive and legislature. The system was significantly changed in 1979 with the establishment of 14 departmentally related select committees empowered to investigate public policy, administration, and finance across the whole range of government responsibility. Might not the new committees, relatively tranquil islands of bipartisan inquiry in a storm-blown ocean of adversarial politics and perhaps possessing the untapped potential to acquire, as Marsh suggests, "independent powers of political initiative," find a special niche in a hung parliament as, "an essential buttress to effective government"? (p. 2).

Marsh's book has two distinct aspects. The first is the product of impressively thorough empirical research. The second is based almost entirely on wishful thinking, only tenuously reinforced by the author's research findings. In the research-based part of the exercise, the author traces the development of the present committee system. He examines with commendable thoroughness the scope and impact of committee inquiries and reports in the fields of economic policy and budgeting, and in relation to "current issues" and strategic policy-making, concentrating on the session of 1982-83. Description of his findings is clear, and there are abundant and helpful summaries in tabular form. His conclusions about the impact of committees are, for the most part, careful and well balanced, though he sometimes exaggerates the significance of committee activity, as when he claims that "there is evidence that committees have crafted a role for themselves in each phase of the budget

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process" (p. 61): that role, in so far as it exists at all, is episodic and highly marginal.

The author has digested a large amount of paper and talked to a very large number of people involved, in one way or another, in committee activity. There is a separate chapter on select committees and interest groups, based largely on the results of a wide-ranging questionnaire survey, supplemented by interviews. The author concludes that committees themselves have made extensive if patchy use of interest-group evidence but have done little actively to cultivate their "outreach" to such groups; the latter, for their part, remain uncertain about the role of committees but generally welcome their "visibility" and their "fairness." There is, the author believes, considerable undeveloped potential for enhancing the scope of interaction between committees and groups.

So far, so good, but the more general, speculative conclusions of the book require the reader to make an imaginative leap from the real world into make-believe. Marsh's working premise is derived from Samuel Beer's indictment of "pluralist stagnation" (*Britain against Itself* [New York: Norton, 1982]). He argues in his introductory chapter that the "drift and immobilism" of current policy making (he does not give a clear impression of what exactly he has in mind here) could best be remedied by integrating "recalcitrant pressure groups" more effectively and comprehensively into the policy-making process via the select-committee system.

This is a reasonable enough starting point. The trouble is that the author fails to sustain his thesis on the evidence derived from his research and has to fall back on assertion and guesswork. No doubt there is unrealized potential for committees to act more extensively as a forum for the articulation and mediation of group demands, but the recent record of committees provides no indication at all of the likelihood of this happening. Arguably, if it did happen, it *might* be a good thing (though one doubts if it would be a panacea for pluralist stagnation), but this is a matter of both opinion and prediction. And the author himself acknowledges that the views of the Conservative and Labour parties are essentially hostile to such a development. Even the alliance, notwithstanding SDP leader David Owen's general sympathy towards enlarging the role of committees, has not so far begun to

go down the road signposted in this book. And whether a hung parliament, of whatever kind, would advance the author's cause takes us even further into the realm of fantasy. It is a pity that the book becomes so wildly speculative, because it contains some interesting things.

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Liverpool on the Brink. By Michael Parkinson (Newbury, England: Policy Journals, 1986. 184p. £22.50 cloth; £9.50 paper).

On 12 March 1987, the Judicial Committee of the House of Lords ruled against 47 Liverpool city councillors and thereby disqualified them from office for five years and left them with total debts of over £500 thousand. This decision marked the end of a four-year fight between the Labour party in Liverpool and Mrs. Thatcher's administration. Michael Parkinson's book tells the story of this fascinating struggle.

The volume provides a detailed account of how the political drama unfolded up to autumn 1985 and, although the book suffers from an absence of references and also lacks an index, Parkinson ably highlights the fiscal, political, organizational, economic, and ideological factors in the potent mix that led inexorably to confrontation with the central government—and to the verge of municipal bankruptcy.

As Parkinson clearly shows, Liverpool Council was *underspending* during the period 1974–83, when no party had a majority and the Liberals held the balance of power. They underfunded services and used reserves to avoid raising the rates; this caused dire consequences after the election of the Thatcher administration, as the artificially low level of expenditure became the basis of financial cuts. "The lost decade" was characterized by confusion and a persistent failure to tackle the city's financial shortfall. The political uncertainty led to bureaucratic inertia. Inefficient council services were not reorganized, and municipal housing was not adequately repaired or replaced.

Within the Liverpool Labour party, where years of patronage and complacency had taken

their toll, the old guard was ousted during the 1970s by new-breed activists, many of whom supported the views of the newspaper, the *Militant*. Under their influence and as a result of a shift in union power, municipal socialism became Labour's central concern. Between 1979 and 1984, 40 thousand jobs disappeared and "Liverpool became known as the Bermuda Triangle of British capitalism" (p. 12). By 1985 the rate of unemployment was 27% and in these circumstances the local Labour party was determined to safeguard council jobs. The Conservative government was equally committed to reining in what it saw as profligate local-government spending.

Parkinson argues that a clash between two ideological responses to urban decline was at the heart of the conflict between Liverpool and the Thatcher government. In addition, the national leadership of the Labour party became increasingly embarrassed by the Liverpool councillors' strategy.

Liverpool on the Brink tells the story of how one council challenged the British government and won a round but lost the fight. Parkinson convincingly shows that the local Labour leadership was politically conceited and tactically inept. Nevertheless, although a combination of factors brought Liverpool to the brink, a principal cause was a blundering and obstinate central government. Although the center has control over legislation and financial grants, local authorities have resources of their own, such as the ability to raise finance, control over the implementation of policies, legitimacy arising from local elections, and access to local expertise and information. Consequently, relations between central and local government in Britain have been generally characterized by bargaining and accommodation.

From 1979 onwards, however, the position changed. Mrs. Thatcher's government arrived with scarcely concealed contempt for politics by negotiation and consensus and with a determination to impose its policies on local government. This approach predictably led to considerable opposition. Much of the resistance has been covert. Resentful local authorities manipulated their finances and procrastinated in response to central directives. In Liverpool's case, however, the opposition was stridently defiant.

According to Parkinson (p. 36), the Liverpool political and fiscal crisis was like a Greek

tragedy in which the ending was determined as the play opened. But what ending has been reached? Liverpool City Council's debts are currently estimated at £850 million. Unemployment and social deprivation are widespread. Serious public disorder may recur. And the people of Liverpool may once more vote councillors into office who, like their disqualified predecessors, are prepared to defy the British government.

JOHN BENYON

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Israel at the Polls, 1981: A Study of the Knesset Elections. Edited by Howard R. Penniman and Daniel J. Elazar (Bloomington: Indiana University Press, 1986. 280p. \$35.00).

For some years the American Enterprise Institute, under the editorship of Howard Penniman, has been sponsoring election studies of selected countries, with each successive election becoming the subject of a volume of collected articles. Of these volumes, the editions on Israel have complemented another series that the political scientist Asher Arian has been editing in Israel since the 1960s. Together, Penniman, Arian, and their contributing authors have given English-language readers excellent access to a political system in which all that can happen in parliamentary politics often does happen.

One drawback of the Penniman-Elazar volume on the 1981 Israeli election is that it was published five years after the event. Major changes have taken place in Israeli politics since then, including the 1983 retirement of Menachem Begin after the Lebanon War of 1982. Events such as these are covered only in a concluding article by Daniel Elazar, but even here the concluding year is 1983. Not mentioned are the 1984 election and the creation of Israel's first government based on a grand coalition of Labor and the Likud.

The value of this book therefore lies less in its description of one election and more in its treatment of long-term political trends. Valuable too are the political-cultural insights into a changing Israel provided by well-informed academic observers.

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Among the trends discussed in depth is the consolidation undergone by the Labor and the Likud alliances, or blocs of parties, so that each is becoming a party rather than a bloc. Israel has acquired a quasi two-party system, in which the two major parties are equally capable of contesting for control of the government. The smaller parties have steadily declined as survivable entities. A case in point is the decline of the National Religious Party (NRP) from its former prominence as an important coalition partner and representative of religious opinion.

Changes in the party system are linked to trends within the political culture. There has been a blurring of the distinction between secular and religious politics in Israel as a new "civil religion" emerges blending rabbinical Judaism with the events of Israeli national existence (for more on this, see Charles S. Liebman and Eliezer Don-Yehiya, *Civil Religion in Israel* [Berkeley: University of California Press, 1983]). Generational changes within political elites and among the electorate have been rendering older ideological labels ever less meaningful despite continuation of the ideological style in Israeli political discourse. Events and elections have legitimized some leaders and factions and delegitimized others. The Israeli Arab minority has come into the mainstream of Jewish party politics rather than remain an alienated group of protest voters. Prohibitions on Israeli and U.S. interventions in each others' political systems have declined in effectiveness, and the relationship between the Israeli and U.S. Jewish communities has experienced strains.

Many of the articles in the Penniman-Elazar volume that cover these trends are valuable encapsulations of ongoing research undertaken either by the authors themselves or by others. Torgovnik and Arian, to cite just two of the contributors to this book, summarize their own research pointing to the steady long-term rise in support for the Likud, and the decline of the Labor Party's ideological distinctiveness (see Arian, *Politics in Israel* [Chatham, NJ: Chatham House, 1985]). Labor has become a party that tries to appeal to as many segments of the electorate as possible. But it is unable to secure loyalty among large numbers of younger Israelis unimpressed by the party's heroic past or among many voters from the "Second Israel" (Sephardic and Oriental Jews).

Shmuel Shandler, another contributor to this book, offers a conceptual historical treatment of the religious parties, thus continuing the sociological style of analyzing party behavior pioneered in Israel by Yonathan Shapiro (*The Formative Years of the Israeli Labor Party* [Beverly Hills: Sage, 1976]) and Horowitz and Lissak (*The Origins of the Israeli Polity* [Chicago: University of Chicago Press, 1978]). His eye-opening article delineates the process whereby ideological blurring among many parties has occurred. That process started with Labor's abandonment of its "consociational" relationship with the NRP after Ben Gurion's departure from Mapai.

So long as Mapai avoided mergers with other socialist parties and relied on the NRP for support in coalition making, the NRP elite respected the Mapai's dominance in domestic and foreign affairs. But the new Labor alignment's reduced need for partnership with the NRP set the stage for younger NRP leaders to take positions on formerly secular matters. This they would do in the wake of the 1967 war and the capture of what many Israelis now call "Judea and Samaria." Yet the NRP suffered because the emergence of the Likud bloc gave religious voters a new way to express themselves. Even regular NRP voters felt safe in turning to a Likud that posed no threat to the place of religion in Israeli life. Those same voters were now apprehensive of a secular Labor alignment no longer made cautious on religious issues by a need to preserve its ties with the NRP.

Each article in the Penniman-Elazar book shows that care was taken to place the 1981 election within historical-developmental contexts of this kind. The most impressive articles in terms of identifying linkages of subtlety and elegance are those of Elazar, Brichta, Reich, Shandler, and Arian. Elazar writes from his understanding of Israeli and Jewish culture; and Brichta offers a theoretical framework within which Israeli nomination methods can still be called oligarchical despite major efforts at reform. Arian draws from his immense longitudinal store of survey and voting data to reach conclusions explained with sarcastic wit.

Unusual perspectives on Israeli elections are provided by Samuel Krislov in describing the U.S. role and by Judith Elizur in detailing the role of the Israeli news media.

The same is true of Efraim Torgovnik who

writes of the Labor party with the knowledge of an insider and with much delicious detail about who-did-what-to-whom. Indeed, an element that could be even more emphasized in a serious study like this one is the sheer fun of watching the antics of Israeli *askan tziburim* (public persons, i.e., politicians). To see the deal making, the betrayals, the names people call each other, the public reconciliations, the decisions achieved by bluster and brinkmanship, the marriages of high principle and low machine manipulation is to capture much of the true flavor of Israeli political life. I hope more of that flavor appears in any 1984 Israel election volumes issued by these or other editors.

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Government and Politics of Singapore. Edited by Jon S.T. Quah, Chan Heng Chee, and Seah Chee Meow (Singapore: Oxford University Press, 1985. xviii, 324p. \$10.85).

This book, completed in 1980, was a project supported by the Southeast Asian Studies Program of the Ford Foundation. The eight authors, including Professors Quah, Chan, and Seah, who served as editors, are Singaporeans teaching at the National University of Singapore. Dr. Quah coordinated the project and also contributed two chapters.

The collection of 12 original essays is designed as a textbook for Singapore University students. It analyzes a country whose prime minister, shortly before independence, warned that an independent Singapore was an economic, geographic, and political absurdity. The two bibliographies, one at the end of the Introduction and one at the end of the book, are well chosen and extensive. Further detailed references are found in the endnotes to each chapter.

The Introduction sympathetically distinguishes the inside and outside researcher and alerts us to the interest of outsiders in Singapore during the 1960s and early 1970s, years of considerable foreign research on Southeast Asia. Unfortunately, an era of cooperative research between insiders and outsiders has yet to emerge. They have data and perspectives that complement each other, and

collaboration should augment our knowledge of and perspectives on Singapore and the Third World substantially.

This work is divided into three parts. Part 1 deals with political, economic, and social-cultural conditioners. Part 2, the longest, deals with the contemporary political system. Part 3 analyzes four policy areas—education, public housing, public transportation, and foreign policy. The chapters are not especially critical in their approach, but neither do they avoid pointing out political shortfalls and problems.

Part 1 offers a useful balance between detail and analysis. The British left behind a modern political infrastructure. A specious anti-colonialism is not part of Singapore's political culture. Seven years after the book was completed, a key issue, identified in chapter 1, remains high on the political agenda—the effort of the ruling People's Action party (PAP) to find a midpoint "between liberalism and benevolent paternalism." There are inevitable gaps. Operation Cold Storage in February 1963 critically wounded the principal opposition party, the Barisan Sosialis, and facilitated the PAP government's efforts to acquire viability and legitimacy. More time should have been allocated to a discussion of government debt as well as income inequality, both mentioned but not analyzed in chapter 2. What is the Singapore identity? Chapter 3's conclusion that "the levels of national identity in Singapore are very high" may be premature. National identity takes decades to evolve. Singapore is a young country. Still to be researched is how and in what ways it is nurturing popular self-identity. Overall, however, part 1 is a very useful heuristic overview.

Part 2, on contemporary politics, focuses on institutions. This is effective in relatively stable, new political systems where most political behavior funnels through existing institutions. An institutional approach frequently is the best entrée to identify and study the processes of influence and decision making as well as to understand how politics actually works. This is an excellent way to look at functions—because functions are essential in defining and describing an institution. Institutional analysis often makes it possible to focus attention immediately on what is of primary political importance. Part 2 evaluates parliament, the civil service, statutory boards, political parties, and parapolitical institutions

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such as the People's Association, Citizens Consultative Committees, and Residents Committees. Chapters on interest groups, the executive elites, and the judicial system were not included. The latter would be especially useful now, particularly in view of the wide publicity given to former Member of Parliament (MP) Jeyaretnam's legal problems.

Singapore illustrates one form of cabinet government. The cabinet dominates, and rule by administrative technocrats is unbroken since the mid-1960s. One opposition MP now sits in Parliament. Parliament attracted increasing public attention when two MPs were in Parliament. Now that Mr. Opposition, J. B. Jeyaratnam, who served from 1981 until November 1986, has served one month in jail and is excluded from Parliament for five years, parliamentary sessions will be less spirited. Parliament's policy-input functions will remain generally minimal, as they have since the 1960s. Chapter 4 notes a few issues where the government saw no easy or logical solution, and debate was bolder. A few earlier policies, which were made to seem obvious to Singaporeans and non-Singaporeans, are partially unraveling. This might encourage hard-hitting questions by more backbenchers, especially as the second-generation leaders assume increasing responsibility. The single opposition member has many valid questions to raise, though no structured alternative program has been developed by any of the 15 registered opposition parties. Perhaps, as in the British Parliament, the Singapore Parliament will remain primarily a tester and maker of persons, not of policy. The civil-service chapter provides a good overview. Future research should include recruitment, institutional identity, administrative initiative and policy formulation, and the Management Services Division.

The diminishing role of the People's Action party began in the early 1960s. It is not, though, a cultural holdover and still undertakes relevant functions like short-term electioneering. More pervasive problems likely to increase demands for change—a younger, more educated middle class desiring a more open society and inevitable periods of economic stress—may revive functions of grass-roots recruitment, two-way communication flow, and a renewed role for party mobilizers and communicators as well as technocrats. Tensions continue between the

need to marshal and coordinate human resources for purposes of economic development and an orderly society, and a growing public desire for individual expression and political pluralism.

Part 3 focuses on four issue areas of the late 1970s. Public housing is solved for all practical purposes. Public transportation alternatives have been considered, but the mass rapid-transit plan selected is on track. The foreign-policy chapter sets out the relevant foreign-policy patterns as they evolved since 1965 and provides an excellent basis for understanding foreign relations in the 1980s. The education analysis is candid in identifying and summarizing conflicts. This latter issue remains the least amenable to a permanent solution. Not included is a chapter on the economy, perhaps because it was not seen as an issue in 1980 or anticipated for the immediate future—which was wrong on both counts.

This book is a successful collection of essays and provides more data and analysis of a developing political system than can be found respecting many other Third World countries. Asianists as well as those interested in Third World political development look forward to a second edition and hope it will be available in paperback for use in upper-division college courses.

THOMAS J. BELLOWES

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Democracy and Pluralism in Africa. Edited by Dov Ronen (Boulder, CO: Lynne Rienner, 1986. xi, 220p. \$26.50).

Probably no scholar of African politics would question the value to participants of a small, intimate seminar on topics such as democracy and pluralism in Africa, especially one sponsored by the United States Information Agency (p. xi) with funds to include a large number of African contributors. The value of the printed proceedings of such a conference is more problematic.

This book does not address the question implicit in its title. It is not a collection of essays on the relationship between pluralism and democracy in Africa. Indeed, there is no consensus among the 18 contributors about what the two terms mean, nor does the editor seem

to have seen fit to organize discussion around any particular theoretical starting point, though he himself, in the closing chapter, defines the two terms as sociocultural heterogeneity (*pluralism*) and self-rule and self-determination (*democracy*) (pp. 194, 202). Had these two definitions constituted the theoretical starting point of the book, one might at least take pleasure in reading a serious debate. Instead, one encounters a hodgepodge of definitions and issues, most of them inadequately addressed in articles of 10 pages or less. Pathe Diagne, for example, defines *pluralism* as almost all differences among African societies, including religious, artistic, and ethical differences. Richard Sklar, in an otherwise interesting short—though perhaps prematurely optimistic—piece on the rule of law in Zimbabwe, defines *pluralism* as divided power (p. 141), perhaps more appropriately viewed as a component part of democracy. Pauline H. Baker uses the term *democracy* interchangeably with *political stability* (p. 53). The term *constitutionalism* is used rather weakly by John W. Harbeson, who appears to think that it means merely the presence of a written constitution rather than the actual subjection of government to the rule of law.

Interesting disagreements among the authors go unnoticed. John A. A. Ayode argues that in Africa, one cannot “opt for . . . aloofness because the wielders of power invade every citizen’s privacy” (p. 29), yet Ilunga Kabongo maintains that African political systems are not dictatorships but rather “inefficient polit[ies] where democracy is negatively expressed” and “the peasants always have the exit option” (p. 37). Which, one wonders, is the more accurate picture?

Two pieces address the ideologically obligatory thesis of African “tradition” and how Africa’s inherent democracy, which I would prefer to call *gerontocracy*, can or cannot be translated into modern practice. David N. Magang discusses Botswana’s successful democratic system but appears not to notice his own point that Botswana is in fact a homogeneous, not a pluralistic, society (p. 106). Lansine Kaba discusses power and democracy in the Songhay empire of the fifteenth to sixteenth centuries, with a brief conclusion that “a fair and long-term effective political system requires autonomous institutions” (p. 100). But the myth of African traditional democracy is not

addressed head-on, nor are the real elements of caste, slavery, and gender discrimination in precolonial societies discussed, although Colin Legum briefly notes, in a refreshingly frank overview, that “there are examples of both authoritarian and democratic systems in pre-modern times” (p. 179).

In general, the participants in the seminar that resulted in this book do not appear to have reconsidered their original contributions in the light of subsequent discussion. While some pieces, such as W. A. E. Skurnik’s article on press freedom, stand well alone, the volume as a whole does not cohere. Some articles are on democracy, some are on pluralism, very few discuss the relationship of the two, and no two use the same definitions. This volume is useful as an introduction to the thinking of some prominent African(ist) scholars, but it is not what the title leads the reader to hope for.

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Voters Begin to Choose: From Closed Class to Open Elections. By Richard Rose and Ian McAllister (Beverly Hills: Sage, 1986. vii, 178p. \$40.00, cloth; \$14.95, paper).

The sense of change and uncertainty in the British party system and electoral behavior is conveyed by the title of this book. The long established two-party, two-class framework accounts for a smaller part of electoral behavior. This important study draws largely on the monthly Gallup surveys and successive British General Election surveys, including the most recent for 1983. The authors argue that the widely accepted model of British voting behavior is one in which voting is *determined* by society, in this case by social class. *Indeterminate* voting is when short-term influences are more important than enduring factors like class and party identification. Party competition can be *minimal* or it can be *multiparty*. The authors combine these characteristics of electors and parties to give four types of electoral competition. Britain has moved from the *closed-class* model, in which two parties compete for a socially determined electorate to *open competition*, that is, an indeterminate electorate and many parties.

Rose and McAllister reject any single-factor

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explanation of voting, though they make use of class, party identification, and issues to provide an umbrella. Social class has become looser, the manual working class is steadily declining in numbers and in share of the population, and the Labour party is getting a steadily smaller share of a steadily declining constituency. Each social class now splits across the three parties, with the major divisions within the working class. Labour gets just over half of the unskilled working class, and the Conservatives 42% of the skilled working class and 56% of the middle class. Whichever definition of class is used, it shows a consistently weak relationship with voting.

Party identification is also declining in importance. Between 1964 and 1983 the proportion of Conservative identifiers fell from 40% to 36% while Labour identifiers fell from 43% to 31%. The weakening of the traditional moorings of the vote, class and partisanship, has increased the scope for issues. But, in view of the general agreement on many issues across voters of all three parties, Rose and McAllister reject issues as an explanation. An explanation resting upon political principles is also rejected, in view of the lack of clear party divisions on such principles as welfare, traditional morality, and racialism. The authors find little support for the neo-Marxian explanations of cleavages of public versus private consumption and production.

If so many of our traditional approaches are found wanting, what does influence voting? Rose and McAllister provide a "class-plus" explanation. They use factor analysis to set out the contribution different variables make to voting. Not surprisingly they find that each factor makes some contribution, though it is interesting to see the relative importance of political principles. What also emerges as important are class-related or economic-interest factors such as housing and trade-union membership. Within any social class homeowners and council tenants are 20% more likely to vote Conservative or Labour respectively, compared to persons without these attributes. The most solid group of Conservative voters are the nonunion owner occupiers, 62% of whom voted Conservative in 1983. The most solid Labour voters are working-class council house tenants, 61% of whom voted Labour. But the first group is 2½ times bigger than the latter and the ratio has been worsening over

time. In 1964 the former outnumbered the latter 34% to 22%; in 1983 they outnumbered them 40% to 17%. The ratio is likely to have widened even more by the time of the next election. In many ways, the alliance is now the more "typical" party in terms of its support. Its vote is minimally influenced by social structure, and on many issues alliance voters' position lies between that of voters for the other parties. Rose and McAllister wonder if we are all alliance today?

Rose and McAllister do well to remind us that class, though declining in importance, never has had the significance in Britain that the number of commentators once attributed to it. For the twentieth century and even since 1945—and the emergence of the Labour party as a majority government for the first time—a substantial part of the working class always voted Conservative. Without that "deviant" minority Britain would not have had a competitive party system and there would not have been Conservative domination. An interesting question is, What is the Labour party, the party of the working class, to do without a majority working class?

It is interesting to compare *Voters Begin To Choose* with two other analyses of the 1983 election. *How Britain Votes* (Oxford: Pergamon, 1985) by Anthony Heath, Roger Jowell, and John Curtice is the successor to the Butler and Stokes landmark study, *Political Change in Britain* (New York: St. Martin's, 1969). It argues that social class, though declining in significance, is still important. But the authors have provided a trichotomous division of the electorate into the middle, intermediate, and working class. Their own approach to social class has resulted in a reduction of so-called manual workers into a "purer" type and, not surprisingly, this type is more inclined to vote Labour than is found with other definitions of *working class*. Even then, however, they find that only 49% of the working class voted Labour in 1949. *British Democracy at the Crossroads* (Boston: Allen & Unwin, 1985) by Patrick Dunleavy and Christopher Husbans is a highly original study that draws on many concepts and insights from U.S. social science. Their own emphasis is on a so-called "radical" approach to electoral behavior in which they stress the importance of consumption cleavages. A welcome feature of *Voters Begin to Choose* is that it is written so

clearly, avoids jargon, and also steers clear of tiresome arguments with proponents of so many other approaches.

DENNIS KAVANAGH

University of Nottingham

Political Liberalization in Brazil: Dynamics, Dilemmas, and Future Prospects. Edited by Wayne A. Selcher (Boulder, CO: Westview, 1986. x, 272p. \$26.50).

After 1974, Brazil's military regime underwent a gradual process of political liberalization. The process began with President Ernesto Geisel's controlled relaxation of authoritarian rule and continued through the transfer of the reins of power by his successor, General João Figueiredo, to the civilian winner of an indirect presidential election in March 1985. *Abertura* (opening) has yet, at this writing, to develop into a fully representative democracy. This prolonged democratization poses a special scholarly challenge. Analysts of liberalization must not only chronicle unfolding events, but they must also place them in a proper theoretical context. This collection of six essays by North American authors succeeds admirably in the former, but less well in the latter, effort. Overall, it makes a valuable contribution to a growing literature.

Three of the essays present detailed accounts of 1977-85 politics, the period in which the regime's highly controlled *distensão* (decompression) strategy gave way to pressures from below and without, resulting in an *abertura* with its own momentum. Enrique Baloyra's essay focuses on the contradiction between the military as a professional institution and as an incumbent regime. Baloyra places inordinate importance on tensions between the presidential advisory group and the military high command as the crucial determinant of both Geisel's and Figueiredo's strategies. This misplaced emphasis on internecine military struggles rather than on the interplay of political, social, and economic factors may stem, in part, from the author's heavy reliance on English-language sources, especially *Latin America Report*, often regarded as a font of unsubstantiated political gossip. Baloyra contributes, nonetheless, a number of astute observations. Most intriguing is his com-

parison of Mexico and Brazil. Mexico's military leaders created a strongly associated, but distinctly civilian, party apparatus that could engage directly in politics without losing its institutional identity. The Brazilian military administrations were never as successful in using their own party, Partido Democrático Social (PDS), to fight their political battles. By entering the political realm directly, the military thus compromised its own institutional professionalism.

The essays by Wayne Selcher and David Fleischer cover much the same ground as Baloyra. Selcher gives a lucid and well-informed account of the 1979-85 period, assigning more responsibility for Figueiredo's mismanagement to the onslaught of economic recession, heightened internal social and political demands, and increasing international pressure for austerity measures to cope with the foreign debt. *Abertura* was pushed beyond a purely institutional reform to address larger social-reform issues, and Figueiredo was forced to try to balance authoritarian and democratic forces in an economic crisis within a socio-economically polarized society. Fleischer focuses more narrowly on the role of the congress and political parties. By describing the casuistry utilized by the government to assure PDS domination in successive municipal, state, and federal elections as well as the presidential succession fight, Fleischer provides a uniquely microscopic view of political procedural mechanisms.

Robert Packenham traces the changing nature of political discourse among Brazilian academics and intellectuals from 1964 to 1985, a discourse that shifted from Marxist domination in the repressive 1960s and 1970s to a more pragmatic *postabertura* liberalism. Much of the essay fixes on the transformation of the work of Fernando Henrique Cardoso, a Brazilian sociologist-turned-politician with whom Packenham has long jostled. Packenham's arguments are interesting in their exploration of the somewhat neglected topic of Brazilian intellectual history. To integrate them more completely into this volume, however, the connection between intellectuals and events occurring in the larger polity itself needed to be more clearly established.

Ronald Schneider attempts to predict Brazil's political future by calculating the changing shares of power held by various

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political actors. These calculations, based on Schneider's 30 years of study of Brazil, are purely subjective. Schneider is to be commended for his effort to measure specifically what most authors assess with broad brush, but his methodology is problematic. Not only are the 29 categories of political actors not mutually exclusive, but one cannot assume that at any given time all actors are fighting over the same issue. The political arena is multidimensional, and political actors may be interested in any number of issues with varying degrees of intensity.

By far the best contribution to this volume is Donald Share and Scott Mainwaring's comparative analysis of political liberalization in Brazil and Spain. The essay argues that both cases embody "transitions through transaction" in which the regime itself initiates the transition, establishes some limits to political changes, remains a significant electoral force during the transition, and negotiates directly with the democratic opposition. Unlike Portugal's transition through regime breakdown (1974) or Peru's transition by extrication (1980), Brazil and Spain both democratized through carefully paced, staggered reforms. The regime excluded selected opposition groups, ruled out both extensive socioeconomic change and punitive measures against former regime members, marginalized extreme groups, and sought to maintain sufficient popular support so that it could compete electorally. The analogy between Spain and Brazil is occasionally overdrawn, but the authors present a well-organized analysis and provide a useful theoretical prism through which to view liberalization.

Overall, this book offers, for the first time in English, a thorough account of political liberalization in Brazil. Several of the essays tread the same ground, however, and one wishes the editor had included an economic or sociological perspective to counter the heavy emphasis on political factors. With the exception of the Share and Mainwaring essay, which makes a more general contribution to theories of comparative politics, the articles are primarily of interest to those who follow contemporary Brazilian politics, both in and out of the scholarly community.

BARBARA NUNBERG

World Bank

Government Violence and Repression: An Agenda for Research. Edited by Michael Stohl and George A. Lopez (New York: Greenwood, 1986. viii, 278p. \$35.00).

Political scientists will not understand Third World politics until they can explain governmental violence and repression. Miles Wolpin notes, "In short, an absolute majority of the population in the developing countries survived under regimes characterized by torture, execution, disappearances, and brutal prison treatment of those suspected of opposing the government" (p. 100). How can one talk about interest articulation or aggregation where every opposition voice is smashed and made speechless? Michael Stohl and George Lopez, first in *The State as Terrorist: The Dynamics of Governmental Violence and Repression* (Westport, CT: Greenwood, 1984) and now in the present volume, have brought together an excellent group of writers to begin to fill the lacuna left by Western social scientists' poor perception of what has been going on in the Third World (and also in the First and Second Worlds).

The previous blindness to what should have been obvious to everyone had its reasons, and one was the lack of conceptual tools. Given that the existing conceptual tools failed to take into account governmental violence and repression, how should one start to develop the proper equipment for studying the subject? Some things can be done by just working on the concept in question: defining it, suggesting how to measure it, estimating values and running correlations, postulating hypotheses, and so on, and this volume does a good job here. In the long run, however, we will have to frame a picture of the social and political world that integrates force, coercion, and terror into a wider view of how politics works. While the book makes a few suggestions, most notably in essays by the editors, it still leaves much for future writers.

Several essays use the notion of utility maximization in a decision model, which provides insight but holds dangers and blind spots as well. The insight is that governments are trying to accomplish something when they kill people. The bias is that it makes the decision to terrorize appear too rational, ignoring entirely the very interesting psychological work from Erich Fromm and T. W. Adorno to Milton

Rokeach and now Robert Jay Lifton. It also makes states seem too much as unitary actors, which, if it were the case, would leave no occasion for repression. Hiding in the shadow of this kind of analysis is the metaphor of a polity as an organic body that must rid itself of disease or cancer, such as is exposed in Lopez's discussion of the national-security doctrine. Social utility cannot exist as it is impossible to aggregate individual utilities, and it could not be arrived at if it did exist, given the logical problems of collective action and the paradox of voting. Utility maximization as a rationale for governmental decisions masks their partisan and self-serving character. None of the authors mentions that the most brutal governments are also exceedingly corrupt.

These essays straddle another conceptual problem—the level of analysis. Most of the writing assumes that since the state carries out the violence, the cause of the violence should be at the level of the state or its constituent parts. For instance, Lopez writes about the national-security doctrine in Latin America with no reference to the predominant role of the United States in military training on the continent since 1948. On the other hand, Wolpin in an exhaustive analysis of variance confirms Ruth Sivard's conclusion: "All of the worst offenders are clients of one or the other superpower" (p. 120), and Stohl, in the most interesting conceptual development of the book, points out that much international-relations theory and military strategy portrays the international world in such a way that one should expect intense conflict within countries in the Third World. One of Gurr's 14 hypotheses reflects the same consideration: "Regimes involved in proxy big power conflicts are likely to use the most extreme forms of violence against challengers, including state terrorism" (p. 60).

One has to turn to two other essays to see the correct implication of this tie between international relations and state violence. Grant Wardlaw shows how terrorism breeds counterterrorism if democracies are not alert, which is just what the terrorists want. Robert Friedlander points out that the only real alternative to international terror is that "implausible dream," international law.

JOHN F. McCAMANT

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Religion and Modernization in Southeast Asia.

By Fred R. von der Mehden (Syracuse, NY: Syracuse University Press, 1986. viii, 240p. \$29.95, cloth; \$14.95, paper).

In this volume Professor von der Mehden returns to some of the topics discussed in his *Religion and Nationalism in Southeast Asia* (Madison: University of Wisconsin, 1963), but he has broadened the inquiry to grapple with the entire range of modernization issues. He is primarily concerned with Theravada Buddhism in Burma and Thailand, Islam in Malaysia and Indonesia, and Roman Catholicism in the Philippines, although mindful of the situation and role of minorities in all these countries. In the early chapters, the author emphasizes the lack of scholarly consensus on modernization theory, but whatever the conceptual and methodological problems, he regards some notion of modernization as indispensable. The core elements of modernization in his definition are "technological development and the maintenance of a modern nation-state" (p. 48).

In this balanced and judiciously argued study, the author asserts that Western scholars since 1945, like some of their nineteenth-century predecessors, have permitted secularist bias to lead them to the too sweeping conclusion that religion is a massive obstacle to modernization in Southeast Asia. The indictment of religion charges that it instills otherworldliness or fatalism, diverts economic resources from development, supports a non-productive clergy, stirs up communal conflict, and in general upholds traditionalism and opposes change. While the book contains evidence to give partial support to all of these charges, the author's aim is to show that the "massive obstacle" generalization is seriously flawed. The undifferentiated dismissal of, or attack on, religion obscures too much.

The author is surely on the right track in insisting that the big question be divided into manageable parts. And throughout the book he demonstrates exemplary restraint in providing answers; in many cases the empirical evidence is ambiguous or simply lacking. Methodological problems abound, particularly in trying to link formal religious tenets with the behavior of adherents of that religion. Even when dealing with more concrete contemporary religious institutions—the hierarchy,

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clergy, and lay organizations—the author is most impressed by the lack of a clear direction. “In sum, although it is difficult to generalize about the role of religious institutions in the process of modernization, recent years have seen expanded efforts toward modernization by both lay organizations and clergy. Technological change remains only a marginal interest to the vast majority of the religious in Southeast Asia, but changes are underway” (p. 98). The reader might wish for a stronger conclusion, but these two sentences accurately summarize the facts presented in the chapter.

The author weighs the pros and cons of religious expenditures as they affect modernization and finds that they are fairly well balanced. The Western critique of popular religious beliefs, he asserts, has frequently missed the point: “At many levels religious belief has been a force in delaying change, but very often as an indistinguishable part of a more complicated process” (p. 201). The author deals with the political manipulation of religious symbols to forge national unity or to foster modernization, and finds that these efforts have frequently been ineffectual and counterproductive. While the influence of religion has been weakened by the forces of modernization, there is no inherent and inevitable conflict, and religion continues to provide identity and security for many educated youth in Southeast Asia.

Von der Mehden has made an important point in this book. The notion of religion as “massive obstacle” has been on the wane for at least a decade and a half but this is the first effort to subject it to systematic analysis with regard to Southeast Asia.

One is left, however, with the sense of an

important question not addressed. Surely Buddhism, Islam, and Catholicism are radically different religious systems. One would expect them to have significantly different socioeconomic-political effects. Yet there is little indication in this book that modernization is likely to fare better interacting with one religion than with another, nor is there any systematic analysis of the differing points of tension in the three faiths. The special character of Southeast Asian Islam may have something to do with the disinclination to make comparative statements. With syncretism in Indonesia and communal identity in plural Malaysia, generalizations about Islam do not work nearly as well in Southeast Asia as, say, in Pakistan. Nevertheless, useful comparative statements can be made.

In terms of political modernization, Buddhism, without a divine law to regulate society and with little inclination toward the formation of political ideologies, presents few problems, whereas the notion of man-made laws in a modern legislative state will continue to encounter serious opposition in Muslim societies. Origins sometimes explain little, but it is of continuing relevance that the Buddha was a prince who fled from political power in the pursuit of individual salvation, while the revelation given the Muhammad concerned the building of a total divinely ordered state and society. This is not to suggest that these differences and manifest tendencies are necessarily the most important facts, but they are a part of the overall picture.

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INTERNATIONAL RELATIONS

South Asian Insecurity and the Great Powers.

By Barry Buzan and Gowher Rizvi, with Rosemary Foot, Nancy Jetly, B. A. Roberson, and Anita Inder Singh (New York: St. Martin's, 1986. xii, 257 p. \$32.50).

We political scientists spend too much time reinventing the wheel. In this case the wheel is

called a *security complex* (p. 8). It bears a striking resemblance to groupings of states that have otherwise been called *subordinate systems*, *subsystems*, or *regional subsystems* since Leonard Binder wrote a pioneering article, “The Middle East as a Subordinate International System” (*World Politics* (NJ) 10 [1958]: 408–29). Subsequently, Michael

Brecher applied a similar approach to the object of study now of concern to Buzan et al. ("International Relations and Asian Studies," *World Politics* (NJ) 15[1963]: 213-35). Taking the perspective a step further, Louis J. Cantori and Steven L. Spiegel published in 1970 a comparative study of subordinate systems, *The International Politics of Regions* (Englewood Cliffs, NJ: Prentice-Hall).

To be fair, Buzan does cite Brecher and Cantori and Spiegel in his introductory chapter ("A Framework for Regional Security Analysis") but implicitly dismisses them as inadequate or at least unworthy of replication. By identifying Cantori and Spiegel with "the perspective of integration" (p. 6), Buzan reveals a lack of knowledge not only of the body of Cantori's and Spiegel's regional-systems work but a lack of appreciation of the distinctiveness of the regional-systems approach from integrationist, transactional, and other approaches to regional study. That Cantori and Spiegel distinguish their approach from the integration perspective could not be more definitively stated than in the title and content of their article, "The Analysis of Regional International Politics: The Integration versus the Empirical Systems Approach" (*International Organization* 27 [Autumn 1973]: 465-94). The regional-systems analyst is concerned with disintegrative as well as integrative variables, with conflict as well as cooperation.

In short, there is a body of regional-systems literature built upon and around the tradition of Binder, Brecher, and Cantori and Spiegel of which Buzan et al. were either unaware or which they chose to ignore in their joint work on South Asia. Concepts from that body of work, much of it security oriented, could have served them well. For example, the Cantori-Spiegel notion of *core* (*International Politics of Regions*, 20) could have been used with much more flexibility than Buzan's *security complex*. A regional subsystem can have by definition (and in "reality," as Buzan properly requires) more than one core, whereas Buzan's analogous nomenclature for a multicore region is that it is a *complex of complexes* (p. 11) or a *supercomplex* (p. 12). The Middle East, with Gulf, Arab-Israeli, and Maghreb cores or security complexes is an example. Furthermore, the Cantori-Spiegel concept of *intrusive system* (*International Politics of Regions*, 25) is

much more elegant and much less confusing than Buzan's *superregional level* of analysis (p. 15), which, he admits, causes him problems in deciding what to do with China: "A case could be made for including China on the global level. . . . The odd position of China means that some confusion will result no matter which level it is included within" (p. 17). It is not confusing to include China in the intrusive system, as Cantori and Spiegel specifically recommend.

While Buzan's introduction is therefore of little use to the international-relations theorist because it neither builds upon long-established conceptual foundations nor, in my view, offers a better alternative approach to regional-security studies, Buzan and Rizvi do deserve credit for assembling a collection of pieces of considerable interest to South Asia security specialists. The substantive chapters (2-8) are generally up-to-date, well presented, and evocative of fresh insights into the multilevel security relations in which South Asian states are involved.

Furthermore, the final chapter by the two principal authors, "The Future of the South Asian Security Complex," does develop useful standards for judging prospects for future system transformations in the area. Buzan and Rizvi convincingly argue that even a change in external power alignments (though unlikely) "in which the Soviet Union is aligned with Pakistan, and China and the U.S. are aligned with India [following, for example, a flip-flop similar to the one that occurred on the Horn of Africa]. . . . from the perspective of our models . . . would not disturb the continuity of the status quo within South Asia. . . . Nothing could illustrate better than this speculation the impact and importance of the local and regional patterns of security relations in the larger picture of international relations as a whole" (p. 239). Nothing indeed. And that is perhaps the greatest overall virtue of this book. It serves to remind globalists and would-be globalists that by ignoring or downplaying regional dynamics they miss much of the essence of world politics.

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In Whose Interest? International Banking and American Foreign Policy. By Benjamin J. Cohen (New Haven: Yale University Press, 1986, xi, 347p. \$19.95).

There has been a substantial resurgence of interest in political economy in recent years. This has been particularly noticeable in the area of international economic relations, where economic issues (initially relegated by political scientists to the realm of low politics) are increasingly recognized as politically salient. At the same time, economists are increasingly willing to acknowledge the political constraints that influence economic policy-making, as well as the important interdependencies among economic, security, foreign-policy, and political considerations.

Benjamin J. Cohen, the William Clayton professor of international economic affairs at the Fletcher School of Law and Diplomacy at Tufts University, has been one of the leading contributors to the "new" field of international political economy. *In Whose Interest?* is of the same high quality as his previous important books, *The Question of Imperialism* (New York: Basic Books, 1973) and *Organizing the World's Money* (New York: Basic Books, 1977). Here he treats the complex and increasingly important nexus of issues involved in international banking and U.S. foreign policy. After a useful and aptly titled introduction, "High Finance/High Politics," Cohen presents a fascinating set of chapters on the historical evolution of international banking and an important series of case studies of U.S.-policy episodes involving the Arab money weapon, the freezing of the Iranian financial assets in the United States during the hostage crisis, the Polish debt question, and the current international debt crisis. In his last chapters, he turns to policy recommendations for dealing with this debt crisis and for improving decision-making processes.

Cohen writes from a sensible, middle-of-the-road perspective. He sees most issues in shades of gray; channels of influence are multidirectional and complex. Banks are neither the all-powerful villains of popular Marxist literature nor the ideal performers of efficient-markets theory. Banks, he argues, have fallen prey to disaster myopia with the consequent overlending that is one, but far from the only, important reason for the current international debt

crisis. Government is viewed sympathetically, with officials generally striving to do good, but the efficiency of policy initiatives is often undermined by poor planning or bureaucratic infighting, or both.

Despite the sensibleness and lack of drama in Cohen's perspective and recommendations, the reader is caught up and carried along by the writer's skills. A cynic might belittle his suggestions, such as a better information-gathering and planning system and the creation of a structured dialogue between the government and banks, but as one who spent a number of years in government service, I can attest that these are messages well worth repeating. What could be a list of platitudes in the hands of some become meaningful lessons through Cohen's enlightened use of historical examples.

Can we hope to improve our decision-making processes systematically and significantly? Having seen numerous reforms of the formal economic-policy coordination mechanisms within government undertaken with few noticeable results achieved, I have become skeptical of the role institutional innovation can play in this area. Perhaps more important are efforts to encourage longer-range thinking and sensitivity to the complexity of issues, but even here the role of institutional innovation is limited. In the 1970s I directed Treasury's international research and planning staff, which was created for just such purposes. It was actively used by one secretary, largely ignored by a second, and abolished by a third. Thus I am inclined to feel that what is most important is the attitude of senior officials toward the need for good policy analysis. Making Cohen's book mandatory reading for all policy officials would be a good beginning toward improving our policy-making process.

THOMAS D. WILLETT

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Foreign Aid, War, and Economic Development: South Vietnam, 1955-1975. By Douglas C. Dacy (Cambridge: Cambridge University Press, 1986. xix, 300p. \$39.50).

Many political scientists see the Vietnam War as the beginning of the end of the United

States' primacy, as the first symptom of its later spastic efforts to regain hegemony by relying on a foreign policy designed almost exclusively around the use of force. But it was more than that (if indeed that is what it was). It was also a test of theories and hopes about the potential of foreign aid as an instrument for the promotion of political and economic development in friendly countries, as an indirect contribution toward the creation of an unthreatening and stable world order. Most books about Vietnam center about the first set of issues; Dacy's book is a thorough and convincing exploration of the second.

There are special circumstances in the Vietnam experience that have discouraged economists and political scientists from drawing generalized conclusions about development. The huge overlay of U.S. foreign aid dominating the economy, the crescendo of rising guerrilla activity culminating in outright war, the hesitant leadership facing apparently insatiable demands for reform, and the absence of the aura of legitimacy, all so overwhelmed the processes of development that the lessons are hard to see. But other nations have faced similar problems, in various combinations. Windfalls like mineral discovery or sudden rise in oil prices, ethnic and ideological unrest, inexperienced governments, and unhistorical claims to power are commonly found equivalents to the special circumstances of Vietnam. Dacy shows how the Vietnamese effort is informative both about the processes of development and the potential application of foreign aid on their behalf.

This carefully documented economic history makes it clear that Vietnamese economic progress during the early years of independence (1955-59) was better than generally reported, though far from sufficient, and that it was political failure that robbed the population of what fruits it produced. The best that foreign aid could do as military costs rose was to check inflation and to encourage an occasional economic reform in the direction of liberalization. One of the by-products of foreign aid—the black market—contributed to both outcomes. A surprising finding is that the major spurt of economic development seemed to have occurred in the wake of the unsuccessful Tet offensive, whose consequences stimulated and liberalized both the economy and polity for a time until the withdrawal of U.S. troops

and the full-scale invasion from the North took their toll.

Economic-development experience in the context of international and domestic political uncertainty is a neglected area of empirical research. It is fortunate that it has been so well carried out here. Major economic decisions, such as the tactics of devaluation during a period of inflation, tax measures designed for redistributive purposes in the context of rapid financial turnovers, the fixing of interest rates to encourage savings, the use of parallel foreign-exchange structures for different imports, the consequences of commodity subsidies, and even the consequences of land reform, are all carefully explored. There is an ingenious use of data to estimate such features of the Vietnam experience as comparisons of growth rates across sectors, the effects of the Commercial Import Program on industrial development, the economics of increased "dependency" associated with different aid levels, the monetization of the economy as an indicator of development, changes in farmers' income associated with land reform, and contrasts between rural and urban income changes resulting from different policies. The data expose some absurdities in economic convention, such as the interpretation of foreign aid as "savings" and the theory that control over foreign-exchange levels could offset the inflationary effect of the very presence of U.S. troops. Perhaps the last persuasive argument appears in the final chapter of the book, in which Dacy tries to show how a military threat can contribute to development if a country is receiving foreign aid. This demonstration consists of a pairing of Israel, Taiwan, Korea, and Vietnam with Latin American countries that were "nonthreatened" and "moderately aided," to show that Vietnam was the odd man out in not developing more impressively. But the conclusion, that the relative failure came about because neither the government nor the donor really placed development as a major priority, is fully justified by other data presented in the book.

The reader will find much to learn from the Vietnamese economic experience, and though the achievements of the foreign-aid program are fully documented here, no one will be likely to conclude that the U.S. presence in the later years of independence was a manifestation of superpower, or even great-power,

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wisdom. This is not a revisionist tract, only a reexamination of an experience from which U.S. citizens have learned all too little.

On the dust jacket, the publisher expresses the modest hope that this book could be used as supplementary material for courses on economic development at both undergraduate and graduate levels. That is a reasonable hope. Courses that make use of this rich case study will be the better for it, as indeed will foreign-policy makers.

JOHN D. MONTGOMERY

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Perceived Images: U.S. and Soviet Assumptions and Perceptions in Disarmament. By Daniel Frei (Totowa, NJ: Rowman & Allanheld, 1986. xvii, 323p. \$26.50).

Daniel Frei correctly asserts that we need a careful, document-based description of Soviet and U.S. perceptions of one another and of the conceptions on which their approaches to security are based. This volume does not even begin to satisfy this need. It is little more than a string of quotations from official documents cited out of context to present the erroneous impression that both superpowers are monolithic in their respective points of view.

Frei bases his "analysis" on official documents by arguing—really proclaiming—that these are indicative of what policymakers in Moscow and Washington really believe. A careful scholar who chose his documents wisely and interpreted them in context might make a good case that they offered insight into policymakers' minds on a carefully defined range of issues. Frei is utterly insensitive to any of these methodological concerns. Take the documents he uses. For the United States, these are the annual reports of the secretary of defense, U.S. military-posture statements, and press releases from the State Department's Bureau of Public Affairs. These are the *least* likely sources to reveal thinking of officials. They are self-conscious public-relations gestures designed to sell military appropriations to the Congress or justify specific administration foreign policies to elite opinion in this and other countries. Any correspondence between documents of this kind and what policymakers really believe is purely coincidental. Their use

is doubly inexcusable in an open society like the United States, where much better documents are available for analysis.

Documents must be interpreted within their proper context. Pronouncements, especially public ones, are always designed to influence particular audiences. Very little can be inferred from such a statement unless one knows who made it, the audience it was aimed at, and the political consideration at the time, internal and external, that influenced its substance, tone, and timing. Students of Soviet and U.S. foreign policy routinely engage in this kind of analysis. Because public documents are all that is generally available in analyzing Soviet policies and decision, Soviet scholars have been forced to develop particularly sophisticated methods for making inferences from their data. Frei makes no attempt to do this or to build on the work of others who have. Instead, he treats all statements as equal in value and equally indicative of what their authors believe. This is either naive or irresponsible.

Finally, there is the question of consensus. Frei portrays both superpowers as unitary in their thinking. This stands in sharp contradiction to the position of numerous highly regarded studies of Soviet and U.S. strategic and foreign policy. They indicate that strategic policies, the view each superpower has of the other, and their respective attitudes toward nuclear war and arms control have evolved considerably over the years. These studies also indicate that there are often substantial areas of disagreement and controversy within the Soviet and U.S. governments at any particular time. The best and most useful analyses are those that document these controversies and use them to shed light both on the substance of policy and the process by which it is made in the two political systems.

Perceived Images is a sham. It pretends to be something that it clearly is not. Surely, the United Nations Institute for Disarmament, which sponsored the study, ought to be able to do better than this.

RICHARD NED LEBOW

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Development Strategies Reconsidered. Edited by John P. Lewis and Valeriana Kallab (New Brunswick, NJ: Transaction Books, 1986. viii, 190p. \$19.95, cloth; \$12.95, paper).

The conventional wisdom in the major international development agencies today is that government is largely responsible for the Third World's disappointing economic performance and that markets are the remedy for most of these problems. Even within the mainstream, however, there remains a healthy degree of skepticism about these neoclassical views, so the Overseas Development Council (ODC) in 1985 brought together several experts to review the current state of development strategy. Eight papers emerging from that conference are reproduced here, the fifth volume of ODC's series on United States-Third World policy perspectives. While none denies the importance of markets, each points out limitations in the laissez-faire paradigm for promoting development and reminds us that governmental and political factors are also critical.

Coeditor John Lewis begins the collection with an overview of the ongoing debate. His spirited defense of foreign aid, which will be familiar to those acquainted with Lewis's work, is guardedly optimistic about planned promotion of economic growth and social equity. In the next essay, Irma Adelman focuses explicitly on the equity issue, and calls for specific programs to expand access to land and education as the first step in development. Equally important, in her view, Third World states need to adopt economic policies that have a high labor content, which means basing development on agriculture or, alternatively, on exports.

John Mellor picks up the agricultural theme in the next paper, renewing his classic argument about how the rural sector can support an employment-based strategy of growth. Government has a major contribution to make by providing public goods such as research, education, and infrastructure. Interestingly, Mellor sees mutual enrichment, not conflict, between an agriculturally oriented strategy and one led by exports.

A noted proponent of export-led development, Jagdish Bhagwati, offers the next paper. Of all the contributors, Bhagwati is most sym-

pathetic to the neoclassical orthodoxy, though he sees virtue in public-private partnerships because they can help assure a stable policy environment. The thrust of his paper, however, is to refute arguments that export-oriented strategies have become less feasible under current world market conditions. Most telling is his observation that outward-looking countries have tended to adjust better than inward-looking ones to the economic shocks of the 1980s, a point reiterated in Leopoldo Solis and Aurelio Montemayor's essay. Colin Bradford, however, warns in his paper against simplistically applying the export-led model, which he shows to be based partly on a misreading of East Asia's experience. He points out that in countries such as Taiwan and South Korea, government intervention and import substitution actually have contributed to the industrialization drive.

In the next essay, Alex Duncan turns to the issue of how donors can help the poorer countries adapt more readily to changes in the global environment, which he feels necessitates greater national capacity for policy analysis and project implementation. According to him, economic assistance should be devoted more directly to improving management and institutions, to enhancing human resources, and to increasing the supply of technology.

The final contribution to this collection is by Atul Kohli, the sole political scientist represented. He disputes the prevailing opinion that authoritarian government is a necessary condition for economic development, pointing to the favorable performance of several democratic developing countries. Kohli is pessimistic about the spread of democracy in the short run, however, the major barrier, in his view, being the fragmentation and competition of Third World elites and not, as the neoclassical paradigm often postulates, the mobilization of the masses.

All the essays collected here are cogent, lively, and informative. Nonspecialists, for whom this volume is particularly intended, will find it a useful introduction to the current debate about development strategy.

ARTHUR A. GOLDSMITH

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Essays in International Law in Honour of Judge Manfred Lachs: Études de Droit International en L'Honneur de Juge Manfred Lachs. Edited by Jerzy Makarczyk (The Hague: Martinus Nijhoff, 1984, 737p. \$108.00).

Manfred Lachs has had a 40-year career in international law, as diplomat, scholar, teacher, and, most recently, judge on the International Court of Justice (ICJ). His interests include disarmament (as architect of the Nuclear Free Zone in Central Europe in 1958); peaceful use of outer space (as chairperson of the UN Legal Subcommittee); legal control over local government; and international dispute settlement under the ICJ. The 50 essays in this volume (22 of them in French) reflect this diversity of both subject matter and authors' backgrounds (writers, scholars, practitioners, judges, and jurists from North America, Mexico, France, Great Britain, Poland, Finland, and the USSR, to mention a few).

Several articles should be of special interest to U.S. professors of international law. Jennings ("Teachings and Teaching in International Law"), following Lachs's footsteps, emphasizes that international law cannot be abstracted from diplomatic history or international political-economic events (p. 129). Similarly, McWhinney ("The Time Dimension in International Law, Historical Relativism and Intertemporal Law") discusses the conflict between the old and new in legal doctrine, the status quo versus the "newer political forces oriented to changing law in accord with changing society" (p. 180), and the ICJ decisions that have embraced Intertemporal Law, including *Namibia Advisory Opinion* (1971) and *Western Sahara Advisory Opinion* (1975). For ideological contrast, students might consult Tunkin of Moscow University ("Contemporary International Law—A New Historical Type of International Law") for a thorough but unoriginal exposition of historical types of international law, from slave-owning societies, to feudal and bourgeois law, to contemporary law defined in terms of the transition from capitalist to communist law.

In other articles, the specific role of the ICJ and international organizations is explored. For example, Elias ("How the International Court of Justice Deals with Requests for Advisory Opinions") dissects the relevant legal

and political criteria for the organs requesting advisory opinions, with special attention to the *Reparations* case. On a more explicitly political level, Franck ("Finding a Voice: How the Secretary General Makes Himself Heard in the Councils of the Nations") explores cases of the secretary's intervention and refusal and the legal precedent and procedures for such choices.

Finally, two substantive issues of most concern to Lachs are evaluated. Castaneda ("Negotiations on the Exclusive Economic Zone at the Third United Nations Conference on the Law of the Sea") offers a fascinating inside account of the negotiating process. And Blix ("Arms Control Treaties Aimed at Reducing the Military Impact on the Environment") emphasizes the Environmental Modification Convention, 1977, discussing the applicability of various provisions.

Numerous articles are of sufficient merit to the international-law community that library purchase is encouraged. Unfortunately, there is little concerted effort to integrate the material by broad themes. The Prott article ("The Judicial Philosophy of Manfred Lachs") might have provided some of the needed integration, especially had it been placed at the beginning or the end. Readers of books containing such diverse articles deserve to have the basic themes elucidated and my reading of the book suggests that this would not have been an unsurmountable task for the editor.

KAREN MINGST

University of Kentucky

Nuclear Ethics. By Joseph S. Nye, Jr. (New York: Free Press, 1986. xiii, 162p. \$14.95).

In this small book the author wrestles thoughtfully with one of the major moral problems of our age, maybe the moral problem of our age: is nuclear deterrence proper?

He is uncomfortable with the "self-indulgent" moral absolutists who see nuclear deterrence as unquestionably wicked and so move quickly to some sort of unilateralist or abolitionist position with, for Nye, too little concern for the dangers or practical realities. But he is also unpersuaded by the too cynical exponents of an amoral *realpolitik*, those "realist" strategists who want to minimize

moral concerns in foreign-policy making and so tend to sacrifice at the outset the Western values they claim they have adopted nuclear deterrence to protect.

Nye urges a middle course. Policy should be influenced by moral principles, policymakers by moral philosophers. The teachings of both the Kantians, with their rule-oriented perspectives, and the utilitarians or consequentialists, with their act-oriented perspectives, should be consulted. Leaders, strategists, and citizens should weigh all three essential dimensions of sound moral reasoning: ends (or motives), means, and consequences (likely results). Nye's recipe and the maxims he derives are solidly in the "just-war" tradition.

The only motive, or *end*, that can warrant possessing nuclear weapons is the "just cause" of self-defense via nuclear deterrence. But self-defense is given a pretty elastic definition; Washington is entitled, Nye argues, to use nuclear threats to defend not only the mere physical survival of the United States and its citizens but also its allies and friends, its "culture" and democratic institutions, and the current global geopolitical equilibrium (pp. 42-49). Such a broad definition of *self-defense* might license nuclear threats to protect either those far-flung "vital interests" so often invoked or even the overseas commercial investments and operations so important to the affluent U.S. lifestyle. Moreover, with such a definition, Nye or a follower of his maxims might even approve nuclear threats with compellant rather than deterrent intentions, threats designed to compel or persuade Moscow to *do* something (exit Africa, say) rather than to deter Moscow from aggression.

The key criteria to help people decide about the propriety of *means* in this arena are the criteria of proportionality and discrimination. Nye argues that nuclear deterrence can meet these tests. Nuclear weapons could conceivably be used in some limited ways such that their use would not be judged as disproportionate by many, though Nye is probably too sanguine about the possibility of controlling escalation up the ladder to clearly disproportionate levels. And if we concede that in the twentieth century the discrimination (or non-combatant immunity) test has already been relaxed to require only the minimization, not the complete avoidance, of civilian casualties, then some usages of nuclear weapons (at sea or

against remote military bases) could comply with the discrimination criterion almost as well as many standard (and apparently widely acceptable) usages of highly lethal conventional weaponry.

Consideration of the *consequences* of reliance on nuclear deterrence involves speculations about whether it really deserves the credit it usually gets for having prevented Soviet aggression that would otherwise by now have occurred, more speculations about risks and possibilities for the future, and then yet more speculations about possible psychic and political and economic effects of reliance on nuclear deterrence. Nye points up the hazards of all this speculating. But given his earlier insistence that we "start with a strong presumption in favor of rules and place a substantial burden of proof upon those who wish to turn too quickly to consequentialist arguments" (p. 23), one is left to wonder if he would really accept all these mushy speculations as "proof" tending to support reliance upon nuclear deterrence on consequentialist grounds.

Nye concludes with the usual "owlish" arms controller's suggestions for achieving "balanced deterrence" in order to reduce the risks of nuclear war during the next decade or two or three. And he urges the standard political and diplomatic approaches for improving U.S.-USSR relations in order to enable a gradual reduction of our reliance on the primitive and still morally troubling instrument of threatening catastrophe.

JAMES A. STEGENGA

Purdue University

The New Order of the Oceans. Edited by Giulio Pontecorvo (New York: Columbia University Press, 1986. 277p. \$30.00).

Covering some 70 percent of Earth's surface, the world's ocean areas are of significance in terms of food, energy, and mineral resources, as a site for waste disposal, a medium for the transit of the vast bulk of international trade, and as an arena for military operations. In recognition of the importance of ocean areas, three major multilateral diplomatic conferences under the auspices of the United Nations have been held since 1958 to determine

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the future legal and political order of ocean space. In light of changing technological and political circumstances, the Third United Nations Conference on the Law of the Sea (UNCLOS III) met for a decade and finally adopted a new comprehensive Law of the Sea Convention in 1982. Treating a vast array of ocean rights and duties, this treaty is of substantial complexity and embodies a mixture of both existing and new rules of international ocean law. From the perspectives of allocation of ocean rights and duties and the development of the international legal system, UNCLOS III ranks as one of the major events since World War II. In terms of broader aspects of international relations, it marks perhaps the first major diplomatic conference at which Third World states played a full and independent role in the shaping of a significant part of the international legal system.

The New Order of the Oceans, edited by Professor Giulio Pontecorvo of Columbia University, provides a selection of essays by a distinguished set of experts and diplomats on the resources of the ocean, ocean science, the development of the new ocean regime from the perspectives of Africa, Latin America, Southeast Asia, and the United States, and a speculative essay on the unfinished business of UNCLOS III. Though little of the substantive material in this book is new to specialists in the field, this volume does provide a good review of the major issues and problem areas treated at UNCLOS III. All of the essays are well written and informative; some are outstanding in their insights.

The issues surrounding deep-seabed mining ultimately led the United States government to refuse to sign the 1982 Law of the Sea Treaty. Accordingly, it is fascinating to read the essay by Marne Dubs, former industry executive and expert in ocean mining, who shatters a variety of myths associated with such mining, myths which contributed to rigid negotiating positions on the part of both the United States and a number of developing states. Surveying the current situation, he finds that the United States lead in ocean mining has withered away and that, diplomatically, the United States has been outmaneuvered by Japan and France.

Professor Thomas Clingan of the University of Miami Law School and vice-chairman of the United States delegation to UNCLOS III, provides a thoughtful and revealing assessment of

U.S. policy. Even in the early days of the conference, U.S. policy was driven not simply by navigational issues but also by concern with the resources of the continental shelf and the exclusive economic zone. Issues relating to deep-seabed mining were not seen to be of major economic or strategic importance and, Clingan asserts, were manipulated skillfully to draw attention away from matters perceived as having more fundamental significance such as those concerning navigation rights, continental shelf, and the exclusive economic zone. With the advent of the Reagan administration, seabed mining took on a new importance, however. Ambassador Djalal of Indonesia, in his contribution to this volume, sees this change as exemplifying this Reagan administration's "insatiable appetite" for having things its way at UNCLOS III.

In the closing essay of this book, Professor Jonathan Charney of Vanderbilt University Law School examines the important theme that a considerable amount of unfinished business remains in the wake of UNCLOS III and that the evolution of the law of the sea is a continuing process. While the drama attendant on UNCLOS III may be lacking in the postconference world, important developments are ongoing. The law of the sea remains a significant issue in contemporary international relations and *The New Order of the Oceans* contributes to our understanding of ocean law and politics.

LAWRENCE JUDA

University of Rhode Island

U.S. Trade Policies in a Changing World Economy. Edited by Robert M. Stern (Cambridge: MIT Press, 1987. 437p. \$25.00).

This book is a nonmathematical survey of the contemporary trade theory and a sample of the efforts of some economists to explicate the politics of trade policy in the United States. The survey of trade theory (notably Deardorff and Stern's essay) will be valuable to political scientists seeking a general knowledge of this area of economics, particularly since several of

the chapters deal also with the latest area of innovation in trade theory—the application of the theory of industrial organization and market structures to international trade. This latter development (known as the “strategic-industry” approach to trade theory and policy) is of special importance to political scientists because it makes relevant the behavior of firms and industries in imperfectly competitive markets, where each actor must predict the actions of others. Traditional trade theory assumed perfect competition, narrowing the scope for domestic politics to the very broad income-distribution effects predicted by the Stolper-Samuelson theorem (as in the Magee and Young essay).

The economists’ explanations of political behavior are less satisfactory since, with one exception (the chapter by Magee and Young), they succumb to the temptation to treat political factors as exogenous constraints rather than objects of explanation. Magee and Young note that the pure theory of trade would predict that Democrats (representing labor-intensive, import-competing interests) should be more protectionist than Republicans and suggest that the reason this is not always so is that Democrats reduce protectionist pressure with policies that increase inflation and reduce unemployment. The authors of other chapters simply take political objectives as given and ask whether trade barriers are the most efficient method of attaining these goals. Thus, the book not only provides a good background in trade theory but is also useful in pointing out to political scientists areas of trade-policy analysis where they may have some comparative advantage over the economists (e.g., in explaining why governments may choose to build up defense industries with trade barriers rather than subsidies).

In explaining U.S. trade policy, the essays span at least four levels of analysis. Corden, Jackson, and Cooper all focus on the role of, and need for, international institutional constraints on sovereignty. Jackson laments the erosion of the General Agreement on Tariffs and Trade (GATT), which he blames partly on confusion (people do not understand that the terms *most-favored nation*, *nondiscrimination*, and GATT are not coterminous) and recommends the strengthening of both the procedures and structures of GATT. Corden’s analysis is more abstract and discusses the need

for a global social compact that will meet both efficiency and distributional goals, protect people from the folly of their governments, and promote general harmony.

Several of the essays emphasize the state as a rational actor in the international economy. Srinivasan discusses the integration of defense objectives into trade policy, mostly by attempting to specify the domestic welfare trade-offs in linking the two. He also suggests that the use of trade barriers for military goals is usually unsuccessful, and that trade promotes peaceful political relations. Other essays adopt the rational-actor approach in recommending bargaining tactics (e.g., Dixit’s emphasis on the need for the United States to be committed to a tit-for-tat policy of retaliation—though he believes the United States is not institutionally equipped to pursue such a tactic).

Krugman’s essay on strategic-industry policy is also in the rational-actor mode, since it assumes a goal of national income maximization and leads to an emphasis on a state’s making credible international commitments to increase its own oligopolists’ share of global profits. However, the strategic-industry approach is so sensitive to critical assumptions about, for example, numbers of domestic firms, foreign firms’ reaction functions, and the internationalization of capital markets, that Krugman concludes there is unlikely to be any gain from attempting to implement a strategic-industry trade policy. Nonetheless, the industrial-organization approach to trade does suggest the importance of domestic interests in explaining trade policy. Dornbusch and Frankel underscore the importance of this aspect of trade policy in an essay concluding that exchange-rate movements have had much less effect on trade policy than have structural changes in industrial organization (e.g., productivity, sectoral interdependencies). None of the essays make use of the large literature on rent-seeking models of interest-group behavior to explain the cross-sectional structure of protection.

All of the papers are worthwhile reading. If there is any general fault, it is that the book exemplifies a typical conundrum for economists: the difficulty of maintaining the abstract rigor of trade theory and at the same time explaining the political realities of trade policy. Political scientists ought to be able to help here, and this book will give them numerous insights

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into areas where such help may be usefully rendered.

JOHN A. C. CONYBEARE

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International Trade and the Tokyo Round Negotiation. Gilbert R. Winham (Princeton: Princeton University Press, 1986. xi, 449p. \$45.00, cloth; \$13.50, paper).

This is an important book on an important subject. The past 10 years have seen a flood of theories that purport to explain the behavior of governments in issues of foreign economic policy, theories based on the simple calculus of organized group interests, on the role of the *hegemon*, and on the conditioning power of economic culture. It is refreshing to look closely at the complex processes of a major economic negotiation, constantly looking for evidence of the utility of the various theories.

Predictably, nothing seems to work in quite the way the theories project. Those who prefer to think that national positions in international economic negotiations are the simple sum of organized group interests may take comfort in the fact that pressure groups indeed played a substantial role in the Tokyo Round. But both the context and the weight of that role varied from one country to the next, depending in considerable part on the nature of the processes available to pressure groups to express themselves. In some cases, the pressure-group representatives came to identify themselves with the larger purposes of the negotiation; in others, they accepted their lack of influence with resignation; in still others, they reacted with intransigence.

Consistent with the approach of most negotiation theorists, Winham looks on international negotiations such as the Tokyo Round as representing for each participating country an exercise in two simultaneous negotiations, one an internal negotiation within the country's domestic establishment, the other an external negotiation with other countries. A highlight of his book—a unique effort, as far as I am aware, in the literature of international economic negotiation—is his contrast of the internal negotiating structures of the United States, the European Community, Japan, and Canada. The U.S. structure drew primarily on

representatives from the special-interest groups, the EC structure primarily on officials representing national viewpoints, the Japanese structure primarily on representatives of the various government bureaucracies, the Canadian structure primarily on provincial and bureaucratic representatives. Thinking of the nation-state simply as a rational unitary actor or as a faithful representative of its organized interests proves not very helpful in predicting its behavior.

Winham produces a provocative generalization to explain the U.S. Congress's overwhelming support of the Tokyo Round, a generalization worth the price of the book. The 1974 Trade Act, which provided the statutory basis for U.S. participation in the Tokyo Round, contained two innovations of profound importance. First, the act modified the basic constitutional relationship of the Congress to the executive branch in trade matters by assuring the executive in advance that the Congress would consider the negotiation's results on a "fast track," that is, under a special set of rules that guaranteed a simple up-or-down decision within 60 days. Second, the act required the executive to involve the representatives of the organized pressure groups deeply in the negotiation, thereby extracting the Congress itself from its uncomfortable position as middleman between the special interests and the executive.

I shall resist the usual temptation of the reviewer to introduce his own hypotheses in explanation of the success of the Tokyo Round; it is sufficient to note that Winham's view may not prove to be the last word. But his explanation is seminal for anyone concerned with understanding foreign economic policy-making in the United States.

In the end, the book leaves unanswered what may be the most important question of all. If in 1979 governments were able to achieve the remarkable agreement represented by the Tokyo Round, why did the agreement prove so empty in its subsequent impact? Why, indeed, did international trade relations run swiftly downhill in the years immediately following? In his first and final chapters, Winham deals somberly with the disappointing years following the Tokyo Round and the disconcerting prospects for the future. But his ruminations on that subject leave the critical question unanswered.

Still, Winham's careful research has carried

us a step nearer to understanding the processes involved in international economic policy-making. His work is especially important in putting some naive theories of economic policy-making in a more realistic perspective.

RAYMOND VERNON

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The Dynamics of Deterrence. Frank Zagare
(Chicago: University of Chicago Press,
1987. xiii, 194p. \$22.00).

For some time now the theory of nuclear deterrence has stood on shaky foundations. Political philosophers, and strategic theorists, as well as U.S. and French clerics have recognized the paradoxical nature of the deterrent threat in the nuclear age: the seeming irrationality of response once deterrence fails, and the dilemma of disassociating the threat from the use of nuclear weapons. Given such contradictions, Frank Zagare observes, "If deterrence were a building, it would probably be condemned" (p. 1). His goal in this book, therefore, is to provide a sturdier framework on which to place the strategic doctrines of the superpowers. Unfortunately, the author erects only a flimsy frame of revealed preferences.

Zagare argues that deterrence works when it has the characteristics of a prisoner's dilemma (PD) game, where the actors prefer war to unilateral disadvantage (the action that is being deterred). Such a formulation is immediately counterintuitive because the Nash equilibrium of the classic PD game is mutual defection or, in this case, war. If deterrence really resembles such a game, then one should expect deterrence to fail and, in a nuclear world, mutual destruction to result. Zagare argues that the inadequacy of this formulation lies not with seeing deterrence as a PD game but rather results from the limitations of classic game theory where actors choose strategies *simultaneously*. By simply stipulating that actors can move *sequentially* and in response to an opponent's strategy, mutual cooperation actually emerges as a "non-myopic equilibrium" in a PD game. This can be seen by following the reasoning of players who start from mutual cooperation in prisoner's dilemma: if one defects to get his or her best outcome, *then* the opponent will also defect

leading to mutual combat, which is a worse outcome than the initial status quo. Zagare's "theory of moves" presumes that both actors have the foresight to recognize that unilateral defection will lead to a worse outcome than one's starting position.

The remaining problem for a theory of deterrence in the nuclear age lies in whether or not the leaders of the United States and the Soviet Union really prefer mutual destruction to some form of capitulation. The author argues that the revealed preferences of the superpowers support such an interpretation; historical examples such as Masada and the Alamo are evidence that actors often reveal a preference for some value above life itself; moreover, such preferences are rational. As Zagare states, "Statesmen seem to prefer, or *at least are perceived to prefer*, [in accord with the slogan] 'better dead than red' or alternatively, 'better a grave than a capitalist wage slave'" (p. 174; my emphasis).

Herein lie two fundamental problems with *The Dynamics of Deterrence*. First, for rationality to be useful as a concept at all, we must be able to stipulate its limits and to recognize that some actions are irrational. Usually we do so by relating the efficacy of an action to the goal that one is trying to attain. In the cases of the Texans at the Alamo and the Jews at Masada, martyrdom is rational when it keeps a larger dream alive, or when losing a battle can lead to winning a war. Likewise, fighting on is rational if combatants believe that they will be killed when captured. Such martyrdom should be seen as irrational, however, as a counterthreat carried out when deterrence has failed. I am willing to grant rationality to the slaughtered at Masada and the Alamo on the principle of charity but not on the basis of them carrying out a suicidal threat when deterrence failed. There were not battles undertaken as a counterresponse when a preferred status quo was upset but battles in a larger war to upset a status quo predicated on the domination of some of the participants. These examples are entirely irrelevant to a discussion of nuclear deterrence and fail to resolve the paradox of counterresponse as irrational in a nuclear age.

Second, while nonnuclear deterrence may resemble a prisoner's dilemma game, such preferences can never be taken for granted in nuclear deterrence. The scale of mutual com-

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bat is so destructive, that there will always be doubt as to whether one's opponent really prefers that to anything else as an outcome. The question of course will remain, "What purpose could justify the risk of mutual destruction?" Deterrence is always about deterring something. It does not exist independently of a specific action to deter. When coupled with a specific action, such as an invasion of Western Europe, a theory of deterrence can have meaning. In connection with such general threats as Communist domination or a red menace, deterrence becomes so elastic as to be worse than useless. To see the world in such stark false oppositions as "better dead than red" may very well create the conflict one is trying to avoid.

Zagare may be right that deterrence as a theory is successful when it resembles a

prisoner's dilemma game. It may be that we have not had a major war between the superpowers since 1945 because the leaders of the Soviet Union and the United States rationally prefer massive death for both of its peoples to some abstract principle or specific encroachment by its antagonist. Alternatively, it may be that deterrence works best when opponents view the other as slightly irrational, that is, as willing to bring ruin on an unprecedented scale for everyone in order to punish a transgression that has already occurred. It is the peculiar dilemma of citizens living in an age of mutually threatened genocide not to know whether to prefer our leaders rational or not.

STEPHEN JOHN STEDMAN

Washington University

POLITICAL ECONOMY

The Evolution and Management of State-Owned Enterprises. By Yair Aharoni (Cambridge, MA: Ballinger, 1986. xvi, 411p. \$29.25).

Despite the recent episodes of privatization in some countries, state-owned enterprises continue to play important roles in the world's economies. Their performance therefore is of practical concern to the citizens in many countries. Theoretically, state ownership remains a meaningful alternative to private—especially private corporate—ownership. As such it is of central interest to political economists, particularly to students of democratic capitalism. Aharoni seeks to inform citizens and policymakers about the roles state enterprises play and to help them design better institutions through which to manage state enterprises. He also seeks to develop a theory of state enterprise in democracy (p. 39).

The book's value lies in the quantity and range of information it contains. Aharoni catalogs a large amount of material having to do with numerous aspects of state enterprise within and across countries. Some of the information he presents is dated. For instance, his

account of the Austrian experience contains little information about the financial difficulties that that country's state steel combine has encountered or about the political reforms that have been adopted recently in an attempt to ameliorate those difficulties (pp. 84–87). Also, most political scientists will want more information about citizens' attitudes toward state enterprises (e.g., survey results) as well as about the views and activities of political parties and interest groups. As a summary of so many aspects of, and national experiences with, state enterprise, however, Aharoni's catalog is relatively current and comprehensive.

Aharoni's discussion rests on some solid analytic footings. For example, his evaluation of state-enterprise performance is rooted in an appreciation of the similarities and differences between public and private firms and also of the character of the industries in which public firms usually operate. As a result, he avoids the mistakes of applying the same performance criteria to both kinds of firms and of assuming that private managers always perform efficiently. In addition, throughout much of his book Aharoni is sensitive to the fact that

government is not a monolithic agent with well-formed (social) preferences. He recognizes that government is a hierarchy of competing interest groups and coalitions (p. 376). He therefore avoids the mistake of assuming that efficiency is the sole criterion by which state-enterprise performance should be evaluated.

Aharoni makes some lasting contributions to the study of state enterprise. He dispels many of the myths about, and refutes, some common, monocausal explanations for state enterprise. He identifies gaps in our knowledge about state enterprise. He illuminates logical tensions between such things as public accountability and the pursuit of long-run efficiency. And he produces collections of (cross-national) generalizations about the formulation of state-enterprise objectives and the determinants of state-enterprise performance (pp. 158-60, 215-16).

In some respects, Aharoni's analysis lacks rigor, and the value of some of his contribution is somewhat limited. For example, Aharoni argues that state enterprise is subject to both more and less public scrutiny than private enterprise (cf. pp. 56, 388 with 41, 215, 373). The conditions under which state-enterprise operations are more "visible" than the operations of private enterprise and hence subject to more government intervention need to be more carefully delineated and analyzed. Also, Aharoni's review of existing empirical studies of state enterprise is not synthetic. He makes little effort to determine the extent to which the conflicting results of extant studies of state-enterprise performance are outgrowths of the use of alternative data sets or statistical techniques, let alone to carry out improved empirical analyses or new, rigorous tests of the generalizations he himself proposes.

Most serious for political scientists is the weak analysis of democratic controls over state enterprise. Aharoni emphasizes citizens' rights, willingness, and abilities to hold state enterprises accountable for their performance (e.g., pp. 56, 194, 251). At the same time, he takes as axiomatic politicians' abilities to use state enterprise to serve their selfish, short-run electoral interests, that is, to exploit citizens' ignorance about the workings and welfare consequences of state enterprise (pp. 37, 40, 67, 237, 388, 403). This is contradictory. We need a more rigorous, in-depth analysis of citizens' roles as collective owners of state enterprises

and of elections' roles as mechanisms for executing collective ownership rights. In addition, Aharoni assumes that interest-group politics is a constant source of inefficiency (pp. 149, 406). He does not systematically analyze the relative virtues of promoting state-enterprise efficiency through democratic-corporatist, as opposed to pluralist, forms of interest intermediation or, more generally, consider the possibility that interest-group negotiations reveal coalitions' (citizens') tolerance of inefficiency for the sake of some other goal like distributional equity; this in spite of the frequent references he makes to the experiences of countries with democratic-corporatist political institutions and, again, his expressed commitment to public accountability (p. 251). Rather, in the end, Aharoni favors turning over state enterprise to a set of "good" state managers. He advocates insulating state-enterprise management from electoral and parliamentary politics (chap. 11). Aharoni, like Shonfield and others before him, proposes, as a "model" for state enterprise, technocracy, not democracy as he seemed to promise in the first chapter. Of course, this is a contribution. The point is that the theory of state enterprise in democracy needs to be more fully developed and evaluated before we conclude that a technocratic approach to state enterprise is preferable to a democratic approach.

In sum, for those political scientists who are interested in the phenomenon of state enterprise, Aharoni's book is a useful catalog of information. Indeed, because of the solid analytic footings on which much of the discussion rests, most readers will find the book to be one of the best catalogs of information we now have on this subject. Those political scientists who are interested in building theories of state enterprise, especially theories of state enterprise in democracy, will find it to be useful as well but more for the theoretical challenges it poses than for the analytic insights and findings that it presents.

JOHN R. FREEMAN

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Superfairness. By William Baumol (Cambridge: MIT Press, 1986. xi, 266p. \$20.00).

"Probably the most persistent reason for noneconomists' resistance to our most cherished recommendations on micropolicy," observes Baumol, "is our determined disregard of their implications for distributional equity" (p. 1). Concepts and analytical tools have recently been developed, however, that permit a new understanding of distributional questions. Baumol's aim is to present this literature and to show its usefulness in the evaluation of concrete policy issues. Although he partially achieves his first objective, he falls far short of achieving the second.

In 1967, Foley proposed that a distribution of goods among agents with equal claims be declared equitable if no agent prefers someone else's bundle to his own. This is the notion of an *envy-free* (EF) allocation. If there is only one good, then *equal division* (ED) is the only EF allocation. Then ED is also *Pareto efficient* (PE). Otherwise, ED is not in general PE, but allocations that are both EF and PE usually exist. Indeed the Walrasian allocations obtained by taking ED as initial position enjoy both properties.

The introduction of the concept of an EF allocation constituted an important development in welfare economics for the following reasons. First, it is ordinal; economists' reluctance to use the cardinal concept of utility is the principal reason for their extensive reliance on the notion of Pareto efficiency, which essentially places no restriction on the distribution of welfare. Second, it provides a significant reduction of the set of PE allocations; in fact, in many economies with a large number of agents, only the Walrasian allocations from ED, which are generally few, are both EF and PE. Third and more importantly, it is a very natural concept of equity in situations where agents can meaningfully exchange positions, and its recommendations are in accordance with intuition. (To illustrate, an allocation at which one agent receives more of all goods than some other agent is obviously not EF.) Also, it does not rely on ad hoc notions such as, for instance, that of implicit incomes; the suggestion often made by economists to select allocations at which implicit incomes are equal across agents, that is, to allocate resources by

operating the Walrasian correspondence from ED, is unnatural since that correspondence has no more normative appeal than a number of other correspondences commonly discussed, when operated from ED.

It was quickly discovered, however, that EF and PE allocations may fail to exist in production economies (because of possible differences in agents' productive abilities). This disappointing result led to the development of an important literature devoted to the formulation and the evaluation of other fairness concepts. Unfortunately, none of them enjoy all of the appealing properties of Foley's original concept.

Baumol does not attempt to review the whole fairness literature, but his exposition of the basic concepts (chaps. 2 and 3) is clear and well illustrated with Edgeworth boxes, a tool ideal for this purpose. He describes the intricate structure of the set of EF allocations in the two-commodity, two-person case, relates the EF concept to the criterion of Pareto domination of ED (an allocation may be EF without Pareto-dominating ED; an allocation in the core from ED may not be EF). He also discusses the problem of evaluating the equity of a trade vector from a given initial position (it is natural to say that a trade vector is envy free if no agent prefers someone else's component to his own), and he notes certain important difficulties that arise when the concepts of EF allocation and EF trade are combined (for example, an EF and Pareto-improving trade from an EF allocation may lead to an allocation with envy). This sort of result has serious implications for the step-by-step implementation of distributional objectives. In this regard, his own attempt at devising a procedure that would yield a single-valued selection from the set of EF allocations via certain sequences of EF trades is instructive, in spite of it being largely unsuccessful. Finally, the author presents, although in much less detail, various generalizations of the EF concept to production economies.

On the other hand, the author's efforts to show the relevance of the fairness concepts to policy issues are unconvincing. His analysis of rationing schemes (chap. 4) yields no useful results, and his treatment of arbitration procedures (chap. 11) is very limited. (The latter would have been considerably helped by a more systematic use of the Edgeworth box

representation, a technique also perfectly suited to this topic.)

In spite of these serious flaws, however, the chapters discussed thus far are well unified by a common conceptual apparatus. They could have constituted the core of a worthwhile monograph. Unfortunately, the other chapters (a large portion of which are reprints from the author's previous works) devoted to policy issues such as conservation (chap. 5), industry pricing (chaps. 6 and 7), peak and off-peak pricing (chap. 8), and income equalization (chap. 9), are very disappointing (I will not discuss chap. 10, concerned with certain methodological issues pertaining to "economic illusion": this chapter, also very weak, stands somewhat apart). The analysis is conducted mainly in terms of the standard concepts of Pareto efficiency and Pareto domination. Chapter 7 does contain a fairly comprehensive, although superficial, review of various normative theories of cost allocation schemes, but an application of the concepts and methods of the first chapters is seriously attempted only in a section concerning utility pricing. However, apart from the fact that nothing specific is really said about utilities, the use of the EF concept to analyze a situation in which the two agents are a representative consumer and a representative firm and the two goods are a physical resource and a price is artificial. In fact, it illustrates the basic difficulty (discussed in chap. 11) with the application of the fairness concepts; their usefulness seems to be limited to situations in which all agents play fundamentally symmetric roles.

WILLIAM THOMSON

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The Reason of Rules: Constitutional Political Economy. By Geoffrey Brennan and James Buchanan (Cambridge: Cambridge University Press, 1985. xiv, 153p. \$34.50).

Constitutionalism is back. We see it return in a variety of forms. One version is the constitutional political economy developed by the "Virginia school" of public choice. The methodology of such a constitutional political economy is the subject of the present book. But there are many other types of constitutionalism now in the works, including forms of a

constitutionalist political economy very different from the Virginia one. These alternatives include applications of interpretive methods to the most fundamental and constitutive political questions as well as the more conventionally scientific study of how impartial standards can restrain collective decisions. There are hermeneutic, neo-Straussian, neo-Parsonian and causal-behavioral constitutionalisms, as well as the public-choice version under review here.

Why is constitutionalism back? To a degree, no doubt, it is simply a fad, fueled perhaps by the approach of the 200th anniversary of the great constitutions (the U.S., 1787; the Polish, 1791; the French, 1793). But it is surely more than a fad. It is a response to the current political situation, especially the crisis of the "democratic welfare state." This motive is quite clear in the book under review. But it is also a response to the crisis of political, social, and economic theory, an effort to redirect it to more fundamental questions and to questions of greater normative significance.

There is no question that the Virginia school of public choice is now and will continue to be an important player in this revival. The works of Buchanan, Brennan, Vanberg, and others define a distinct constitutionalist alternative. Anyone who takes the new constitutionalist renaissance seriously will have to study them carefully. Brennan and Buchanan's *The Reason of Rules* can best be evaluated in this context. It is important as a report of the current stage of development of the "Virginia" alternative. But it is unlikely, in my judgment, to be a more definitive statement. The book has many marks, in fact, of a transitional work. It is short and sketchy. Most of its arguments are not worked out in great detail, many are short summaries of more elaborate presentations elsewhere, without any attempt to achieve a "grand synthesis." The tone of the book is highly tentative, it is full of *maybes* and *mights*. The overall organization is rather weak; there are at least two dramatic and unexplained shifts in argument. Finally and most importantly, Brennan and Buchanan's book defends a position that, by the end, the authors themselves acknowledge to be self-contradictory.

The Reason of Rules is engaged in two important intellectual battles. It defends a morally skeptical, contractarian position ac-

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according to which politics is the pursuit of self-interest analogous to exchange in the market. It attacks the view that politics involves the search for right answers, that it is more like the work of scientists or members of a jury. But the book is simultaneously engaged in another kind of dispute. It sets out a case for the constitutionalist position, for the importance of choosing relatively rigid rules that will provide constraints for day-to-day decision making. It bases this case on the empirical hypothesis that people are more "future oriented" in their private decisions than in their public ones. Hence, without constitutional rules collective decisions will be shortsighted relative to individual decisions. Among possible consequences of this pattern Brennan and Buchanan discuss excessive taxes, growing public debt, and inflation. In the end, these two positions, the morally skeptical and the constitutionalist, come into conflict. If politics is only the rational pursuit of self-interest, then the constitutionalist argument cannot succeed politically, because it cannot succeed (as the authors note) by appealing exclusively to self-interest. One of the two positions must give way. Let me offer a suggestion: in order to maintain their "public-choice" position, Brennan and Buchanan need only assume that "politics as exchange" is ordinarily dominant over "politics as science"; they do not have to assume the latter does not exist. If we allow both kinds of politics in our theory, we can be more optimistic about the future of constitutionalism and about the future of democracy as well. But we also have a new task: to understand that aspect of politics—however weak—that is best seen as the search for a right answer. It is an aspect of politics that seems to me far less well understood than "politics as exchange." And it requires something very unlike economic theory to understand it.

KAROL EDWARD SOLTAN

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Electoral Laws and Their Political Consequences. Edited by Bernard Grofman and Arend Lijphart (New York: Agathon, 1986. xiv, 335p. \$38.00, cloth; \$16.50, paper)

In an era of renewed legal and scholarly interest in the subject of democratic representa-

tion, Bernard Grofman and Arend Lijphart have collected 19 articles—18 previously unpublished—in a useful volume on the impact of electoral laws. The volume, which includes a very good bibliography and index, is divided into four parts: the first part (chaps. 1–5) examines the impact of election format on political competition; the second (chaps. 6–10) reviews systems of proportional representation (PR) and semiproportional representation; the third (chaps. 11–16) considers various aspects of plurality systems; and the fourth (chaps. 17–19) looks at redistricting.

The approach of the book is broadly comparative in nature, with 11 of the 19 essays dealing in whole or in part with electoral practices outside the United States. The first three chapters examine the validity of Maurice Duverger's "laws" on the relationship between electoral format and party competition. William H. Riker (chap. 1) and Giovanni Sartori (chap. 2) agree that Duverger's original formulations are "ambiguous" (pp. 20, 44), but Sartori disputes Riker's reformulation of the principal law. Sartori lays out a careful analysis of the conditions that give rise to various party systems and concludes with two "tendency laws" that are, in Sartori's words, "just about all that Duverger said": "Plurality formulas facilitate . . . a two party format and, conversely, obstruct multipartyism" and "PR formulas facilitate multipartyism and are, conversely, hardly conducive to two-partyism" (p. 64). Duverger himself (chap. 3) reviews party competition in Western European countries in the four decades since he first proposed his "sociological laws" (p. 70).

A second group of the "comparative" chapters (4, 6–7, 9–11, and 18–19) deals primarily with various aspects of PR abroad; these chapters cover such matters as national and state representational formulas in Australia; semiproportional representation (limited voting) in Japan and Spain; and special provisions for ethnic representation in Belgium, Cyprus, and several other countries. Lijphart (in chap. 10) cogently examines the different forms of PR, explaining the formulas and ranking the various approaches in terms of the correspondence of party representation to party vote. In a related article, Peter Taylor et al. review the "geography of representation."

For the most part, the "comparative" chapters offer a very readable, and sometimes eye-opening, introduction to foreign election

systems. An example is Richard S. Katz's "Intraparty Preference Voting." While pointing to the direct primary in the United States as the best example of voter control over the selection of party candidates, Katz notes that "the majority of democratic electoral systems . . . allow those voting for a party to determine, or . . . to influence, the identity of the particular candidates from among those the party has nominated." In a number of countries—Finland and Italy among them—intraparty defeat created more parliamentary turnover than did interparty defeat. Peter Mair's article on Ireland, where members of the Dáil are elected from multimember constituencies by the single transferable vote (STV), notes that candidates often seek election through constituent service to "particular bailiwicks" (pp. 293–94). Students of reapportionment in this country will be amused by R. J. Johnston's article on recent redistricting of the British Parliament. British courts in 1982–83 upheld the plan adopted by the Boundary Commission; in the process, the courts refused to establish a precise "definition of excessive discrepancy" (p. 282) in the population of legislative districts and, at the same time, recognized that (under statutory guidelines) "equality of representation for people is secondary to that of representation for places" (p. 284).

Among the articles focused primarily on the United States are selections on format of partisan ballots and candidate cross filing. Three deal primarily with the consequences of the Progressive movement of the early twentieth century. Leon Weaver traces the history of PR (usually STV) in the United States. Twenty-two city councils experimented with PR, mostly in the period between the two world wars, but by the late 1950s PR had nearly disappeared. In recent years PR has attracted renewed interest. Richard L. Engstrom and Michael D. MacDonald address the impact of at-large (as opposed to ward or mixed) elections on minority representation on city councils. The thesis that blacks are less likely to achieve "proportionate" success under at-large than under ward elections is, they conclude, "impressively documented" (p. 224). However, they find the evidence of the linkage between black representation and governmental responsiveness to minority interests to be ambiguous. Interestingly, in her review of the ef-

fects of nonpartisan elections (as compared to partisan elections), Carol A. Cassel suggests that a nonpartisan format "inhibits the election of black council members but promotes the election of black mayors" (p. 237).

Among the other articles on the United States, Gordon E. Baker evaluates the impact of the reapportionment and calls for "a long-overdue 'dialogue' about the fundamentals of representation" (p. 274). Arguing that two decades of court-supervised redistricting activity has brought "only a partial victory" (p. 266) in the battle for "fair representation," Baker criticizes the Supreme Court's preoccupation with strict mathematical equality in legislative districts to the neglect of broader questions of representation: "Finding ways to check partisan gerrymandering that has proliferated in the wake of one-person, one-vote rulings is probably the most pressing challenge" (p. 274).

Baker's essay raises questions about representational values that merit more attention than they receive in this volume. Litigation against at-large elections and partisan gerrymandering, for instance, has focused attention on political scientists' conflicting interpretations of the consequences of particular election rules. Yet taken as a whole, this collection gives too little attention to the substance—and drama—of the controversy surrounding "fair representation." In this respect, one wishes that the spirit of debate that marks the opening chapters might have been extended to other parts of the book. Then too, the editors' introductory essay might have more clearly identified the underlying issues that connect the wide-ranging subject matter of the various articles. Two or three of the articles are too brief, and thus sketchy; conversely, too much space (about one-fifth of the text) is devoted to the three chapters on Duverger.

Yet, on balance, this is a worthwhile collection. In the editors' words, this volume is supposed to serve as "an overview of recent research on electoral laws . . . by scholars who have helped shape the field" (p. 1). The volume surely achieves this modest objective. In doing this, the book establishes a broader international and interdisciplinary perspective on the methods of representation.

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Who Profits: Winners, Losers, and Government Regulation. By Robert A. Leone (New York: Basic Books, 1986. xiii, 248p. \$17.95).

The last decade has witnessed a revolution in the regulatory process that rivals the New Deal in scope and impact. For the first time in history, the executive branch has achieved an unprecedented degree of control over the vast body of regulatory activity in both the independent and executive agencies. Initiated by the Carter administration, and accelerated by President Ronald Reagan, the new regulatory process is as different in its treatment of administrative power as were President Roosevelt's initiatives in the 1930s. Now, however, the role of government has changed. In the New Deal period, the president viewed government as a growth industry, to be used as a vehicle for problem solving; while today, White House efforts focus on minimizing government, in order to allow market forces to work more effectively.

Who Profits is an example of the kind of thinking, much in vogue in the late 1970s, that led to this revolution in executive power. It is primarily a study of the negative impacts of regulation on the manufacturing process, using examples that draw heavily on case materials from the Harvard Business School. The author is an economist who understands the problems facing manufacturers suddenly confronted with regulatory decisions that can drive them out of business or cut substantially into their profit margins. He analyzes the impact of regulatory decisions on such factors as differential costs, tracing what happens to affect the final drafting of the bottom line. Especially interesting is his discussion of regulation as a disincentive to new investment; how the existence of a cumbersome regulatory process leads to a bias against building new manufacturing facilities, and affects U.S. competitiveness. Leone's extensive use of the vast literature on the costs of regulation brings this body of work up to date and casts a critical eye on some of its problems.

Actually, *Who Profits* could serve as a road map for what has already transpired in the federal government, although the author does not seem aware of these developments. In that sense, the book seems out of date. The author argues eloquently for the use of economic

analysis in regulatory decision making on the grounds that it does not exist in the federal arena. In fact, economic analysis has been mandated in all major regulatory decisions since 1981 and the promulgation of Executive Order 12291. The order is rigorously enforced by the Office of Management and Budget, and the agencies have heard the message loud and clear. Benefit-cost analysis now supersedes technical and scientific analysis, as well as health, safety, and environmental considerations.

The book's theoretical approach posits a view that looks very much like public-choice theory. Briefly, Leone argues that public-policy decisions have an enormous impact on industry, that there are heavy winners and losers, and that policymakers need to be made more aware of the economic consequences of their decisions. A strong faith in the value of marketplace competition is repeatedly expressed throughout the book. The idea is that the forces of the free market almost always lead to outcomes preferable to government intervention. There is virtually no recognition of the unanticipated negative consequences of untrammelled market forces or of what happens when an unregulated marketplace is overtaken by predators out to destroy competition. Scholars needn't go far to find examples of this outcome: ample evidence exists in the fields of antitrust, airline, trucking, and railroad deregulation.

Another important subtheme emphasizes that regulatory decision makers are unaware of the unanticipated consequences of their decisions. To buttress this argument, the book offers many examples, most of which fall into one category: the impact on a firm or an industry's profit margins. Some of these examples are rich in detail and offer dramatic proof of the costs of regulation. Other examples show inadequate documentation or inaccuracies. The author, in attacking liberals for ignoring the long-term effects of government intervention, writes, "When policy makers for the state of California look at the prospect for a unitary tax. . . ." California has not looked at the "prospect" for a unitary tax for a long time; it has had a unitary tax since 1929, repealing it in 1986. When dealing with auto-safety regulation, the book concludes that there are more "cost effective ways of achieving levels of safety benefits than passive restraints." The documentation for this state-

ment is a General Motors report.

The book's strength is its appreciation for some of the economic problems incurred by industry as a result of its relationship to government. Its weakness lies in its treatment of the political and regulatory process, and its lack of understanding of those processes. The argument that public managers "tend to be held accountable almost exclusively to process and rarely to outcome" misses the importance of integrity to the regulatory process: if the process is fair, there is a greater chance that the outcomes will favor the public's interests. This is the basis for the Administrative Procedure Act and for appeals by citizens and corporations who feel that they have been disadvantaged by regulatory decisions.

The author's major concerns are competitive realities, and he deals with these very well. He skillfully disentangles the roles of equity and efficiency, emphasizes the importance of business managers' understanding the regulatory process, and gives solid advice to industry on how to manage the regulatory process in a more sophisticated way. "Take preemptive action," "Take individual action," and "Use information as a weapon," are several suggestions that are offered corporate public-affairs managers. Public managers are advised to manage their data better and to intrude on industry as little as possible.

But regulatory problems have a broader dimension, as practitioners and scholars are beginning to recognize. What happens in the absence of regulation, and what are the long-term costs on this "bottom line" approach? Who suffers? and Who pays in the long run? are questions notably missing from the Who-profits? approach. In fact, new technologies need more government regulation, not less, as their leaders are beginning to recognize: recent efforts by the biotechnology industry to seek additional government regulation bears witness to this change in thinking. That process needs to be fair, cost conscious, and adequate to the task, in order to help industry fulfill its potential and to protect the consumer. And, as government and enlightened business leaders agree, regulators need to be at least equal in strength to the industry they are regulating, not subordinate to the current vogue of market forces.

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Game Theory and Political Theory. By Peter C. Ordeshook (Cambridge: Cambridge University Press, 1986. xv, 511p. \$49.50, cloth; \$17.95, paper).

Many of us have been awaiting publication of Peter Ordeshook's *Game Theory and Political Theory* for quite a few years. Now it has arrived, and the wait has been worthwhile. As the subdiscipline generally known as positive political theory enters its third decade, Ordeshook has crafted what for the time being must be regarded as the definitive survey of the field. The treatment is comprehensive (within the generous limits of five hundred pages) with respect to the theoretical substructure of the field and with respect to applications of that theory to electoral and committee voting processes (though bureaucratic and nondemocratic processes are not covered); up-to-date; and—given the author's many contributions to the literature, both theoretical and experimental, that he is reviewing—distinctly authoritative.

Ordeshook's premise—and what his book demonstrates—is that "formal models of politics" now form an integrated whole, much like "formal models of markets." It is game theory that unifies the heretofore disparate models into what Ordeshook boldly calls "political theory" (dropping the modifier *positive*). From this premise come both the title and the organization of Ordeshook's book.

Ordeshook's first chapter presents an essentially standard, but exceptionally clear and comprehensive, discussion of individual preference and choice. In the first part of chapter 2, he presents Arrow's impossibility theorem and a variety of other voting paradoxes, but to this point he assumes that electoral institutions mechanically aggregate individual preferences into social choices. In the second part of the chapter, Ordeshook establishes the possibility of strategic manipulation of institutions, first by considering the power of agendas in influencing voting outcomes and second by considering incentives for misrepresentation of preferences under voting procedures and social-choice functions of all types. This sets the stage for the systematic analysis—provided by game theory—of strategic interaction of agents within institutions. The remaining eight chapters then survey standard topics in game theory, but with interesting dif-

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ferences in organization and emphasis compared with the classic texts in the field.

Indeed, Ordeshook's book invites comparison with R. Duncan Luce and Howard Raiffa's *Games and Decisions* (New York: Wiley, 1957), which for years has been considered the standard treatment of game theory for social scientists. It would be fair to say the two books are pitched at about the same level of abstraction and difficulty. And Ordeshook's writing approaches the standard of clarity set by Luce and Raiffa (quite a compliment). To some degree, Ordeshook shares the critical posture of Luce and Raiffa, at least to the extent of regularly pointing out that much conceptual development and research remains to be done in the construction of comprehensive political theory. At the same time, there are major differences between the two books, which derive from two obvious and related facts. The first is the Luce and Raiffa book is now 30 years old; while the main lines of game theory were clearly established when they wrote, there have been many very significant developments in game theory itself (especially new solution concepts) and in its application over the past decades. Second, Luce and Raiffa were not political scientists, and their survey did not focus particularly on applications of game theory to politics. Indeed, apart from a discussion of the Shapley-Shubik power index to weighted voting systems and a brief and unsystematic discussion of strategic aspects of majority rule, their survey includes no distinctively political topics. Of course, at the time they wrote there was little to survey in this respect, while many of the more recent applications of game theory are to political processes.

Ordeshook's focus on political applications of game theory leads him to depart from the standard mode of presentation established by Luce and Raiffa. Conventionally, two-person games are considered before n -person games, and zero-sum games before variable-sum games, but Ordeshook moves directly (in chap. 3) to consider n -person variable-sum noncooperative games (focusing on voting games in particular). And conventionally, cooperative games are considered under the assumption of transferable utility (generally relevant to economics but not politics), while Ordeshook treats transferable utility only as a very special case. Perhaps most important,

Ordeshook devotes a great deal of attention (beginning in chap. 1) to spatially defined preferences, on which most models of electoral competition and committee voting are now based. This orientation is completely missing from Luce and Raiffa and most other game-theory texts.

In addition, Ordeshook's new book invites comparison with his previous text, coauthored with William Riker, *An Introduction to Positive Political Theory* (Englewood Cliffs, NJ: Prentice-Hall, 1973, now out of print). The new book is no mere updating, as it presents a far more coherent synthesis of the seemingly distinct topics that defined the chapters of the earlier book. And of course the new book reflects the progress of research over the past decade (most notably, perhaps, with respect to the theory of spatial voting games and agenda processes and with respect to experimental testing of solution concepts).

Game Theory and Political Theory will serve two audiences especially well. First, specialists in the area of positive political theory will surely want to read it to see how a leading scholar in the field organizes and presents familiar but difficult material; they will also find it handy reference work with respect to recent and perhaps unfamiliar developments. Second, present nonspecialists who are seriously interested in gaining an understanding of positive political theory will find this an excellent place to start. In particular, the book will serve as a fine text for graduate-level courses in the field. (I doubt it can be successfully used at the undergraduate level). The fact that a literature guide is attached to each chapter together with the fact that Cambridge is publishing the book in a reasonably priced paperback edition, will enhance the book's utility as a course text.

NICHOLAS R. MILLER

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Coalitional Behaviour in Theory and Practice: An Inductive Model for Western Europe.
Edited by Geoffrey Pridham (Cambridge: Cambridge University Press, 1986. xix, 308p. \$44.50).

The recent revival of interest in government coalitions in parliamentary democracies is in-

disputable. Geoffrey Pridham's recent book underscores the rediscovery of this traditional topic among Western Europeanists. However, this is not another collection of theoretically unconnected, country-specific articles. Nor is it a volume of abstract game-theoretic exercises. On the contrary, the purpose of Pridham's book is to present a sustained critique of, and alternative to, the game-theoretic approach to parliamentary government. Pridham refers to this enterprise as a fresh look at coalition theory in the light of coalition experience (p. xvii) and calls his alternative an inductive theoretical framework within a multi-dimensional perspective.

This edited volume is the fruit of a workshop held during the sessions of the European Consortium for Political Research at Salzburg in April 1984. The volume is implicitly divided into three parts. The first two chapters (by Pridham and Michael Laver) outline the analytical framework and critique the existing literature. The bulk of the volume (the next nine chapters) consists of country studies more or less committed to the analytical tools Pridham provides in the first chapter. Finally, chapters 12 and 13 explore the applicability of the same tools to coalitions at subnational levels and introduce the reader to a couple of important ongoing research projects. Neither of the latter, however, bears much theoretical resemblance to Pridham's approach.

In the Preface, the editor informs us that the idea for the volume originated in his own work on Italian coalition politics. In fact, Pridham's chapter on Italian coalitions is a good place to begin familiarizing oneself with his approach to coalition formation and maintenance. Although the chapter is neither parsimonious nor particularly elegant, it does demonstrate the relevance of his seven dimensions to the study of coalition politics. More specifically, Pridham shows how the governmental game in Italy is constrained by historical events (such as established coalition formulae), institutional characteristics (e.g., the role of the president), differences in party motivation (patronage vs. policy), local and regional coalition patterns (often precursors of national coalitions), internal party factionalization (most notably among the Christian Democrats), socio-political factors (e.g., declining mass deference and party identification), domestic events (stagflation), and international commitments

(e.g., to NATO and the United States).

Pridham's attempt in the introductory chapter to deal with these constraints analytically is much less persuasive. The discussion is frequently rambling and repetitive. Pridham presents no sustained critique of the deductive theories he opposes, and his own model is not presented with great clarity. His invocation of the concept of dimensionality is confusing. Deductive coalition theories are labelled "one-dimensional," but this somewhat mystifying characterization is never explained to the reader. Nor is it clear what is dimensional about Pridham's own dimensions, which seem more aptly described as constraint categories. Moreover, one must question the editor's contention that his inductive approach "is imperative for handling the complexity of this subject in a viable and comprehensive way" (p. 14). Complex subjects may require complex models, but not necessarily inductive ones. These flaws notwithstanding, Pridham deserves credit for his ambitious attempt to bring a broad range of literature to bear on the subject of coalition formation and maintenance.

The reader is treated to another aperitif in the form of Michael Laver's analytical chapter on deductive models and cabinet coalitions in Europe. This concise, witty, and admirably nontechnical chapter is clearly the best in the book. It should be especially instructive for the intelligent novice to this field of study. Like Pridham, Laver criticizes formal coalition theories for a number of unrealistic assumptions. Foremost among these problems is the static nature of most theories. However, Laver's conclusions are more sanguine, in the sense that he sees more potential for progressive modifications of deductive coalition theory.

Laver is also partly responsible (with Michael D. Higgins) for a very readable and informative chapter on government coalitions in Ireland. The country chapters are on the whole surprisingly even in quality and structure. Certain heterogeneities and imbalances do, however, exist. Two chapters focus on the coalitional behavior of particular parties rather than national party systems. The chapter on France (by Eric C. Browne and Dennis W. Gleiber), on the other hand, scrutinizes a single government (Ramadier) and departs radically from Pridham's multidimensional approach.

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Geographical coverage is, as usual, not strictly determined by theoretical interest. Thus it is difficult to see why Germany, with its paucity of parties and coalitions, deserves two chapters, whereas the Scandinavian countries collectively get the short shrift of one. The latter chapter (by John Fitzmaurice) is in turn internally uneven. Denmark is covered in a much more satisfactory manner than Sweden or (particularly) Norway. On the latter country, Fitzmaurice commits a multitude of embarrassing factual errors, such as misspelling the names of all but 2 parties (out of 11) in the Appendix (pp. 276-77). A positive feature of the country coverage is the inclusion of Spain, despite the limited history of party coalitions in that new democracy.

In sum, Geoffrey Pridham and his collaborators have made a useful, but hardly indispensable, contribution to our understanding of coalition governments. The book may be read for its juxtaposition of Pridham's and Laver's critiques of deductive coalition theory. Some readers may also value the insight they gain concerning ongoing research projects. Yet it is fair to assume that most will find its main contribution in the rich and relatively systematic country-specific treatment of historical, cultural, and institutional constraints on coalition politics.

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Erratum

Francis A. Beer, Alice F. Healy, Grant P. Sinclair, & Lyle Bourne, Jr. "War Cues and Foreign Policy Acts" (September 1987, 701-55). On page 706, the figures should be titled as follows:

Figure 1. Mean Cooperation-Conflict Scale Score ("Conflict Score") as a Function of Dominance Group and Priming Condition

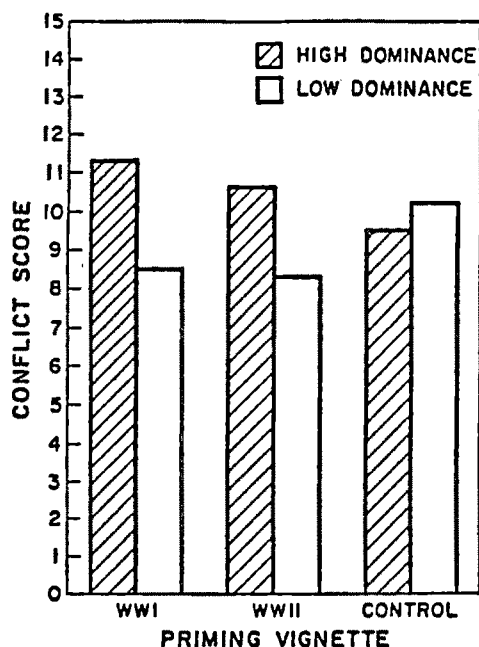
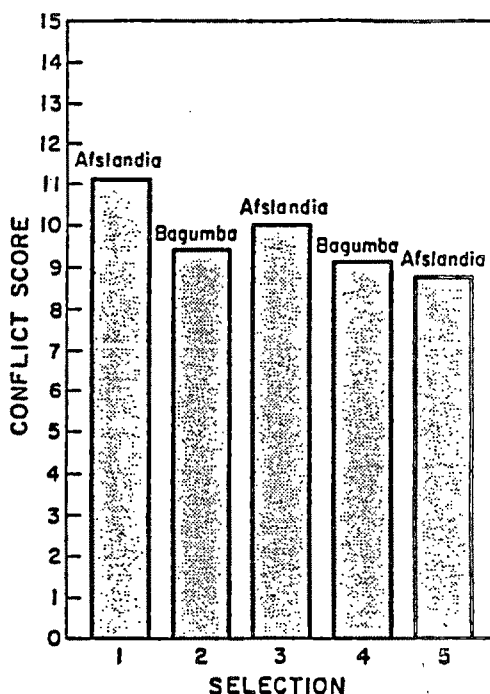


Figure 2. Mean Cooperation-Conflict Scale Score ("Conflict Score") as a Function of Successive Selection



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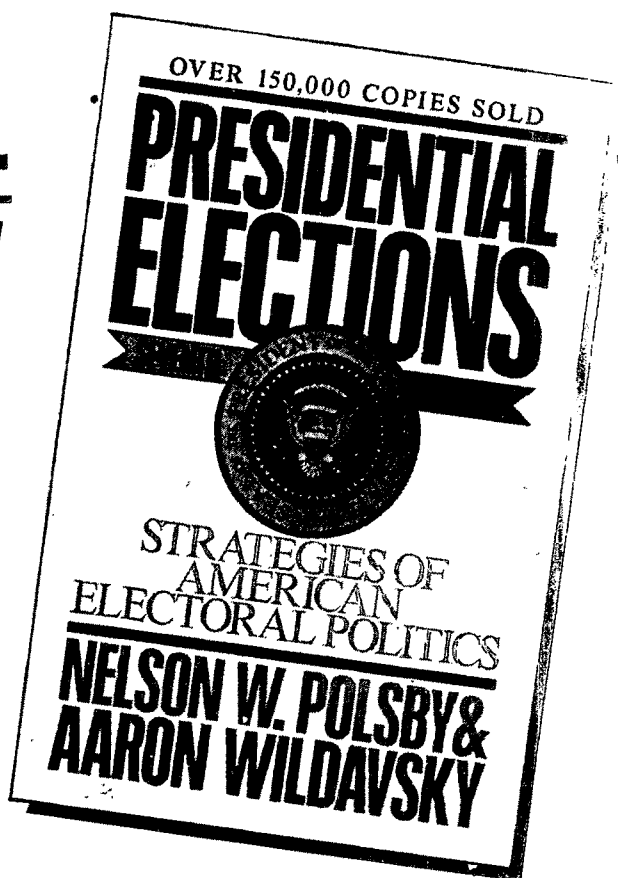
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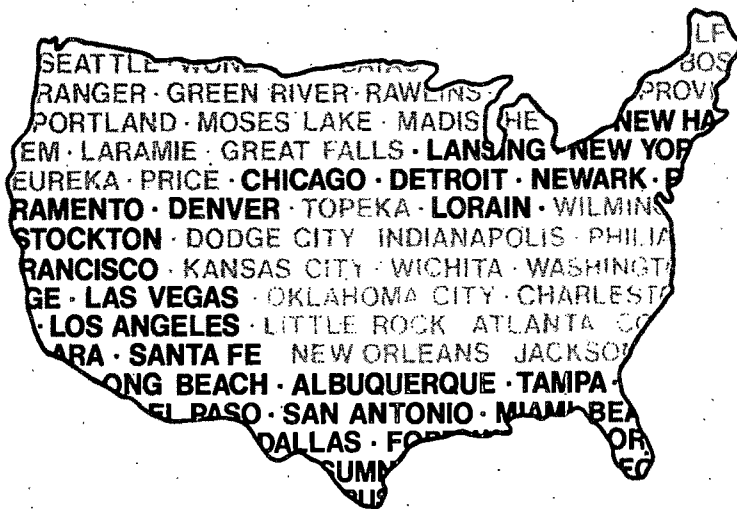


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